

# Predictive and non-predictive validation strategies for system dynamics models

By M. Sommer

## Dokumentation

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## Zusammenfassung

Ausgehend von dem „klassischen“ Validierungskonzept von System Dynamics, das eher prognostische Methoden bevorzugt (Abschn. 2), werden die wichtigsten von Forrester und seinen Mitarbeitern erhobenen Einwände gegen statistische Testverfahren zur Modellvalidierung untersucht (Abschn. 3). Die kritische Beurteilung dieser Forschungsarbeiten zeigt, daß man die aus nicht-prognostischen Verfahren resultierenden Informationen als wichtigen Schritt im Rahmen eines mehrstufigen Validierungskonzepts anerkennen und nicht ablehnen sollte (Abschn. 4).

## Abstract

Predictive and non-predictive validation strategies for system dynamics models

Starting out with the "classical" system dynamics validation concept which favours predictive instead of non-predictive procedures (section 2), we will investigate the crucial objections which Forrester and his co-workers have recently launched against statistical tests as model validation procedures (section 3). The critical appreciation of these research activities results in a plea for accepting rather than neglecting the informations yielded by non-predictive tests as an important step within a multi-stage validation concept (section 4).

## Résumé

Stratégies de validation prédictives et non-prédictives pour les modèles de system dynamics

En partant du concept «classique» de validation de system dynamics, qui encourage plutôt les procédés prédictifs que les procédés non-prédictifs (2ème partie), nous nous proposons d'analyser ici les objection cruciales que Forrester et ses assistants ont récemment soulevées à l'encontre de l'utilisation des tests statistiques comme procédés de validation des modèles (3ème partie). L'appréciation critique de ces recherches montre que l'on devrait plutôt avoir tendance à accepter et non négliger les informations résultant des tests non-prédictifs et les considérer comme un élément important dans le cadre du concept de validation en plusieurs étapes (4ème partie).

## 1 Introduction

Validation of dynamic, socio-economic models has long been one of the most controversial issues between followers of a more data-based methodology – mainly econometricians – and those of a more concept-based philosophy – mainly system dynamicists [1; 2]. In the last few years some attempts have emerged to assess the degree of compatibility of econometrics and system dynamics and to examine at least four possible future relations between these two important modelling approaches: *dominance* of one approach and finally elimination of the other [3], *convergence* into a single

and broader methodology [4; 5, 6], *passive coexistence* in different ecological niches [1] and different forms of *active cooperation* [7; 8; 9; 10; 11]. Although model validation is just one aspect within a much wider range of characteristics – see [12] for a comprehensive treatment of the subject – it has been the central feature of a sometimes eclectic discussion between both parties long before the just mentioned efforts for systematic inter-paradigmatic comparisons were started. These controversies may well be traced back to the 1962 discussion between Forrester, Holt and Howard [13], i. e. one year after the publication of "Industrial Dynamics" [14].

## 2 The "classical" system dynamics validation theory

### 2.1 Forrester's view

The main source of the SD-validation<sup>1</sup> theory is Chapter 13 "Judging Model Validity" in [14], which is still considered to be obliging by most system dynamicists (see e. g. [15]). Following Forrester's views, validity is not an absolute and "truth"-searching but rather a relative and *purpose-dependent concept*. Since the dominant purpose of SD-modelling is the improvement of a real system's dynamic behaviour, a model's *final validity* rests upon the success of system redesign recommendations as results of model experiments.

Two problems arise with final validity:

1. since final validity can only be assessed after system redesign it cannot guide the redesign recommendations themselves,
2. it is very hard to examine which part of behavioral improvement in the real system is actually to be attributed to the proposed policy change [15].

Therefore the researcher has to confine himself to an evaluation of the model's *interim validity*, which exists of two components:

#### 1. *non-predictive validation*

- a) of the set of endogenous variables necessary for describing the relevant behaviour modes (system boundary),
  - b) of the interconnections between variables including time-lags and functional forms (specification analysis),
  - c) and least important the validity of chosen parameter values;
2. *predictive validation* of the correspondence of model and real system behaviour by reasonable crisis tests, and examination of the model's ability to reproduce trouble symptom, periodicities, time-phase relationships and transition characteristics observable in the real system.

Because of the dominant purpose of real-system improvement by model-guided policy change, behaviour correspondence would just then be not only a necessary but also sufficient prerequisite for final validity, if it were achieved by a structurally valid model.

## 2.2 Consenting and dissenting views

### 2.2.1 Model purpose

EC as well as SD recognize that model validity heavily depends on model purpose which may easily be under-

<sup>1</sup> for the abbreviations see the list at the end

pinned by a quote from a multi-authored, paradigmatic article on econometric model evaluation:

"In the current state of our knowledge and analytical needs, to concentrate our attention solely on proving or disproving the 'truth' of an econometric model is to choose an activity virtually guaranteed to suppress the major benefits which can flow from the proper use of econometric models. Having constructed the best models of which we are capable, we ought to concern ourselves with whether or not particular models can be considered to be reliable tools for particular uses, regardless of the strict faithfulness of their specification. In this context, 'validation' becomes a problem-dependent or decision-dependent process, differing from case to case as the proposed use of the model under consideration changes. Thus a particular model may be validated for one purpose and not for another [16]."

In spite of very similar statements by SD-authors [14; 15] one should be careful not to stretch the unison too far, because "model purpose" apparently has a double meaning as can be demonstrated by the following passage:

"Thus a model which accurately predicts the employment effects of alternative tax policies may be considered 'successful' even if its prediction of the composition of GNP is poor by the standards for other uses of a model [16]."

SD-followers accord to the relevance of a *specific purpose* the model should serve, but disagree on the more *general aspect* of the model purpose: while EC favours accurate predictions of future system states as a desirable goal of knowledge to be gained from modelling, SD holds for prediction of global behaviour characteristics in order to achieve system improvement. This dissent on the proper model purpose is of overwhelming but often underrated importance not only for understanding differences in model structures (e. g. the SD-verdict against and the EC-allowance for exogenous variables) but also for distinctions in the way predictive validations are performed.

### 2.2.2 Predictive validation

Forrester has given broad scope to the necessity to distinguish between prediction of behaviour characteristics and the prediction of future system states and argues that only the former is able to contribute to a model's interim validity [14]. This position has been questioned by many critics, e. g. Apel [17]:

"A model, whose variables reproduce the dynamic behaviour of a real system, only shows that the model already hits the system characteristics. But why employ quantitative modelling, if one rests content with this?"

Actually, objections like these raise doubt against the feasibility of Forrester's modelling goals (system improvement) and implicitly call for the econometric goal of accurate predictions, but they are not sound criticisms of Forrester's validation by behaviour mode prediction as they claim to be. In fact, it can be demonstrated [12] that Forrester's insistence on predictive validation by behaviour correspondence tests is quite defensible, provided one accepts his view of model purposes. On the other hand, it is equally plausible that a model's capability to produce dynamic behaviour similar to that of the real system is not sufficient, if accurate prediction of ex ante values of endogenous variables at certain points of time were the postulated goal of the modeling effort. We believe that the focus of the validation controversies should not be the alternative between behaviour prediction and future state prediction within the realm of predictive validation itself – because the choice between the two so much depends on the chosen purpose – but should rather concentrate on questions about the relation between predictive and non-predictive validation.

### 2.2.3 Non-predictive validation

We did already touch the crucial point that behaviour correspondence can only be relevant for final validity, if it is accomplished by a structurally valid model. Avoiding to be caught in a vicious circle obviously requires means of assessing the structural and parameter correspondence between a model and the reality *independently* of model behaviour tests. Forrester himself admits that the latter tests are “meaningful only because we believe *independently* that the causal relationships of the actual system are represented in the mechanisms of the model. An endless variety of model details having no similarity to the actual system could be assembled that would create the . . . (model) curve [14]”.

A careful comparison of section 4.7 on “Sources of Information for Constructing Models” and section 13.4 in the validation chapter of [14] substantiates that Forrester recommends to use the same information for validating parameters and specification assumptions (structure) that have already been incorporated within the prior formulation of the model:

“In the *design and justification* of a model, we need to call upon the full variety of knowledge that is available about the system. Most of our knowledge is in the experience and the minds of people who have observed and worked with the system. Much information is in the descriptive literature. Only occasionally will there be numerical and statistical evidence sufficient to settle

important model-building questions [14].” (See also [18; 19; 20].)

Since using the same information twice cannot yield any genuine insights, this strategy must be regarded as a *spurious validation*. On another occasion Forrester did even go further arguing that only poor models present insurmountable questions of validation, in other word: “good” models do not require validation, “bad” models cannot be exposed to validation [21]. The whole validation topic – including Forrester's own treatment – is thus in danger of erosion and might in the end be regarded as a *spurious problem*. But this may not be the last word, because the question remains unanswered how one knows which models are the “good” ones and which belong to the “bad” species.

Forrester's reluctance to employ statistical tests in the stage of parameter and specification evaluation obviously leads us back to his analogous opinion in “Industrial Dynamics” that statistical methods for parameter estimation are superfluous. It would indeed make no sense to evaluate the validity of parameters, which have been estimated in an informal ad hoc fashion, by statistical test tools. Due to limited space we have to sustain from appraising the SD-theory of ad hoc parameter estimation (see [12] sect. 4.5 for a detailed discussion), which draws heavily on two assumptions:

1. *Insensitivity* of model behaviour to most parameter values wherefore their exact values need not be known anyway, and
2. direct *observability* of parameters from the real system.

With respect to parameter observability we have to diagnose a fundamental dissent between SD and EC. On the scale which ranks data and a-priori-information used in the process of parameter value determination, we find as extreme cases SD-models at one end which use only a-priori-information, and EC-models at the other end relying only on data information [22]. But there seems to be a contradiction in the SD argumentation against formal parameter estimation: while claiming that parameters do not have to be estimated because they are observable, variables are held to be frequently unobservable which in turn prohibits statistical parameter estimation because of lack of data on variables. At least for socio-economic models on a macro-level, the official statistics provide a vast amount of time-series and cross-section data on variables but almost none on parameters. This is implicitly acknowledged by Lehmann, whose SD-model of the Federal Republic of Germany [23] partially draws on time-series data for variables but not for parameters. Although he recognizes that EC could complement SD in the areas of estimation and validation, he does not make any use of these offers within his own model.

The consequence of this discussion is rather simple: the proposition of the "classical" SD-validation theory which regarded statistical methods for estimating and evaluating parameters as well as equation specifications as superfluous is not defensible. This leaves a logical gap in the SD parameter determination philosophy as well as in the SD-concept of interim validity, causing its pretended multi-stage validation procedure to shrink into the single stage of predictive validation of model behaviour.

### 3 New SD-objections against non-predictive validation: some critical remarks

Probably as a by-product of the extensive work on an SD National Model of the USA, the increasing econometric challenge to the SD-methodology has encouraged the reoccupation with the statistical testing of parameters and specifications – in line with research on econometric estimation of SD-models:

"Although the literature of regression analysis and econometrics dominates the social sciences in describing the use of data for relating real life to models, much new light can be shed on the proper and possible uses of statistical methods by experiments such as Peter Senge is now conducting at MIT (not yet published) [24]."

"The laboratory tests indicate that the generalized least-squares data analysis can give not only major errors in the estimates of parameters but also *misleading indications from the internal measures of validity* [25]."

These statements must be considered as a remarkable shift within the SD-validation theory: while in "Industrial Dynamics" statistical tests were assumed to be an acceptable though mostly useless and only exceptionally necessary instrument for independently supporting faith in the parameter values and model structure, they are now judged as even dangerous because their internal validity criteria are supposed to provide wrong inferences.

#### 3.1 The methodology of the estimation-validation experiments

The methodology of Senge's experiments [26] is well known from Monte Carlo studies which have been performed in EC to assess the small sample properties of various estimators when certain "classical" assumptions of the linear regression model are violated [27, 28; 29]. Senge used a linear-in-the-parameters version of Forrester's Market Growth Model [30] to produce synthetic data instead of real world data for the parameter estimation. These data can be corrupted by errors in variables

to take account of sampling and measurement errors. Furthermore, it is possible to study the effects of misspecifications of structural equations and of econometric hypotheses about the residuals on the parameter estimates and test statistics.

#### 3.2 The main results and some critical comments

*Ideal conditions:* Using error-free data and the same model specification for parameter estimation that has generated the data base, OLS yields very satisfactory parameter estimates for the Market Growth Model [26]. Runs 1 and 2 (see Table 1) confirm that the OLS-parameters are hardly biased and differ from zero at a 10 %-level of significance.

Table 1: Parameter estimates of Forrester's Market Growth Model

Parameters	True values	Ideal conditions (OLS)	Measurement errors (GLS)	Misspecification (GLS)
K1	475.0	486.8	252.8	199.3
K2	-61.5	-61.13	-31.6	-44.5
K3	-0.6178	-0.6686	-0.2942	-
K4	0.1324	0.144	0.066	-
K5	-0.00975	-0.01066	-0.00586	-
K6	-	-	-	0.133
K8	0.6178	0.6317	0.0876	
K9	-0.1324	-0.1335	-0.0246	
K10	0.00975	0.00962	0.00227	
K11	-1.0	-1.029	-0.1024	
K12	0.0003	0.0003288	0.000511	
K13	-0.05	-0.0575	-0.098	
K14	-0.0698	-0.0752	-0.0345	
K15	0.1244	0.1366	0.0364*	
K16	-0.08138	-0.08983	-0.0133*	
K17	0.02704	0.02887	0.01049	
	Run 1	Run 2	Run 3	Run 4

\* parameters not significant at a 10 % confidence level

*Errors in variables: acceptance of inaccurate parameters.* A 10 % random error of measurement causes inaccurate GLS-estimates for many parameters. Nevertheless Senge's main objection against econometric parameter estimation and testing which he derives from this experiment, is not directed against the inaccuracies but towards the fact that all parameters except two (K15 and K16, see run 4) are significant. "Hence estimation results relying on statistical significance measures would lead to the acceptance of estimates which are, in fact, quite inaccurate [26]."

There are two problems with Senge's interpretations of his results. The first one is mentioned by himself: "Bias occurs in the estimates . . . because measurement errors violate a key assumption made in both OLS and

GLS estimation. Both techniques assume that the error process is uncorrelated with each of the explanatory variables. This assumption is satisfied in the ideal case, but not when the explanatory variables are measured imperfectly [26].”

We think that OLS and GLS should not be blamed for being unable to solve problems they were not designed to tackle. We rather believe that an estimation technique adequate for errors in variables conditions, should have been chosen (see e. g. [28; 29, 31]). Peterson [32] has proved with the same model that employing an adequate estimation technique renders excellent parameters.

Senge’s second critique against the acceptance of inaccurate parameters is even more problematic. Following the usual rule of thumb, a t-statistic greater than 2 implies significance of the parameter and only tells that the estimated parameter certainly *differs from zero* at the chosen confidence level – it does not give an indication whether there is a significant *bias between the actual and true value*. It would have been preferable, if Senge had also applied a t-test for this latter hypothesis which is possible under experimental conditions where the true parameter value is known. So far there is no reason why the estimated parameters should not differ significantly from zero only because they are biased. Looking at Senge’s results we would rather come up with an almost contradictory conclusion: the t-values greater than 2 indicate that the respective variables can be regarded to inhibit a good deal of explanatory power for the dependent variable of the particular equation in spite of biased parameter values due to an inadequate estimation technique.

*Acceptance of a misspecified functional form.* In another experiment Senge changed the true nonlinear dependence of the delivery rate on the production capacity into a linear function.

True delivery rate equation (see Table 1, run 1):

$$DR = [-0.6178 \cdot (BL/PC) + 0.1324 \cdot (BL/PC)^2 - 0.00975 \cdot (BL/PC)^3] \cdot PC \tag{1}$$

Estimated linear equation<sup>2</sup> (see Table 1, run 4):

$$DR = 0.133 \cdot PC \tag{2}$$

(t = 2.8)

Although it is true that “the erroneous capacity utilization estimate is statistically significant, thereby giving the model-builder no

2 Actually, the delivery rate equation was estimated as a part of the backlog equation. We will not comment on the possible pitfalls of such a procedure in this paper.

warning of the consequences of the structural misspecification” [26],

we could again like to draw attention to the other and positive side of the same coin: the estimate still confirms that the production capacity is a relevant variable for the explanation of deliveries; non-significance would not necessarily have led to the idea that something might be wrong with the way PC enters the DR-equation, but could as well have suggested to drop PC at all. On the other hand: the researcher is free to try a non-linear formulation as well as linear one, specially when his a-priori-information raises doubts about a constant capital utilization factor.

*Misleading test statistics: where do they lead to?* Section 3 started with Forrester’s thesis that internal measures of validity might be misleading. At a first glance Senge’s experiments seemed to support this statement. But besides the so far exposed objections, the question has to be brought up why the outcome of Senge’s econometric estimation of Forrester’s Market Growth Model should be hazardous for system dynamicists. Since they are not interested in obtaining as accurate as possible estimates of individual parameters like

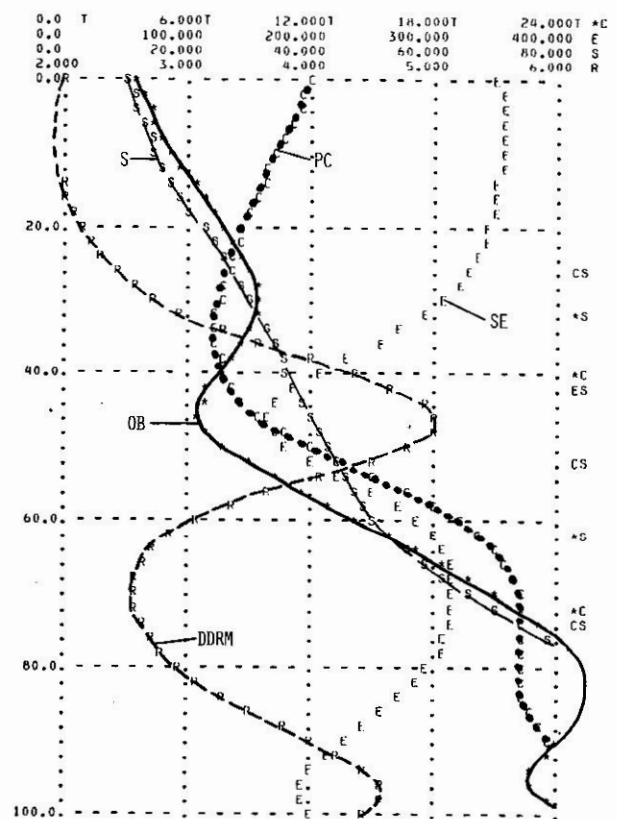


Figure 1: Reference run (run 1)

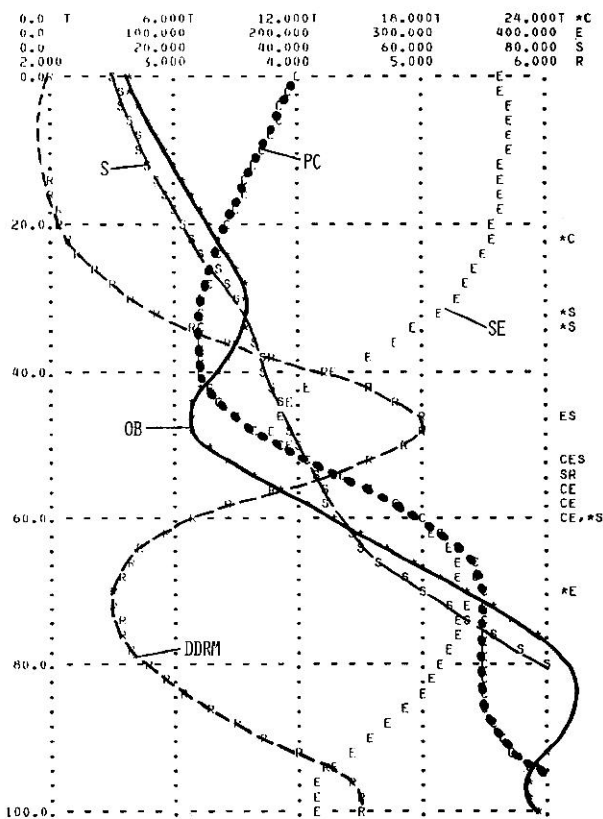


Figure 2: Ideal conditions (run 2)

econometricians are, a bias in parameter estimates is not per se a problem. The bias only becomes critical if it questions the main SD purpose: behaviour mode prediction. Only in this sense would it be justified to speak of “misleading indications from internal measures of validity”. Since it is well known that “optimal properties of the individual estimates of the coefficients are not a necessary prerequisite for the good predictive performance of a model [29]” it should be interesting to supplement Senge’s estimation experiments with an analysis of their impact on the behaviour modes – otherwise the heart of the SD-validation theory.

Figures 1 through 4 provide the simulations with the parameter estimates of the above discussed experiments (see again Table 1). The reader may ascertain himself that the general dynamic characteristics are not destroyed in runs 2 to 4 compared to the reference run 1. It is easy to imagine that the correspondence could well be improved with some of the above mentioned refinements. All in all, the simulations do not support the negative conclusions Forrester and Senge have drawn from their investigations.

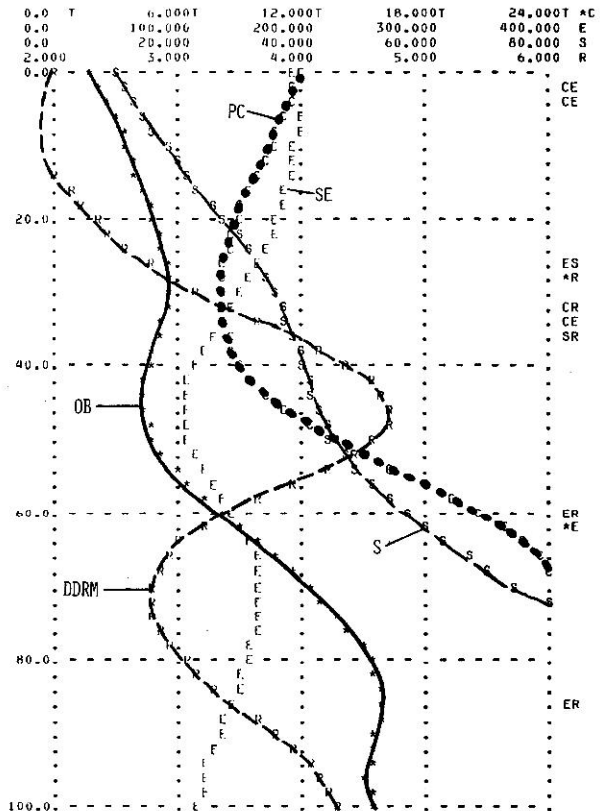


Figure 3: Errors in variables (run 3)

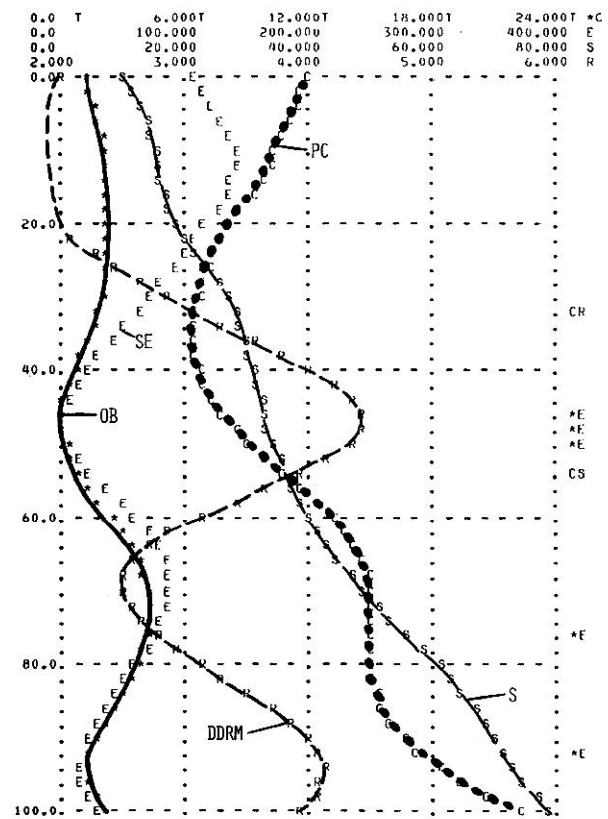


Figure 4: Misspecification (run 4)

#### 4 Conclusion

We feel that the simulation results do not contradict the statistical validity tests but rather corroborate them. Furthermore, it should not be neglected that unlike in an experimental set-up, the true parameter values as well as some aspects of the structural specification are unknown or at least rather uncertain in actual model-building. This assigns econometric procedures an important role within a sound concept of multi-stage validation, where different evaluation techniques make independent contributions. Thus, there seems to be a good chance that the apparent contradictions between predictive and non-predictive validation might be cleared not by neglecting but rather by accepting the latter. These chances are improved when the non-predictive validation is performed with a careful combination of economic, statistical, and econometric criteria [29].

#### List of abbreviations

BL	= Backlog
DDRM	= Delivery delay recognized by market
DR	= Delivery rate
EC	= Econometrics
GLS	= Generalized least squares estimation
OB	= Orders booked
OLS	= Ordinary least squares estimation
PC	= Production capacity
S	= Salesmen
SD	= System Dynamics
SE	= Sales effectiveness

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