Bias and Systematic Change in the Parameter Estimates of Macro-Level Diffusion Models

Christophe Van den Bulte • Gary L. Lilien
The Wharton School, 1400 Steinberg Hall-Dietrich Hall, University of Pennsylvania, Philadelphia, Pennsylvania 19104-6371, christophe@marketing.wharton.upenn.edu
Smeal College of Business Administration, The Pennsylvania State University, University Park, Pennsylvania 16802

Abstract

Studies estimating the Bass model and other macro-level diffusion models with an unknown ceiling feature three curious empirical regularities: (i) the estimated ceiling is often close to the cumulative number of adopters in the last observation period, (ii) the estimated coefficient of social contagion or imitation tends to decrease as one adds later observations to the data set, and (iii) the estimated coefficient of social contagion or imitation tends to decrease systematically as the estimated ceiling increases.

We analyze these patterns in detail, focusing on the Bass model and the nonlinear least squares (NLS) estimation method. Using both empirical and simulated diffusion data, we show that NLS estimates of the Bass model coefficients are biased and that they change systematically as one extends the number of observations used in the estimation. We also identify the lack of richness in the data compared to the complexity of the model (known as ill-conditioning) as the cause of these estimation problems.

In an empirical analysis of twelve innovations, we assess how the model parameter estimates change as one adds later observations to the data set. Our analysis shows that, on average, a 10% increase in the observed cumulative market penetration is associated with, roughly, a 5% increase in estimated market size m, a 10% decrease in the estimated coefficient of imitation q, and a 15% increase the estimated coefficient of innovation p.

A simulation study shows that the NLS parameter estimates of the Bass model change systematically as one adds later observations to the data set, even in the absence of model misspecification. We find about the same effect sizes as in the empirical analysis. The simulation also shows that the estimates are biased and that the amount of bias is a function of (i) the amount of noise in the data, (ii) the number of data points, and (iii) the difference between the cumulative penetration in the last observation period and the true penetration ceiling (i.e., the extent of right censoring). All three conditions affect the level of ill-conditioning in the estimation, which, in turn, affects bias in NLS regression. In situations consistent with marketing applications, m can be underestimated by 20%, p underestimated by the same amount, and q overestimated by 30%.

The existence of a downward bias in the estimate of m and an upward bias in the estimate of q, and the fact that these biases become smaller as the number of data points increases and the censoring decreases, can explain why systematic changes in the parameter estimates are observed in many applications. A reduced bias, though, is not the only possible explanation for the systematic change in parameter estimates observed in empirical studies. Not accounting for the growth in the population, for the effect of economic and marketing variables, or for population heterogeneity is likely to result in increasing $\hat{m}$ and decreasing $\hat{q}$ as well. In an analysis of six innovations, however, we find that attempts to address possible model misspecification problems by making the model more flexible and adding free parameters result in larger rather than smaller systematic changes in the estimates.

The bias and systematic change problems we identify are sufficiently large to make long-term predictive, prescriptive and descriptive applications of Bass-type models problematic. Hence, our results should be of interest to diffusion researchers as well as to users of diffusion models, including market forecasters and strategic market planners.
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1. Introduction

This paper shows that the nonlinear least squares (NLS) estimates of Bass model parameters are biased and change systematically as one adds later observations to the data set. While similar problems have long been recognized by statisticians (e.g., Box 1971, Seber and Wild 1989), the marketing literature provides little evidence of awareness or appreciation of these biases and systematic changes.

For nearly three decades, marketing scientists have used the Bass (1969) model and many variants to understand the diffusion of new products and to predict their eventual penetration levels. Bass assumed that innovation acceptance is driven in part by social contagion. He therefore specified the limiting probability that an actor who has not adopted yet at time t does so at time $t + \Delta t$ ($\Delta t \to 0$), often referred to as the hazard rate of adoption $h(t)$, as a linear function of the proportion of eventual adopters that has already adopted:

$$h(t) = p + q\,F(t), \quad \text{where} \qquad (1)$$

$h(t)$ = hazard rate of adoption at time t,

$F(t)$ = cumulative distribution function of adoptions at time t,

p = coefficient of innovation, capturing the intrinsic tendency to adopt regardless of social influence as well as the effect of time invariant external influences, and

q = coefficient of imitation or social contagion, capturing the extent to which the hazard rate increases with the proportion of eventual adopters that has already adopted.

Using the definitions of the density function, $f(t) = dF(t)/dt$, and of the hazard rate, $h(t) = f(t)/[1 - F(t)]$, Bass reexpressed Equation (1) as:

$$dF(t)/dt = [p + q\,F(t)]\,[1 - F(t)]. \qquad (2)$$

Multiplying both sides of this equation by the number of eventual adopters, Bass obtained a macro-level expression:

$$dX(t)/dt = [p + q\,(X(t)/m)]\,[m - X(t)], \quad \text{where} \qquad (3)$$

$X(t)$ = the number of people having adopted by time t, and

m = the number of eventual adopters ($F(t) = X(t)/m$).

Although the hazard Equation (1) and its variant (2) had been developed before Bass published his work, the model saw few applications because it required knowing the number of eventual adopters to obtain $F(t)$ (Coleman 1964, Mansfield 1961). Bass (1969) expressed that adoption ceiling as a parameter and showed how one could use available sales data to estimate not only the behavioral parameters p and q, but also the adoption ceiling m, making the Bass model a promising tool for forecasting and understanding the development of a market. Empirical diffusion studies in marketing have typically followed the Bass approach, using a time series of aggregate-level sales or penetration data to estimate a three parameter model like Equation (3). Originally, researchers used ordinary least squares (OLS) to estimate model parameters, but more recently they have favored nonlinear least squares (NLS). The three-parameter, macro-level approach using NLS estimation has gained widespread acceptance (Mahajan et al. 1993, Parker 1994).

Three curious patterns exist across the applications of the Bass model and its variants. First, the estimated ceiling $\hat{m}$ often approximates the cumulative number of adopters observed in the last period used in the estimation, $X(t_+)$, where t_+ denotes the last observation period. The pattern occurs with the original Bass model, as well as with more flexible versions allowing for dynamic population size, non-uniform influence, and price effects (Table 1). The finding that $X(t_+)/\hat{m} \approx 1$ in many applications raises the possibility of a systematic downward bias in the estimate of the adoption ceiling or market size ($\hat{m}/m < 1$), since $X(t_+) \le m$ by definition. The pattern in Table 1 is not conclusive, though, since it is conceivable that in all these studies the market was already close to saturation, in which case an unbiased estimator would produce the observed result, $X(t_+)/\hat{m} \approx 1$. Two recent studies provide more compelling evidence that underestimation does indeed often occur. Analyzing weekly adoption data for 19 different food items using six different models, including the Bass model, Hardie, Fader, and Wisniewski (forthcoming) found that in more than 70 percent of the cases, the penetration ceiling estimated

Table 1 The Estimated Penetration Ceiling in Diffusion Models Is Often Very Close to the Penetration Observed in the Last Period

	Ratio of penetration in last observation period and the estimated penetration ceiling, $X(t_+)/\hat{m}$	
Original Bass model[a]		
Mammography scanners	1.07	
Tetracycline	1.00	
Ultrasound scanners	1.00	
Accelerated school program	0.99	
Foreign language education	0.96	
Room air conditioners	0.81	
Clothes dryers (1949–1961)	0.77	
Color televisions (1963–1970)	0.75	
	Excl. Price	Inc. Price
Bass model with dynamic population, heterogeneity, and nonuniform influence[b]		
Ranges	1.00	1.00
Disposers	0.98	1.02
Clothes dryers (1948–1979)	0.97	0.98
Calculators	0.97	0.97
Refrigerators	0.96	0.99
Room air conditioners	0.96	0.97
Freezers	0.94	1.02
Dishwashers	0.92	—
Ironers	0.89	0.88
Color televisions (1960–1986)	0.89	—
Black and white televisions	0.87	0.95
Water pulsators	0.81	0.98
Steam irons	0.78	—
Blenders	0.72	—
Ranges, built in	0.62	0.98

[a]Source: The tetracycline entry is based on Burt (1987, 1986). All others are based on Mahajan et al. (1986).

[b]Source: Parker (1992, Tables 1, 2, and 3).

using the first 26 weeks of data was less than the actual penetration observed at week 52. A simulation study by Debecker and Modis (1994) shows that underestimation of the asymptotic ceiling in the logistic curve is quite common and sizable when the cumulative penetration is less than 70 percent. This tendency to underestimate the asymptotic ceiling is not unique to diffusion models, but has been observed in other applications of nonlinear modeling as well (e.g., Gillis and Ratkowsky 1978).

A second recurrent pattern is that the estimate of q tends to decrease as one adds later observations to the data set (Balasubramanian and Ghosh 1992, Srinivasan and Mason 1986, Sultan et al. 1990). A related finding is that the estimated time of peak adoption in the Bass model, i.e., $\log(\hat{q}/\hat{p})/(\hat{p} + \hat{q})$, often increases as the number of years used in the estimation increases (Heeler and Hustad 1980).

A third recurrent pattern is that the estimate of q decreases systematically as the estimate of m increases. In a reanalysis of Griliches' (1957) study on the diffusion of hybrid corn, Dixon (1980) observed that Griliches' estimates of the penetration ceilings were closely related to the maximum recorded values in each U.S. state prior to 1957. Twenty years later, though, hybrid corn had achieved close to 100 percent penetration. Using these higher ceilings, Dixon found that most of the revised estimates of the slope parameter in the logistic diffusion model were lower than those Griliches obtained. In an earlier but little-noted study, Martino (1972) had already shown that the bias in the least squares estimate of the slope parameter of a logistic diffusion model is proportional to the error in the estimate of the upper limit, but with the opposite sign. Many studies fitting Bass, Gompertz, and logistic diffusion models show results in which the estimates of ceiling and slope parameters are strongly negatively correlated (Bretschneider and Mahajan 1980, Debecker and Modis 1994, Jain and Rao 1990, Jones and Ritz 1991, Meade 1984, Oliver 1981, Parker 1993, Tigert and Farivar 1981).

While the two patterns involving $\hat{q}$ have received some attention in the marketing literature, Parker (1994, p. 373) states that "little is generalizable concerning the extent of this phenomenon and whether there are systematic patterns of instability." Moreover, we are not aware of prior discussions of the pattern involving $\hat{m}$, except for the two recent studies mentioned. This is worrisome, because that empirical regularity may mean that $\hat{m}$ is often negatively biased, especially when the model is estimated with only few data points early in the diffusion cycle. If such a bias indeed exists, $\hat{q}$ will be affected as well (Martino 1972), which may explain the other two patterns.

This paper shows that in data structures similar to those of prior diffusion studies, the parameter estimates change systematically when one adds later data points to the data set, and that $\hat{m}$ and $\hat{p}$ are generally biased downwards, with $\hat{q}$ biased upwards. We also show that these estimation problems need not result from model misspecification: Biases and systematic changes can occur even when the diffusion process truly follows the Bass model.

2. Nonlinear Least Squares Regression Theory and Hypotheses

2.1. NLS Regression Theory

The two most common NLS-based estimation approaches for the Bass model are those of Srinivasan and Mason (1986) and Jain and Rao (1990). Both use the same statistical estimation technique (NLS), but apply it to slightly different model operationalizations. Defining $x(t)$ as the number of adopters in period t, the Srinivasan-Mason approach uses:

$$x(t) = m[F(t) - F(t-1)] + \varepsilon(t), \quad (4)$$

and the Jain-Rao approach uses:

$$x(t) = [m - X(t-1)]\,[F(t) - F(t-1)]/[(1 - F(t-1)] + \varepsilon(t), \quad (5)$$

where

$\varepsilon(t)$ = an independently distributed error term, and

$F(t)$ = the cumulative distribution function of adopters obtained from integrating Equation (2) assuming $F(0) = 0$;

$$= [1 - \exp(-(p+q)t)]/[1 + (q/p)\exp(-(p+q)t)]. \quad (6)$$

These functions are nonlinear in the parameters, so NLS rather than OLS must be used to obtain the least squares estimates. This has an important implication for the properties of these estimates, because the NLS estimator is not unbiased but only consistent. Parameter estimates converge in probability to the true values only as the number of observations approaches infinity (for any given error variance) or as the error variance approaches zero (for any fixed sample size) (Seber and Wild 1989). In addition, the characteristics of the NLS estimator, such as parameter bias and correlation matrix, are local, i.e., they depend on the true but unknown values of the parameters (Ivanov 1997). In sum, NLS estimates can be quite poor and biased when obtained from data sets with few and noisy observations, but there is no theory to predict their behavior in finite samples (Gillis and Ratkowsky 1978, p. 94). Hence, one cannot derive a practical, exact closed-form expression of the direction and size of the bias and use this expression to adjust one's estimates (Box 1971). Nevertheless, a few informative theoretical results exist. Let the nonlinear regression model for the Bass specification (e.g., Equation (4)) be represented as:

$$x(t) = g(t, \theta) + \varepsilon(t), \quad \text{where}$$

θ = the 3×1 parameter vector $(m, q, p)'$, and

$\epsilon(t)$ = error term, i.i.d. $N(0, \sigma^2)$.

Further, let

V = the $t_+ \times 3$ matrix of first derivatives $\partial g(t)/\partial\theta_r$ $(t = 1, \ldots, t_+; r = 1, 2, 3)$,

W_t = the 3×3 matrix of second derivatives $\partial^2 g(t)/\partial\theta_r\partial\theta_s$ $(r, s = 1, 2, 3)$, and

d = the $t_+ \times 1$ vector with elements $\mathrm{tr}\{(V'V)^{-1}W_t\}$,

where all derivatives are evaluated at the true parameter values. Box (1971) derived an approximation of the bias of the NLS estimator for θ, denoted as $b \approx E[\hat{\theta} - \theta]$, which can be expressed as:

$$b = -(1/2)\,\sigma^2\,(V'V)^{-1}V'd. \quad (7)$$

Cook, Tsai, and Wei (1986) later showed that extreme data points can have a substantial influence on the expected bias b. Note that the values of $(V'V)^{-1}$ will be large when the columns of V are nearly linearly dependent, indicating that the data are not sufficiently rich to clearly identify each parameter in the model, a phenomenon called ill-conditioning (Belsley 1990). In linear regression, where V corresponds to the design matrix, ill-conditioning often takes the form of collinearity and emerges when the regressors are too highly correlated to clearly identify all parameters or when, for a given pattern of correlation, the variability in the regressors is too low (Belsley 1990, Mason and Perreault 1991). Note that ill-conditioning does not lead to bias in linear models because all matrices W_t, and hence d, are zero.

Research experience with nonlinear growth models documents the effect of these factors. Seber and Wild (1989) describe two problems. First, the growth path may not have a large enough signal-to-noise ratio to identify more than one hazard parameter (p. 336). Second, the data may not cover a sufficient range to identify the ceiling parameter separately from the hazard parameter(s). Data from the early part of a diffusion process provide information confounding hazard and ceiling parameters, resulting in large covariances and poor point estimates (pp. 111–115). Berkey (1982, p. 959) experienced similar problems and, in line with the formal results obtained by Cook et al. (1986), remarked that "if the last . . . observations are missing, the least squares method may not have enough information to estimate the asymptote." In another application, Gillis and Ratkowsky (1978) found that the estimate of the asymptotic ceiling was often not only imprecise but also too low, i.e., the ceiling was underestimated. These results suggest that problems are more likely to arise when one stops observing the diffusion process before it is completed such that $X(t_+) < m$, a situation commonly referred to as censoring or right-censoring.

In sum, the statistical literature indicates that the NLS Bass parameter estimates are likely to be biased even when the model is correctly specified, and that this bias is related to both the size of the error variance and the lack of richness of the data. Practical applications of NLS regression to growth models suggest that problems are likely to occur in situations with: (i) too few observations, (ii) early censoring, and (iii) a little informative pattern in the data resulting in a poor "signal-to-noise" ratio. All three situations cause ill-conditioning, making parameter estimates sensitive to the addition or deletion of observations, and increasing the likelihood of sizable bias in the NLS estimates of Bass diffusion parameters.

2.2. Research Hypotheses

Our review of the literature on diffusion modeling (§1) and NLS regression (§2.1) suggests a number of hypotheses about systematic changes and biases. While we focus in this study on the Bass model and NLS estimation, we expect that similar problems exist with other diffusion models with an unknown ceiling and other estimation procedures using aggregate data.

Systematic Change. We propose three hypotheses on systematic change in the parameter estimates occurring as one adds later observations to the data set. We frame our hypotheses in terms of changes in both the number of observations and the amount of censoring, since the research reviewed above suggests that both affect the quality of estimates. Note that the number of observations and the extent of censoring are closely related, because the amount of censoring is the gap between the true ceiling and the observed penetration, which can be represented as $[m - X(t_+)]/m$. Since m is constant in the Bass model, changes in censoring are fully captured by changes in $X(t_+)$. Increases in t_+ result in non-negative changes in $X(t_+)$, meaning that the number of observations and the extent of censoring are closely linked. But it is possible to have a large value of t_+ and still have a high degree of censoring if the diffusion process is slow, so we separate the effects in our hypotheses:

HYPOTHESIS 1. *$\hat{m}$ increases as (a) the number of observations increases and as (b) censoring decreases.*

HYPOTHESIS 2. *$\hat{q}$ decreases as (a) the number of observations increases and as (b) censoring decreases.*

HYPOTHESIS 3. *$\hat{p}$ increases as (a) the number of observations increases and as (b) censoring decreases.*

H1 and H2 are in line with the findings of Debecker and Modis (1994), though their study did not distinguish between the two effects (a) and (b). As indicated in the literature review in §1, evidence in favor of H2a can be found in a few marketing studies as well. Except for an application reported in Sultan et al. (1990), we are not aware of either analytical or empirical prior support for H3, but include it based on our own experience.

Size of Bias. We propose three hypotheses regarding the extent of bias and its causes. The hypotheses are derived from statistical theory and are known to be approximately true, conditional on the data structure and the true parameter values. Hence, what we test is not whether these hypotheses are true in general, but whether they hold for the type of data structures encountered in diffusion research. Although the literature reviewed in §1 suggests that m may often be underestimated and q overestimated, formal statistical theory does not exclude the reverse, and we leave the direction of the bias open as a question to be answered by the hypothesis tests.[1]

HYPOTHESIS 4. *Higher error variance causes larger biases in (a)* $\hat{m}$, *(b)* $\hat{q}$, *and (c)* $\hat{p}$.

HYPOTHESIS 5. *A smaller number of observations causes larger biases in (a)* $\hat{m}$, *(b)* $\hat{q}$, *and (c)* $\hat{p}$.

HYPOTHESIS 6. *Earlier censoring causes larger biases in (a)* $\hat{m}$, *(b)* $\hat{q}$, *and (c)* $\hat{p}$.

The systematic change and bias hypotheses are interrelated once one knows the direction of the bias. For instance, if H6a is true and if m is typically underestimated, then H1b should also hold. We test H1–H3 on both real and simulated data. We test H4–H6 on simulated data only, since one must know the true data generating process to identify and quantify bias.

3. Empirical Analysis

We test the three change hypotheses using a number of well-known data sets. We first estimate a three-parameter Bass model for each data set using NLS, at different levels of t_+. Next, we pool the estimates into a panel and test the hypotheses by modeling the change in the estimates as a function of the changing levels of t_+ and $X(t_+)$. Such analysis addresses not only whether there is support for the posited relationships, but also how strong the effects are. To make our conclusions as general as possible, we use data sets for a variety of innovations from different time periods obtained using different data collection methods by different researchers.

3.1. Diffusion Data and Modeling

To minimize data problems and to have the results be representative of the published literature, we used four criteria when building our database. (1) Data must have been analyzed previously and published by other researchers. Thus, diffusion researchers should be familiar with the data or with the studies in which they were used. (2) Data sets must contain at least 10 observations, at least one of which comes after the inflection point, to reduce the risk of nonconvergence and random parameter instability due to extreme data sparseness (Heeler and Hustad 1980, Srinivasan and Mason 1986). (3) The size of the population or sample, say M, must be known and constant, to reduce the risk that the number of eventual adopters m ($m \leq M$) changes over time. (4) Adoptions must be distinguished from multiunit purchases or replacements by previous adopters, because the Bass model assumes a binary adoption process.

Appendix A lists the data we used. They cover a variety of situations: Adopters range from households, to professionals, to businesses and institutional organizations, while the innovations range from an inexpensive drug, to larger ticket items such as consumer durables, to radical and expensive innovations such as medical imaging equipment. The data originate from different streams within diffusion research: sociology, marketing, economics, education, and political science. We rejected data from 19 other studies because they did not meet one or more of our criteria.

For each data set, we estimated the three-parameter Bass model with NLS using both the Srinivasan-Mason and the Jain-Rao operationalization. Since NLS results are sensitive to the choice of starting values, we used a grid search for p and q. We used M as the starting value for m, and only when we did not reach convergence within 100 iterations did we bring the starting value down toward $X(t_+)$. We constrained the parameter estimates to be non-negative. We repeated the procedure varying t_+. We chose the minimum value of t_+

[1]In a preliminary analysis, we applied Box's bias approximation to the Srinivasan-Mason operationalization of the Bass model for different values of p, q, error variance, and number of observations. In most cases, the approximation predicted a downward bias in $\hat{p}$. Often, it predicted an upward bias in $\hat{q}$. We did not obtain a predicted downward bias for $\hat{m}$, however. Because these analytical but approximate results run somewhat counter to prior reported research results, we do not specify the direction of the bias in H4–H6. For almost all the cases we examined, the predicted bias obtained using Equation (7) behaved according to hypotheses H4 and H5 as stated here. Regardless of the direction, the bias tended to be smaller as t_+ increased and as the error variance decreased. The majority of exceptions happened for situations in which t_+ was prior to the diffusion curve's inflection point.

so that the shortest time series still contained at least one period beyond the inflection point and was at least 10 periods long. We then added one period at the time to the estimation sample, and repeated the estimation procedure above. We estimated 56 different sets of parameters for each operationalization.

3.2. Analysis and Results

We pooled the parameter estimates from every innovation at all levels of t_+, and tested our hypotheses by regressing the parameter estimate of interest on t_+ and $X(t_+)$. As mentioned above, increases in $X(t_+)$ capture decreases in censoring. To control for cross-innovation heterogeneity, we used a least squares dummy variable regression model. Using the subscript by r to identify each parameter ($r = 1$ for m, $r = 2$ for q, and $r = 3$ for p), the model has the form:

$$\log[y_{irt}] = \delta_{ir} + \Sigma_k \beta_{kr} \log[Z_{ikt}] + e_{irt}, \qquad (8)$$

where

y_{irt} = estimate for innovation i obtained using t_+ data points,

δ_{ir} = innovation-specific fixed effect,

Z_{ikt} = kth regressor of interest, $X(t_+)$ ($k = 1$) or t_+ ($k = 2$),

β_{kr} = scale-free elasticity with respect to kth regressor, used to test our hypotheses,

e_{irt} = normally, independently distributed random error with mean zero.

This specification has the advantages of (i) capturing within-series rather than across-series variance and (ii) not requiring one to know the true value of p, q, or m, which is captured by the parameter δ_{ir}. We first pooled all 56 "observations" into a panel, and estimated model (8) for each hypothesis. We discarded one observation, air conditioning at $t_+ = 13$, that heavily influenced the level of error in the regression for H1 (e.g., studentized residual = 8.9) and created heteroscedasticity problems. We thus have 55 data points for hypothesis testing. Because the correlation between $X(t_+)$ and t_+ is likely to result in regression artifacts, we assess their effects both separately and jointly.

Table 2 reports the results. Considering models in which $X(t_+)$ or t_+ enter alone, all hypotheses are supported. $X(t_+)$ is clearly a better predictor than t_+, since the model fit is always greater for $X(t_+)$ alone than for t_+ alone. Also, adding t_+ to a model that already contains $X(t_+)$ does not improve model fit much,

Table 2 Regression Results for Test of Systematic Change Hypotheses on Empirical Data, Using $X(t_+)$ Alone, t_+ Alone, and Both Together

	Srinivasan-Mason operationalization			Jain-Rao operationalization		
	$X(t_+)$ ($\hat{\beta}_{1r}$)	t_+ ($\hat{\beta}_{2r}$)	R^2	$X(t_+)$ ($\hat{\beta}_{1r}$)	t_+ ($\hat{\beta}_{2r}$)	R^2
H1	0.442[c] (0.085)	—	0.335	0.610[c] (0.095)	—	0.435
($r = 1$)	—	0.258[c] (0.068)	0.212	—	0.376[c] (0.077)	0.306
	0.856[c] (0.225)	−0.327 (0.165)	0.381	0.998[c] (0.254)	−0.306 (0.186)	0.463
H2	−0.891[c] (0.142)	—	0.423	−1.360[c] (0.205)	—	0.448
($r = 2$)	—	−0.618[c] (0.108)	0.376	—	−0.931[c] (0.159)	0.389
	−0.813[a] (0.390)	−0.062 (0.286)	0.423	−1.344[c] (0.254)	−0.010 (0.415)	0.448
H3	1.380[c] (0.326)	—	0.248	1.700[b] (0.545)	—	0.153
($r = 3$)	—	0.635[b] (0.262)	0.098	—	0.768[a] (0.423)	0.058
	4.258[c] (0.790)	−2.275 (0.580)	0.417	5.410[c] (1.397)	−2.930 (1.026)	0.266
($N = 55$)						

[a] $\alpha < .05$; [b] $\alpha < .01$; [c] $\alpha < .001$ (one-sided t tests).

Note. The left-hand entries in the columns $X(t_+)$ and t_+ are the estimated elasticities of the Bass model's NLS parameter estimates with respect to the variable of interest. The term in parentheses is the estimated standard error. The R^2 is the proportion of within-innovation variance explained by the explanatory variables, i.e., the proportion explained of the variance remaining after controlling for innovation-specific effects.

but leads to a sign reversal for the effect of t_+. This is probably due, at least partly, to collinearity (cf. Darlington 1968). For $X(t_+)$, all the coefficients have the expected sign and the null is rejected for all three hypotheses at the $\alpha = .05$ level or better, whether only controls for the number of observations or not. The effects in models featuring $X(t_+)$ only are sizable: A 10 percent increase in observed penetration is associated with, roughly, a 5 percent increase in estimated market size $\hat{m}$, a 10 percent decrease in $\hat{q}$, and a 15 percent increase in $\hat{p}$. We also ran analyses controlling for differences in data periodicity (monthly, semi-annual, annual, and four-year) and for the varying number of observations per innovation, with essentially the same conclusions.

4. Simulation Analysis

To test the three bias hypotheses and to check whether systematic changes can occur even in absence of any misspecification, we conducted a Monte Carlo simulation study. We manipulated the three sources of ill-conditioning identified above and error variance, and combined them into a full-factorial design. We manipulated the signal-to-noise ratio by varying the shape of the true diffusion curve through the q/p ratio (signal) over four levels and by varying the level of error variance (noise) over five levels.[2] We varied the length of the data series used in the model estimation, t_+, over four levels: 10, 12, 14, and 16. To avoid the problems in the empirical analysis and to manipulate censoring independently from t_+, we made the speed of diffusion low versus high by reducing the values of both p and q by 20 percent, leaving their ratio unchanged. Thus, our design has 160 cells: Signal (4) × Noise (5) × Speed (2) × Number of observations (4). For each cell, we created 50 data series for which we estimated the Bass model using NLS, resulting in 8,000 estimations; 7,085 of these converged. We used the Srinivasan-Mason operationalization for both data creation and parameter estimation. (Details of the design and procedures are available on request.)

Bias Analysis. Table 3 reports how the cell medians for the estimates relate to the factors driving ill-conditioning. Overall, the results confirm the importance of ill-conditioning on the performance of the NLS estimator: A small number of periods, a high degree of error, and low diffusion speed result in a downward bias for $\hat{m}$ and $\hat{p}$ and an upward bias in $\hat{q}$. The size of the biases can be quite substantial. For conditions

Table 3 Factors Driving Ill-Conditioning Affect the Parameter Estimates in the Simulation: Analysis of Variance on the Percentage Deviation of Cell Medians from True Value[d]

	$(\hat{m} - m)/m$	$(\hat{q} - q)/q$	$(\hat{p} - p)/p$
Base	0.049[c]	−0.232[c]	0.023
q/p ratio			
2	−0.036[c]	0.336[c]	0.007
5	0.02	0.062	0.013
50	−0.043[c]	0.068[a]	−0.142[c]
Low speed	−0.043[c]	0.136[c]	−0.004
Error variance			
Moderate ($\sigma = .42$)	−0.039[c]	0.091[b]	−0.058[c]
Average ($\sigma = .60$)	−0.097[c]	0.222[c]	−0.121[c]
High ($\sigma = .78$)	−0.169[c]	0.392[c]	−0.218[c]
Number of observations			
$t_+ = 14$	−0.008	0.009	−0.018
$t_+ = 12$	−0.034[c]	0.071[a]	−0.063[c]
$t_+ = 10$	−0.113[c]	0.227[c]	−0.077[c]
R^2	0.85	0.78	0.79
($N = 128$)			

[a] $\alpha < .05$; [b] $\alpha < .01$; [c] $\alpha < .001$ (two-sided t tests).

[d]We regressed the cells' median percentage deviation on the factors manipulated in the simulation using a main effects dummy regression. As expected from NLS theory, the NLS regressions always attained consistency as the error approached zero, regardless of the number of observations included, speed of diffusion, or q/p ratio. Because such a pattern is not compatible with a main effects specification, we excluded the lowest level of error variance ($\sigma = 0.06$), reducing the number of cells from 160 to 128. The base condition is a "best case" with parameter values $p = 0.03$ and $q = 0.38$ ($q/p = 13$, high speed), low error variance ($\sigma = 0.24$), and $t_+ = 16$.

[2]Assuming that the true errors are proportional to the probability of adoption in period t (Debecker and Modis 1994, Dixon 1980, Srinivasan and Mason 1986), we created the perturbed adoption data $x(t)$ using a multiplicative error specification: $x(t) = \psi(t)\epsilon(t)$, where $\psi(t)$ are the true adoption data series generated from the Bass model and $\epsilon(t)$ are random errors generated from a log-normal distribution with mean 1 and variance $\exp\{\sigma^2\} - 1$. We selected five levels of error variance: σ equaling .06, .24, .42, .60, and .78. The three highest values reflect the levels of error variance found in the 12 data sets. We included the two lower values to check whether we attained consistency as $\sigma^2 \to 0$ regardless of the number of observations included, as NLS theory posits.

representative of many marketing applications (p = 0.024; q = 0.304; σ = 0.60; and t_+ = 10), m can be underestimated by 20 percent, p underestimated by the same amount, and q overestimated by 30 percent. These biases are larger than those reported by Srinivasan and Mason (1986, p. 175), who performed a small simulation analysis (N = 50) and concluded that "the biases in the parameter estimates $\hat{p}$, $\hat{q}$, and $\hat{m}$ (e.g., $|\bar{\hat{p}} - p|/p$) were less than 7%." Their results, however, were based on a single set of parameter settings, less serious censoring and less noise in the data than the average in our 12 data sets and simulation. When using similar parameter values, censoring, and noise levels, we found results similar to those of Srinivasan and Mason. We conducted formal tests of H4, H5, and H6 using the same model as in Table 3, but with σ^2 and t_+ entering as continuous regressors. These tests reject all but one of the null hypotheses at $\alpha < .001$ (two-sided t tests) and show that the direction of the biases is the same as in Table 3. The exception is H6c: Earlier censoring, operationalized as low speed, is not significantly associated with the bias in $\hat{p}$.

Systematic Change Analysis. We also used our simulation results to perform a second test of H1, H2, and H3 under conditions where misspecification can be excluded as a confounding factor. We used the same procedure as in the analysis of the empirical data: a least squares dummy variable model with log-transformed variables (Equation (8)). As the dummy variables δ_{ir} capture the effects of the q/p ratio, low versus high speed, and the true error variance, these three variables cannot enter in the models. We restricted the data to parameter estimates generated from data series with σ equaling .42, .60, or .78, noise levels one is more likely to encounter in practice, and discarded 40 data points that were the only remaining member of their series, reducing the sample to 3,946 data points. H1b, H2b, and H3b are again strongly supported. The elasticities of the parameter estimates with regard to $X(t_+)$ as the sole predictor are very similar to those found in the empirical analysis for the Srinivasan-Mason operationalization: about 0.5 for $\hat{m}$, -0.7 for $\hat{q}$, and 1.3 for $\hat{p}$, all with $\alpha < 0.001$ (one-sided t tests).

5. Can Model Misspecification Explain the Occurrence of Systematic Changes?

The simulation study shows that the systematic change in the parameter estimates observed in the empirical study can occur even when the model is correctly specified. The analysis of bias indicates why: p and m tend to be underestimated, and q underestimated, but these biases become smaller as one adds observations to the data set. Hence, the systematic changes can be explained as the result of better conditioning and decreased bias. There are some indications that conditioning improvement and bias reduction may indeed have driven the changes we observed. The fact that $\hat{m}$, $\hat{q}$, and $\hat{p}$ change more for the more complex Jain-Rao operationalization suggests that the changes may have originated from a gap between the richness of the data and the complexity of the regression models. Formal conditioning analysis (Belsley 1990) reveals that the Bass model parameters we estimated show indications of ill-conditioning. For seven of the data sets, the rate parameters p and q load quite heavily (95 percent or higher) on the same component of the parameter correlation matrix. On the other hand, the typical condition number was well below 30, the value suggested by Belsley (1990) to distinguish large from small condition numbers in OLS (we are not aware of any similar rule of thumb for NLS). Thus, though the systematic change in parameter estimates of the 12 innovations may have originated from ill-conditioning, the evidence is not conclusive. An alternative explanation for the observed systematic changes is the presence of various types of model misspecification, such as population growth, omitted variables, and unobserved heterogeneity. In this section, we assess the merits of this alternative explanation.

One explanation for the changes in $\hat{m}$ that occur as one adds later data points is that the populations, and hence the true penetration ceilings m, increase over time. This is a viable explanation for systematic changes in many studies using U.S. sales data for consumer durables over the period 1950–1985, when the U.S. population increased from about 150 million to about 240 million. However, population growth cannot explain systematically changing parameter estimates in studies using penetration data or adoption data from samples or censuses with a fixed size, as is the case with all 12 data sets we used.

Omitting time-varying variables is a second cause of misspecification that may explain systematic changes in parameter estimates. The Bass model does not account for the effect of declining real prices, increasing distribution penetration, and improving product performance. Such variables may lead the penetration ceiling to increase even in a population of fixed size (e.g., Kalish and Lilien 1986). For instance, as the real price of refrigerators drops over time, lower-income households become able to afford them, leading m to increase. Under such conditions, one can expect that the NLS estimate $\hat{m}$ will not only systematically increase, but also be biased downwards (Xie et al. 1997). Omitted variables may also affect the rate parameters. One can expect omitted variables that are positively (or negatively) related to the hazard rate and increase (or decrease) at a decreasing (or increasing) rate to result in a downward pressure on the estimate of q as one adds more observations to the data set.[3] For instance, the fact that product performance often increases more rapidly in the first years after launch than in later periods (Bayus 1994, Trajtenberg 1990) and is likely to affect the decision to adopt may explain why $\hat{p}$ and $\hat{q}$ in a traditional Bass model often change as one extends the number of years used in the estimation.

A third cause of model misspecification that might explain systematic changes in the parameter estimates is unobserved heterogeneity. For instance, assume all people have the same intrinsic tendency to adopt, captured by p, but differ in their susceptibility to social contagion, captured by q. At any time, those with the highest q have a higher adoption hazard $h(t)$ and tend to adopt earlier. As a result, the population of remaining eventual adopters is increasingly made up of people with a lower q as time progresses. As a result, $\hat{q}$ may decrease as one extends the number of periods in the estimation. Unobserved heterogeneity in p can induce a similar downward pressure on the slope of the estimated hazard rate (cf. Blossfeld et al. 1989), and hence affect the estimate of q.

If the tendency for parameter estimates to change as one adds later observations to the data set were due mostly to model misspecification rather than the result of ill-conditioning, then the changes observed in §3.2 should be less severe once one makes the model more flexible. To assess whether this indeed is the case, we analyzed the behavior of three extensions of the Bass model: the non-uniform influence model, which allows the social contagion effect to decrease as the level of penetration goes up (Easingwood et al. 1983), the non-uniform influence model with a multiplicative control for the hazard rate (e.g., Parker 1992), and the generalized Bass model (Bass et al. 1994). We collected data on control variables (nominal and deflated price, average income, and hedonic price or profitability) for six of the 12 innovations in Appendix A (color TVs, dryers, air conditioners, CT scanners, and the two hybrid corn series), and used the same procedure as before to test H1–H3. The results (details available on request) show that the extended models exhibit the same pattern as the Bass model: $\hat{m}$ increases and $\hat{q}$ decreases as the observed cumulative penetration increases. (As we had to fix the value of p in a few cases to obtain estimation convergence, we did not assess changes in $\hat{p}$.) Moreover, we found the extended models to be more sensitive to changes in the underlying data than the Bass model. These results indicate that nonuniform influence or omitted variables are unlikely to account for the systematic changes reported in §3.2. Rather, the results provide further support to the ill-conditioning explanation: Making the model specification more complex and increasing the number of parameters make the regression problem even more ill-conditioned and the estimates more unstable.

In conclusion, both estimation problems and model misspecification can explain the systematic changes observed in the empirical analysis. Because better specified models are often also more complex, their estimation is more problematic. As a result, developing more complete models to reduce misspecification bias may actually lead to even more pronounced systematic changes.

[3]This is in line with the generalized Bass model or GBM specification (Bass et al. 1994). If one estimates a Bass model on diffusion data generated by a GBM process, then the estimates of p and q will be biased but they will not change systematically if the omitted variables change at a constant rate.

6. Discussion

Using both empirical and synthetic data, we have shown that systematic changes occur in the *nonlinear* least squares (NLS) parameter estimates of the Bass model. We have shown using synthetic data that these estimates are biased and that these biases are related to ill-conditioning: A small number of observations, a high degree of error, and early censoring result in a downward bias for $\hat{m}$ and $\hat{p}$ and an upward bias in $\hat{q}$. Although in empirical studies the systematic change and bias problems may be aggravated by model misspecification, the latter is not required: Ill-conditioning alone can generate systematic change and bias. We also found that, *given the ill-conditioning of the data, it is* hard to address the model misspecification problem by making the model more flexible and adding free parameters. Making the model more complex can actually aggravate the tendency for parameter estimates to change systematically as one extends the data set. Based on NLS theory, we suspect that increasing model complexity can also increase the bias stemming from ill-conditioning, but we did not directly assess this conjecture. While we focused on the NLS estimation procedure and the Bass model here, similar problems are likely to exist with other macro-level diffusion models featuring an unknown ceiling and other estimation procedures (e.g., MLE, OLS)—a poor signal-to-noise ratio degrades the performance of any estimator. Below, we discuss the relevance of the bias and systematic change problems and make a few suggestions.

6.1. Relevance for Predictive, Prescriptive, and Descriptive Uses

Some researcherss may be willing to trade off some bias to gain the benefits from a useful model. Others may correctly point out that ill-conditioning is not a major issue if one is only interested in the shape of the diffusion curve or its inflection point, since ill-conditioned parameter estimates can vary substantially as a consequence of relatively minor perturbations in the data while the predicted curves are very similar in shape over the range covered by the estimation data. These points should be viewed with caution because the bias and systematic change problems are serious enough to make predictive, prescriptive, and descriptive applications of Bass-type models suspect.

Market Size Assessment (Predictive Use Outside the Data Range). Our results indicate that managers interpreting $\hat{m}$ as the eventual penetration a new product or technology can be expected to achieve may seriously underestimate their market potential. Managers who believe the output of the model might invest insufficient resources in product and market development, so that the belief that the market is close to saturation becomes a self-fulfilling prophecy.

Marketing Strategies (Prescriptive Use). Decision makers may also be misguided by the inflated $\hat{q}$, leading them to underspend on advertising and overuse penetration pricing. Normative models of advertising in a diffusion environment generally suggest high initial levels of advertising followed by a gradual decline (e.g., Horsky and Simon 1983). How fast advertising should decrease depends on the strength of the word-of-mouth effect: The higher q, the faster social contagion takes over the demand-inducing role of advertising, and the faster one can reduce one's spending. Our results indicate that, since the true q is not as high as its estimate, taking $\hat{q}$ at face value could result in underinvestments in advertising. The inflated word-of-mouth effect may also result in overusing penetration pricing. Horsky (1990) derived the following decision rule for monopolists: Penetration pricing is optimal if word-of-mouth effects are so large that $q > (2p + k)/4F(t)$, where k stands for the cost of capital. If q is smaller, price skimming is more profitable. From Horsky's formula, it is clear that an inflated $\hat{q}$ can lead firms to choose penetration pricing too often. Estimating a three-parameter Bass model for clothes dryers over the period 1950–1964, for example, one would estimate the critical cost of capital for clothes dryers to be 49 percent. Estimating p and q from a two-parameter model in which m is exogenously fixed to the actual penetration in 1979 (61.5 percent), the critical cost of capital would be only 9 percent, a value at which most monopolists would skim rather than penetrate.

Diffusion Acceleration (Descriptive Use). Many managers and academics believe that diffusion cycles are shortening. Olshavsky (1980) and Takada and Jain (1991) reported that the slope parameter of the hazard rate in a diffusion model is positively related to the year of launch for consumer durables. Others, however, have not found evidence that more recent consumer durables have a higher diffusion rate (e.g., Bayus 1992). Our results suggest that evidence supporting diffusion acceleration may be at least partly due to a method artifact, since both Olshavsky (1980) and Takada and Jain (1991) used more data points to estimate diffusion parameters for earlier innovations than for more recent ones. Hence, the bias we have identified may have been higher for recent than for old innovations, artificially inflating the former's parameter estimates $\hat{q}$, which Olshavsky (1980) and Takada and Jain (1991) used as a measure of diffusion speed. To investigate this competing explanation, we regressed the parameters reported in these two studies against not only the year each innovation was launched, but also against the number of observations used to estimate these parameters. We found that the positive relationship between diffusion speed and year of launch disappears once one controls for the number of observations used in the estimation (Table 4). This result casts some doubt on the accepted belief of accelerating diffusion cycles.

6.2. Methodological Suggestions

The biases and systematic changes are not minor technicalities that can be practically ignored. Though ill-conditioning is a problem for which no quick fix is available (Belsley 1990), one can make a few methodological suggestions based on the extant literature and our findings.

Simplifying the Model. Our results caution against estimating market potential using aggregate-level diffusion models. Across a variety of innovations, we found those estimates to be strongly driven by the level of adoption observed in the last period for which data are used. Using exogenous estimates of m obtained from market surveys, secondary sources, management judgments, or other models can lead to better results (Mahajan et al. 1993, p. 391, Parker 1994). If multiple exogenous estimates of m are available, Trajtenberg and Yitzhaki (1989) suggest investigating how sensitive the other parameters and the shape of the diffusion curve are to these different values of m. Using an exogenous ceiling also linearizes the regression problem in the adoption domain (based on

Table 4 Prior Evidence That Diffusion Speed (q) Is Positively Related to Year of Launch Disappears When Controlling for the Number of Observations Used to Estimate the Diffusion Models

	Model 1 Year of launch	Model 2 Number of Observations	Model 3 Both Factors[b]
Olshavsky (1980)[c] (N = 25)			
Intercept	−0.025	0.368[a]	0.285
Year of launch	0.005[a]		0.001
Number of observations		−0.006[a]	−0.005
R^2	0.45	0.52	0.53
Takada and Jain (1991)[d] (N = 26)			
Intercept	0.112	0.797[a]	0.879[a]
Year of launch	0.006[a]		−0.001
Number of observations		−0.022[a]	−0.024[a]
R^2	0.38	0.63	0.64

[a] $\alpha < 0.001$.

[b] Note that the launch year effect becomes insignificant once one controls for the number of data points used to obtain the parameter estimates.

[c] The regression estimated is $\hat{q} = \beta_0 + \beta_1$ year $+ \beta_2 t_+$, and its nested variants. Olshavsky obtained the parameter estimate $\hat{q}$ from a logistic diffusion model (i.e., $p = 0$) in which he fixed the ceiling m exogenously as the maximum penetration achieved (i.e., $m = X(t_+)$). He reported slightly different estimates for Model 1, but the difference is very small and the statistical inference is not different from ours.

[d] We estimated the same regression models as for Olshavsky. Because Takada and Jain's data come from four countries, we also estimated alternative models in which we controlled for product and country effects. The results were similar and are not reported here. Takada and Jain estimated the slope parameter q using the traditional three-parameter Bass model.

Equation (3)) such that higher quality estimates for p and q may result. Parker (1994) suggests another way to reduce the number of free parameters: fixing p to $x(1)/m$. Our results suggest that using the simpler Srinivasan-Mason rather than the Jain-Rao operationalization may also reduce the tendency for parameter estimates to change.

Getting More Data. Increasing the number of data points and including observations from the tail end of the process will improve the performance of the NLS estimator. If extending the length of the data series is not an option, using data of higher periodicity could alleviate the ill-conditioning problem, but only if data quality does not deteriorate and the cumulative penetration is sufficiently close to the ceiling. Putsis (1996) found that the quality of OLS and NLS estimates for the Bass model improved when shifting from annual to quarterly data, but improved only marginally when going from quarterly to monthly data. Unfortunately, it is not clear to what extent the improvements he recorded were driven by reduced temporal aggregation bias or by better conditioning. Future research distinguishing between these two effects may be useful.

Using Box's Bias Equation. Box (1971) and Gillis and Ratkowsky (1978) show that Box's bias equation (Equation (7)) in which the derivatives are computed using the estimated rather than the true but unknown parameter values can give a good indication of the extent of bias. If this procedure proves effective for diffusion models, it would allow researchers to compute "debiased" estimates. We expect, however, that innovation diffusion models typically feature more specification errors than the models for physical processes studied by these authors, so that the bias calculation—which assumes that the model is correctly specified and the error is totally random—may be much less precise. A rigorous study of the performance of Box's bias approximation and its corresponding "debiasing patch" in a context representative of innovation diffusion applications would therefore be quite useful.

6.3. Conclusion

We have identified and analyzed systematic changes and biases in the NLS parameter estimates of the Bass model. Although there is still some ambiguity about why such problems occur regularly, there is abundant evidence that they do exist. Until we have a better understanding of why these patterns occur and of how to avoid or control for them, researchers and managers would benefit from exercising caution. Expecting a simple time series with a handful of noisy data points to foretell both the ultimate market size and the time path of market evolution is asking too much of too little data.[4]

Appendix A. Adoption Data[a]

		Adoptions in period *t*[b]																		
	X(0)	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	*M*[c]
Air conditioner	0.7	0.1	0.1	0.4	1.9	1.3	1.1	2.0	1.9	2.2	1.1	2.3	1.9	1.8	0.6	0.8				100
Clothes dryer	0.7	0.7	1	1.2	1.4	1.6	2.6	2.7	1.8	1.9	2.2	1.8	1.5	1.8	1.6	0.7				100
Color television	5.1	4.4	5.5	11.2	9.5	2.5	4.3	8.6	9.6	6.4	4.4	2.9	3.3	3.6	3.9	4.6				100
Corn (1948)	0	7	9	4	17	14	41	40	54	89	83	43	22	7	3					433
Corn (1943)	0	1	2	4	4	6	1	6	16	21	36	61	46	36	14	3				259
Tetracycline	0	11	9	9	11	11	11	13	7	4	1	5	3	3	4	4	2	1		125
Ultrasound	0	5	3	2	5	7	12	6	16	16	28	28	21	13	6					209
Mammography	0	2	2	2	3	4	9	7	16	23	24	15	6	5	1					209
CT scanner	0	1	5	9	18	42	74	89	91	159	146	94	81	73	69	73	57	60	61	3,078
Foreign language	0	1.25	0.77	0.86	0.48	1.34	3.56	3.36	6.24	5.95	6.24	4.89	1.25							100
Accelerated program	0	0.67	0.48	2.11	0.29	2.59	2.21	16.80	11.04	14.40	6.43	6.15	1.15							100
Compulsory school	0	1	1	0	1	1	5	6	2	4	3	3	1	2	1					31

[a]Period of measurement, periodicity, and data sources: Air conditioner (1950–64), clothes dryer (1950–64), and color television (1956–79): annual figures computed from penetration data published in *Merchandising Week* and *Merchandising*, various issues. Hybrid corn: annual data for 1927–41 from Ryan and Gross (1943) and annual data for 1929–42 from Ryan (1948). Tetracycline: monthly data from Nov. 1953 to Feb. 1955 from Burt (1986). Ultrasound and mammography scanner: annual data (1965–78) from Mahajan et al. (1986). CT scanner: semi-annual data (1973–1981) computed from Trajtenberg and Yitzhaki (1989). Foreign language education and accelerated school program: annual data (1952–63) from Mahajan et al. (1986). Compulsory school attendance: four-year data (1849–1902) computed from United States (1921).

[b]For each innovation, the underlined data point corresponds to the highest level of censoring used in the estimation.

[c]These are the true sizes of the population or sample (*M*), which need not be identical to the number of eventual adopters, i.e., the adoption ceiling (*m*).

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