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Preface

It is often difficult to rigorously evaluate Model-Driven Engineering (MDE) technologies. Performing empirical studies require skills, experience and tacit knowledge that are in many ways very different from the "core" MDE research. Furthermore, empirical studies often entail large investments in terms of human resources, time and money. Nevertheless, evaluations of MDE technologies are needed in order to demonstrate the soundness, applicability, and cost effectiveness of proposed technologies in various development contexts.

The aim of this workshop is to exemplify and discuss ways in which proposed model-driven engineering (MDE) technologies should be evaluated, with a specific emphasis on how to plan, conduct, analyze and report the results of empirical studies. The workshop will have focus on the challenges of empirical studies involving human users, since MDE technologies are typically expected to be used by software engineers to improve various quality aspects of software systems and the productivity of software development. More detailed topics include: What are the main obstacles and potential remedies when performing empirical studies of MDE? What are the main threats to validity of empirical studies of MDE, and how should they be dealt with? For example, since MDE often represent new and complex technology, the selection and training of human subjects who participate in empirical studies often become critical factors. What are the most important outcome variables of the costs and benefits of MDE? How can quality be measured in the context of MDE? And can we define an unambiguous set of (benchmark) outcome measures to facilitate meta-analyses across subjects, systems, tasks and technologies?

The goal of the workshop is to pave the way for the development of a MDE-specific framework for empirical evaluation of MDE technologies, or at least provide a minimum standard for evaluation that published work in the MDE community should abide by.

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Content

On the Quantitative Assessment of Class Model Compositions: An Exploratory Study

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Abstract. Model composition can be viewed in model-driven engineering as an operation where a set of activities should be performed to merge two input models into a single output model. The latter aggregates syntactical and semantic properties from the original models. However, given the growing heterogeneity of model composition strategies, it is particularly challenging for designers to objectively assess them given a particular problem at hand. The key problem is that there is a lack of canonical set of indicators to quantify harmful properties associated with the output models, such as composition conflicts and modularity anomalies. This paper presents an inquisitive study in order to capture an initial set of metrics for assessing and comparing model composition strategies in two case studies. We apply a number of metrics to quantify different conflict types and modularity properties arising at composite class models produced with override and merge-based strategies. We have observed that some of the quantitative indicators were effective to pinpoint when a model composition strategy is not properly chosen. In some cases, the output models exhibited non-obvious undesirable conflicts and anti-modularity factors.

Keywords: Model Composition, MDE, Metrics, Assessment.

1 Introduction

Given the central role that model composition plays in model-driven engineering nowadays, researchers are increasingly focusing on defining and improving alternative techniques for composing structural or behavioural models. Model composition can be defined by a composition operation, a special type of model transformation, that takes two models Ma and Mb as input models and combines their elements into an output model Mab. Several mechanisms have been proposed in order to put model composition into practice (e.g., see [2, 3, 4, 5, 6]), based on related work

in many different domains, such as database integration [7], aspect-oriented modeling, model transformation , and merging of state charts.

However, not much attention has been paid to the quality assessment of such model composition techniques. Even worse, according to [5] there is very little experience that can be used to determine the worth of current approaches. Given the growing heterogeneity of model composition strategies [3], such as *override* and *merge*, it is intrinsically difficult to systematically quantify undesirable phenomena that arise in the output composite models, including abstract syntax conflicts and semantic clashes. It is particularly challenging for researchers or designers to objectively assess the output model and the composition strategy itself given the problem at hand.

In this paper we start to tackle such needs through an exploratory study (Section 2) on assessing composition strategies for class models. The goal is to inquisitively identify an initial set of indicators for the evaluation and comparison of alternative composition strategies. We have applied a metrics suite (Section 3) to quantify the conflicts rate and modularity properties arising in class model compositions based on merge and override. Our long-term goal is to define a comprehensive assessment framework intended to guide researchers and designers on the assessment of model composition techniques. In our study, we have detected that some of the used quantitative indicators were effective to determine when a model composition strategy is not properly chosen (Section 4). In certain cases, the output models exhibited non-obvious syntactic and semantic conflicts and a number of modularity anomalies not existing in the original input models. We also contrast the initial findings of our exploratory investigation with related work (Section 5). Finally, we present some concluding remarks (Section 6).

2 Experimental Procedures

This section describes the experimental procedures used in our exploratory study. Two case studies were performed in order to investigate possible problems associated with the use of composition strategies for class models. The first study comprises a set of real-life models for an Automated Highway Toll System. In this case, different members of a distributed software development team were in charge of modeling different use cases of the system. They would need to cope with model composition problems when bringing the use cases together.

There are three packages, namely (for simplification) Packages A, B, and C, where each of them implements a set of use cases. There are two explicit compositions defined for these packages (Figure 1). Package A presents a UML class diagram that specifies basically functionalities related to: create user account, add funds, and stop toll booth. Package B specifies functionalities related to: synchronizes accounts, process credit card, transponder and vehicle. While the Package C specifies functionalities related to add transponder and start toll booth. The goal is to produce an output Package that gathers all functionalities together. To this end, we need to merge the Package A, B and C according to a particular composition strategy (*override* or *merge* specifically). The choice of a particular composition strategy is

very important to produce sound output models while not introducing modularity impairments.

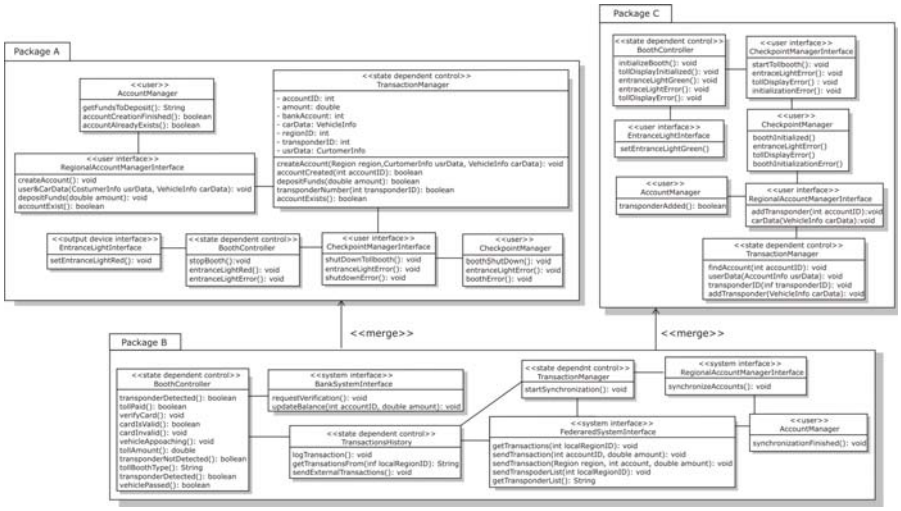

Figure 1. Example of composition of an automated highway toll system.

The second study consists of a literature-based [11] example of a calculator that is depicted in Figure 2. It has two packages: (1) Package A presents a UML class diagram that specifies a Calculator to implement two basic functionalities: sum and subtraction; and (2) Package B represents a Calculator that implements three functionalities: sum, division, and multiplication. The goal is to produce an output Calculator that contains four operations: sum, subtraction, division, multiplication. To do this, we need to put these functionalities together in a single Package by merging Package A and Package B.

Our aim is to assess in which ways the composition strategies (*override* and *merge* specifically) impact on the input models' properties. The *merge strategy* usage is more appropriate when the input design models contain specifications for different requirements of a software system. On the other hand, the *override strategy* can be indicated when elements in an existing model need to be somehow evolved or changed. The semantics of the override strategy [3] can be briefly defined as: (i) for all elements in the Package A and Package B that are corresponding, the Package A's element should override its corresponding element; and (ii) elements in the Package A and B that are not involved in a correspondence match remain unchanged and they are inserted into the output model (Package AB).

The semantics of the merge strategy [3] can also be defined as: (i) for all elements in the Package A and Package B that are corresponding elements, they should be combined; and (ii) elements in the Package A and B that are not involved in a correspondence match remain unchanged and they are inserted into the output model (Package AB). However, when we put these elements together in the output model (as the result of either overriding corresponding elements or adding elements in

the Package AB directly) may result in some problems such as semantic clashes. We will propose a metrics suite to provide ways to assess how useful or harmful such composition relationships are following a specific composition strategy. The goal is to provide initial support for designers and researchers objectively analyze which composition strategy minimizes the conflicts rate while maximizing modularity benefits in the output model.

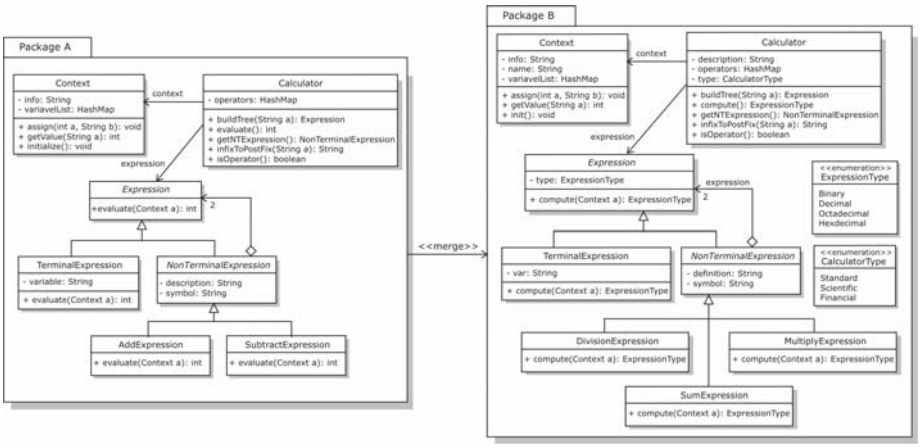

Figure 2. Example of composition of calculators

3 A Metrics Suite for Model Composition

This section presents the metrics suite defined for assessing the model compositions in our exploratory study. This framework guides the researchers for assessing and coping with difficulties of UML model composition assessment.

3.1 Quantifying Composition Conflicts Rate

Number of Abstract Syntax Conflicts **(NAbSC)**
This metric counts the number of *abstract syntax conflicts* in a class model. *Abstract syntax conflicts* occur when a model does not comply with the UML metamodel' metaclasses and their structural relationships. It is a well-known problem, for instance, in graph transformations. The goal is to quantify and check inconsistencies of the target models against the UML metamodel. Once all the conflicts have been addressed (i.e. NAbSC = 0), the output model can be considered as compliant to he UML metamodel. Otherwise, the output model is an invalid or non-compliant model.

$$NAbSC = \sum_{i=1}^{SM} k_i \ ,$$

where:
SM – a set of model elements.
k_i – the number of AbSC of the i-th model element.

***Number of Semantic Clash Conflicts (*NSCC)**
This metric counts the *number of semantic clash conflicts* in a model. A semantic clash conflict occurs when model elements have different names, however, with same semantic value. We need to quantify such conflicts in order to identify unexpected semantic clash problems in the output models. For instance, models with semantic clashes may become ambiguous and inconsistent. In addition, it may affect the model understandability or complicate some tasks such as model transformation and code generation. If the NSCC has a high value, it may imply that the output model is useless. This metric is given by the formula:

$$NSCC = \frac{1}{2}\sum_{i=1}^{SM} w_i \ ,$$

where:
SM – a set of model elements.
w_i – a boolean value that represents if an i-th model

***Number of Compositions of a Model Element* (NCME)**
This metric counts the number of compositions that a model element has participated. The number of compositions may be an effective indicator of *semantic mix conflict*. When model elements are composed, their semantics are mixed and it may lead to unsound model elements. For example, a design pattern assigns roles to their participant classes, which define the functionality of the participants in the pattern context. When UML class diagrams are merged such roles may be modified having negative impacts on quality attributes of the design pattern. This metric is given by the formula:

$$NCME = |M| \ ,$$

where:
M – the number of compositions that a model element has participated during the composition process.

***Number of Behavioral Feature Conflicts* (NBFC)**
This metric counts the number of behavioral feature conflicts in a class. A *behavioral feature conflict* may occur when a class: (1) has two (or more) methods that are used with the same purpose, and (2) refers to a method that no longer exists, or exists under a different behavior that is not expected. The high NBFC measure may represent some undesirable model composition phenomena. This metric is determined by the formula:

$$NBFC = |B| \ ,$$

where:
B – the number of behavioral feature (method) conflicts in a class

***Number of Unmeaning Model Elements* (NUME)**
This metric counts the number of unmeaning model elements in a model. During the composition process, the model elements are manipulated and sometimes some elements are not referred nor make reference to other elements, that is, they are isolated. This metric is given by the formula:

$$NUME = |U| \ ,$$

where:
U – the number of unmeaning model element in a mode.

3.2 Quantifying Modularity Anomalies in Composite Models

We have also applied some classical metrics intended to measure some modularity-related characteristics of a class, such as coupling degree, number of attributes, and operations. These metrics are described in Table II. Due to space constraints, these metrics are briefly presented. In fact, most of these metrics (e.g. NATC and CBC) were originally defined by other authors and their definitions can be found in their respective publications [14, 17]. The goal of using these metrics is to assess how the composition process affects the output models regarding some design principles, such as low coupling, when we specify different composition strategies. In addition, in many cases, composition strategies can artificially lead to the introduction of design anomalies ("bad smells"), such as "Temporary Field"; this bad smell can be identified comparing the NATC of a class in the output model against the respective classes in the input models used for the composition.

Table I. The Class-level Modularity Metrics

Metric	Description
Number of Attributes in a Class (NATC)	Counts the number of attributes in a class.
Number of Operations in a Class (NOPC)	Counts the number of operation in a class.
Number of Associations between Classes (NASC)	Counts the number of associations per class; the new language produced from a model composition may not be consistent with the domain defined previously.
Coupling between Classes (CBC)	Counts the number of all dependencies of a class to other classes in the system.
Number of Subclasses of a Class (NSUBC)	Counts the number of children of a class.
Number of Superclasses of a Class (NSUPC)	Counts the parents of a class.

4. Results and Discussion

Quantitative assessment is an effective way to supply measures and evidence that may improve our understanding about model-driven engineering techniques, in our case, model composition. Although quantitative studies have some disadvantages, they are very useful because they boil a complex situation down to simple numbers that are easier to grasp and discuss. This section provides a general analysis and discussion of the data that have been collected from applying the set of defined metrics to model compositions derived in the two case studies (Section 2).

Graphics are used to represent the data gathered in the measurement process. The Y-axis presents the absolute values gathered by the metrics. Each pair of bars is attached to an integer value, which represents the measure. The X-axis specifies the metric itself. These graphics help analyzing how the composition of the input models affects (or not) the output model regarding a particular metric. These graphics support an analysis of how the change of composition strategy affect (or not) the output model. The results shown in the graphics were gathered according to the model point

of view; that is, they represent the total of metric values associated with all the model elements for each model (output model) that is being considered.

Figure 3 depicts the overall composition results between Package A, B and C of the Automated Highway Toll System following the override and merge strategy. We compare the output model produced by the override and merge strategy and it is possible to observe that no measure was detected to the metrics such as NSUNC, NSUPC, NAbSC, and NSCC. On the other hand, the NOPC metrics have a higher measurement following merge strategy than override strategy. This observation can indicate a negative point considering reusability. Although not showing differences between each other with regard to the NASC, NSUBC, and NSUPC metrics, the output models presents significant differences, for example, the Package BAC produced by the merge strategy presents all functionalities defined in the Package A, B and C, while the Package BAC produced by the override strategy contains only functionalities defined in the Package B.

According to the measures concerning the number of associations between classes (NASC), the number of abstract syntax conflicts (NAbSC), and the number of subclasses (NSUBC) and superclasses of a class (NSUPC), no significant difference was detected in favor of a specific composition strategy when applied to the two case studies. The measures of NSUBC and NSUPC can be easily explained because both case studies do not exhibit a hierarchy-depth in their inheritance relationships. On the other hand, the measure of NASC supplies evidence of that number of associations of a Class contains is independent of type of composition strategy.

Package BAC produced by the override strategy provided higher results in two measurements, NCME and NUME. When *EntranceLightInterface* is inserted in the Package BAC this class becomes unmeaning, because of the class that it makes relationship, *PackageA.BoothController*, no longer exists (*PackageA.BoothController* is overridden by the *PackageB.BoothController*). The NASC and CBC measurement have same values. So the coupling in the Package BAC is independent of the kind of composition strategy in this case.

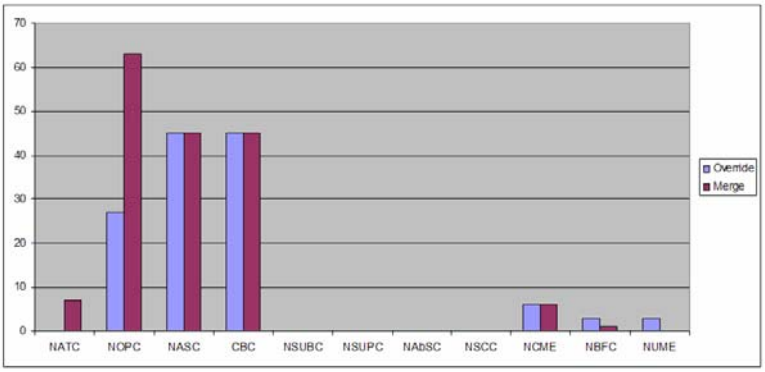

Figure 3. Comparison between output models produced following override and merge strategy.

The measurement regarding the output Calculators has provided some results that are depicted in Figure 4. We compare the output Calculators produced following override and merge strategy. Although not showing differences between each other regarding the NASC, NSUBC, and NSUPC metrics, the output models present significant differences. The Package AB produced by the merge strategy has higher values for some metric measures such as NATC, NOPC, CBC, NSCC, NCME, and NBFC. On the other hand, Package AB produced by the override strategy provided higher results in one only measure, NUME, because two enumerations, *CalculatorType* and *ExpressionType,* are unmeaning in the Package.

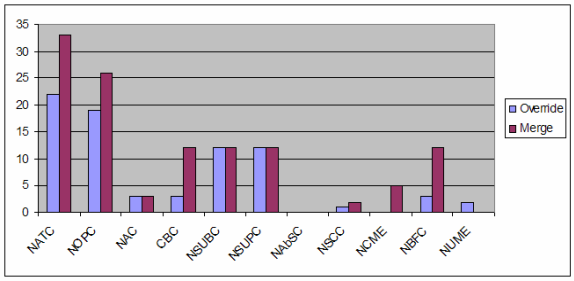

Figure 4. Comparison between calculators produced following override and merge strategy.

According to the data gathered, the most useful metrics in this exploratory study were as follows. First, number *of semantic clash conflicts* (NSCC) as it indicated the presence of a significative number of negative semantic clashes. This measure served as warnings of not helpful output models using a particular composition strategy through the identification of ambiguity and inconsistency arising in semantic clashes. The observation of Figure 3 provides evidence of the effectiveness of this metrics. Second, *number of unmeaning model elements* (NUME) supplied evidence that override strategy is potentially harmful when used beyond the purposed of evolving or changing an existing model. The output models, based on override-driven compositions, had elements that are not referred nor make reference to other elements, that is, they are isolated (unmeaning in the package). Thus, regarding this metric the better strategy to be applied in the case studies was the merge strategy.

Finally, after observing all the conflict rate and modularity results, the metrics indicated that the merge strategy is the best strategy to be used in our two case studies. This finding is also mainly based on the measures of NUME and NSCC (discussed above). Moreover, we should highlight that, as expected, it is particularly challenging for researches to objectively assess the output models and identify conflicts associated with some metrics such as NUME, NAbSC and NSCC. Therefore, the issue of improving automated support for measuring conflict rates should be a topic of future work.

5. Related Work

There is little related work focusing on either the quantitative assessment of models in general or on the quantitative assessment of model compositions. Up to now, most

approaches involving model composition rest on subjective assessment criteria. Even worse, they lead to dependence on experts who have built up an arsenal of mentally-held indicators to evaluate the growing complexity of design models in general [5]. As a consequence, the truth is that modelers ultimately rely on feedback from experts to determine "how good" the input models and their compositions are. According to [5], the state of the practice in assessing model quality provides evidence that modeling is still in the craftsmanship era and when we assess model composition this problem is accentuated.

To the best of our knowledge, the need for assessing models during a model composition process neither have been pointed out nor even proposed by current model composition techniques [2, 3, 4, 8, 9]. For example, the UML built-in composition mechanism, namely *package merge*, does not define metrics or criteria to assess the merged UML models. Moreover, it has been found to be incomplete, ambiguous and inconsistent [6].

The lack of quantitative indicators for model compositions hinder our process of understanding better side effects peculiar to certain model composition strategies. Many different types of metrics have been developed during the past few decades for different UML models. These metrics have certainly helped designers analyze their UML models to some extent. However, as researchers' focus has shifted to the activities related to model management (such as model composition, evolution and transformation), hence the shortcomings and limitation of UML model metrics have become more apparent. Some authors [1, 12, 13-18] have proposed a set of metrics that consider UML model's properties. These works have shown that their measures satisfy some properties expected for good measures of design models. However, these metrics can not be employed to assess problems that may arise in a model composition process such as semantic clashes.

6. Concluding Remarks and Future Work

If models are seen as primary development and transformation artifacts in model-driven engineering, then software designers naturally become concerned with how their quality is evaluated. In order to be considered for use in mainstream software development, model composition techniques should be supplemented with quality criteria and indicators. These elements are fundamental for developing and analyzing composition processes and output models. We presented an exploratory study and an initial metrics suite for assessing class model compositions generated by a selected set of model composition strategies. Such metrics are applied to output models and some analysis are performed according to the data gathered.

Our initial evaluation has demonstrated the feasibility of our candidate set of metrics for quantifying modularity properties and conflict rates in composition processes. Obviously, more investigations on its applicability to large UML model compositions are required. Further empirical evaluations are indeed fundamental to validate our quantitative indicators in real-world design settings involving UML model compositions. Thus, future work will concentrate on designing and carrying out a family of empirical studies to assess, for example, compositions of the most popular OMG's UML profiles in realistic scenarios. Finally, we should point out that

model composition assessment is in initial stage and there is very little experience that can be used to determine the feasibility of current approaches. Moreover, its empirical-driven improvement, supported by a comprehensive set of well-validated metrics suite, is absolutely necessary to the evolution of model-driven engineering field. This work represents one of the first stepping stones towards this end.

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Preparing Meta-Analysis of Metamodel Understandability

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Abstract. Metamodels are designed to be used by machines and humans. For human users, the understandability of the metamodel is important. Experimental investigations of understandability in computer science have led to conflicting results. To resolve such conflicts and gain insights into the nature of some phenomenon beyond singular experiments, meta-analysis can be applied, i.e., the statistical analysis of results obtained by other (primary) empirical studies. This paper shows the current obstacles for a meta-analysis of metamodel understandability: They consist in the heterogeneity of the individual experiments and deficient reporting. The paper provides a framework to increase the comparability of experiments on understandability. Such comparability enables future meta-analysis.

Keywords: Understandability, Metamodels, Experimental Research

1 Motivation

Designing and modifying metamodels are major topics of model-driven development. Metamodels must be understandable for both machine and human users. Following a definition of language understandability in cognitive psychology [1], the *understandability* of a metamodel means the effort required to read and correctly interpret its constructs and their connections. Understandability is a prerequisite both for reading artifacts (like documents or source code) that have been created by a applying a metamodel (*comprehension*) and for creating such artifacts (*specification*).

The 'understandability' of a metamodel for a machine shows up in error-free compilation. For human users, metamodel understandability must be empirically investigated, usually by controlled experiments. The results of such experiments are conflicting (see Section 3).

Conflicting empirical results can be statistically evaluated by meta-analysis (see Section 2). Meta-analysis could increase our knowledge about the nature of understandability – also to facilitate future metamodel design or modification. But, Section 3 shows that meta-analysis on metamodel understandability is currently hindered by (1) the heterogeneity of the conducted experiments and (2) insufficient reporting of the experimental results. This paper provides a framework to achieve comparability of experiments on metamodel understandability (see Section 4), which is a prerequisite for meta-analysis. Appropriate reporting guidelines exist (e.g., [16], [19]).

2 Meta-Analysis

Meta-Analysis is the statistical evaluation of numerical results that have been obtained by other (primary) studies [25], [13]. Hence, it is a kind of secondary research that aims at (1) finding evidence for some investigated phenomenon beyond individual studies (by calculating general descriptive statistics), (2) explaining conflicting results (by discovering new influencing variables), and (3) removing the bias potentially contained in 'normal' literature reviews of empirical studies [27].

Literature reviews often concentrate only on significant results that support the reviewer's theoretical position. But, statistical significance can be misleading, because it is affected by sample size [9]: If the same experiment is conducted independently, a larger sample may yield a statistically significant result, while a smaller one does not. The 'empirical truth' can be revealed by *effect size. Effect size* expresses the magnitude of a result, independently of sample size [9]. Table 1 summarizes main effect size measures and defines what constitutes a small, medium or large effect.

Table 1. Measuring Effect Size

Effect size measure	**Statistical test procedure**	**Reference**	**Effect**		
			Small	**Medium**	**Large**
$d = \frac{\mu_1 - \mu_2}{\sigma}$ or $r_{es,t} = \sqrt{\frac{t^2}{t^2 + df}}$	t-test*	[10]: d	0.2	0.5	0.8
		[10]: r	0.1	0.3	0.5
$\omega = \sqrt{\frac{\chi^2}{N}}$	χ^2-test	[10]	0.1	0.3	0.5
$\eta^2 = \frac{\sigma_\mu^2}{\sigma^2}$	F-test (ANOVA)	[10], [9]	0.01	0.06	0.14
$r_{es,z} = \frac{\lvert z \rvert}{\sqrt{N}}$	U-test or any other that yields a z-score♦	[28]	0.1	0.3	0.5

μ_1, μ_2: Group means, σ: Standard deviation. σ^2 (σ_μ^2): Total (Between group means) variance, *t*, χ^2 (z): Test statistics (z: normal distribution), *N*: Total number of participants ($N = \Sigma n_i$), *df*: Degrees of freedom (df = n-2; n: [constant] number of participants of each group)

∗ Between-subjects design: σ of either group, within-subjects design: adjusted σ [9].
♦ Requires N ≥ 25 to calculate the z-scores by assuming normal distribution [10].

Meta-analysis typically integrates the effect sizes of singular studies. The *basic steps* are as follows [27], [25]:

1. Define the independent and the dependent variables of interest.
2. Systematically collect the studies to be included in the meta-analysis.
3. Estimate effect sizes for each study.
4. Combine the individual effect sizes to calculate and test the central tendency (e.g., the mean or median) and dispersion (e.g., variance) of the overall effect.

Various ways of combining effect sizes exist (see, e.g. [27]). The combined effect size quantifies the overall magnitude of some observed result, at least in the population of

the included studies. To yield useful results from meta-analysis, the included studies must satisfy the following *requirements* [25]:

[RQ1] They must be of the same type (e.g., controlled experiments or case studies).

[RQ2] They must test the same hypothesis. Since a statistical hypothesis assumes that the independent variable(s) will *cause* the changes in the dependent variable(s) [28], these variables should be identical or comparable.

[RQ3] Often several measures for the same variable exist. Ideally, all included studies should use the same or comparable measures.

[RQ4] The studies should report effect sizes or provide at least statistics according to Table 1 or raw data to calculate the effect sizes.

The next section will show that current experiments on the understandability of metamodels do not satisfy these requirements.

3 Meta-Analysis of Research on Metamodel Understandability

This section sketches a failed attempt of meta-analysis – to prepare the ground for the framework in Section 4. The intended meta-analysis should find out whether certain (types of) metamodels have proven to be generally better understandable for human users. One of the earliest disputes relevant for this question took place in artificial intelligence by praising the merits of either predicate logic [12], which is usually written as text, or visual representations and diagrams [29]. This debate is excluded here from further investigation as it is based only on (quite suggestive) examples and, thus, differs in type from controlled experiments (see [RQ1] in Section2).

Table 2 lists some experiments examining the understandability of (types of) metamodels. The selection of the studies (deliberately) does not satisfy the requirements postulated in Section 2, as it is intended to point out the obstacles for meta-analysis:

The experiments differ in their independent variables and, thus, in the hypotheses (H_a)[1] investigated. Most independent variables are related to metamodels, but refer to *abstract*[2] syntax ([3]: H_a: metamodels with more constructs easier to understand), *concrete*[2] syntax ([8], [14]; H_a: graphical notation is easier to understand) or a *mixture* of both ([5], [6], [20], [22]). In the mixture case, the understandability of particular metamodels (the listed 'levels' in Table 2) is tested, whereas syntactically pure independent variables characterize types of metamodels. Besides the metamodel, also other factors influencing understandability are investigated, e.g., the complexity of the presented artifacts [14] and the knowledge of the participants [23].

The dependent variables are more homogeneous (correctness, time, perceived ease of use), but the particular measures vary. For example, correctness is quantified by the number of correct answers and by reviews. Additionally, diverse experimental designs have been used. Experimental design, i.e., the way participants are selected and assigned to experimental conditions [26], is discussed in Section 4.2

None of the studies in Table 2 reported effect sizes. [3], [6], [8], [20] and [22] provide at least enough aggregated data to calculate the effect sizes ex post according

[1] H_a denotes the alternative hypothesis, which is given in an aggregated and simplified form.

[2] Abstract (concrete) syntax mean the constructs and their allowed connections (notation).

to Table 1. The effects are small [6], medium [3] or large [6], [8], [20], [22]; see Table 2. But, because of the heterogeneous variables and hypotheses, a methodologically sound meta-analysis cannot be conducted.

Meta-analysis of understandability would be facilitated by some guideline for the planning, conducting and reporting of the underlying experiments. The following groups of guidelines have been proposed:

1. *General guidelines* on experimental research in software engineering (e.g., [4], [19]) with 'best practices' for planning, conducting, evaluating and reporting any kind of experiment. They do not help researchers in selecting variables and experimental designs to investigate understandability.
2. *Guidelines on reporting* the results of experiments e.g., [19], [16]. Though the latter ones have recently been criticized [18], they provide a solid foundation for the prospective availability of data needed to calculate effect sizes.
3. *Guidelines* for experiments in the field of *conceptual modeling*, e.g. 23], [2], [11] or management information systems (MIS) research [15]: These guidelines cover specific aspects of metamodel understandability (e.g., the role of domain knowledge) [23], remain vague sets of hints without well-founded recommendations of variables or experimental designs [2] or aim at classifying existing experimental studies [11]. As a consequence, the classification guideline [11] concentrates on variables that have been used in experiments on metamodel understandability, but neglects potential variables known from cognitive psychology, which is the major field for scientific investigations of understandability. Meta-analytic comparability, however, requires the consideration of all known factors affecting some phenomenon. Experimental design is only discussed in the MIS research framework [15]. Because of focusing on the usage of MIS, 'metamodel' is not considered as an independent variable. Corresponding modifications of the framework have been proposed [6], but remain at the surface. Additionally, the MIS research framework differs in terminology and methodology form empirical software engineering.

To sum it up, owing to heterogeneous experiments and deficient reporting of the experimental results, meta-analysis of metamodel understandability is currently not possible. Appropriate reporting guidelines exist. The next section proposes a framework that is to increase the comparability of experiments on metamodel understandability, which is a prerequisite for meta-analysis.

4 A Framework for Comparable Experiments on Metamodel Understandability

4.1 Affecting Factors

An *experiment* is a scientific investigation in which one or more independent variables (IV) are systematically manipulated to observe their effects on one or more dependent variables (DV) [28]. The outcome of an experiment depends on the *affecting factors* [11]. This term comprises both *independent variables* whose (causal) relationship to the dependent variables is examined and other factors (*extraneous variables, EV*) that confound the causal results [28]. Whether some affecting factor

Table 2. Experiments on the Understandability of Metamodels

Ref.	Independent Variables (Levels)	Tasks (Number)	Dependent Variables	Experim. Design	N (T)	Statistical Procedure	Results	Effect
[5]	Data model (EER, RDM)	Spec (1 case)	CO (review), PEU	2 groups, matched in experience	42 (M)	t-test of means	EER leads to higher correctness; no difference in perceived ease of use	not applicable
[6]	Conceptual data model (EER, KOOM)	Spec (1 case)	CO (review)	2 groups	38 (–)	matched-pairs t-test for means	Mostly no differences in correctness; higher correctness of EER only for some facets	d = 0.04 to d = 2.12
[8]	Graphical query languages	Comp (32), Spec (14)	CO (review)	1 group	27 (U)	χ^2-test on distribution	Graphical queries are easy to comprehend, not easy to specify	$\omega = 0.61$
[14]	Datebase representation (graphical, textual), complexity	Spec (20)	CO (review), ST, PEU	2 x 2 factorial	36 (M)	ANOVA	Graphical representations are faster, lead to higher correctness and higher perceived ease of use.	not applicable
[20]	Conceptual data models (EER, SOM, ORM, OMT)	Spec (2 cases)	CO (review), MT, PEU	4 groups	100 (–)	Duncan test	Increased correctness and faster solutions for EER and OMT	$\eta^2 = 0.14$
[22]	Conceptual models (DSD, ERM, OOM)	Comp (30)	CO (answers), ST	3 groups	121 (M)	ANOVA, correlation analysis	Highest correctness for OOM; faster for OOM, followed by DSD, ERM	$\eta^2 = 0.15$
[3]	Conceptual data models (varying construct number)	Comp (40)	a) CO (answers), b) inverse of time, c) learnability	2 groups	64 (M)	t-test of means difference	Models with more constructs lead to more accurate conceptualization, increase the time to process a schema, are faster to learn	r = 0.41
[23]	Conceptual data models (ER, EER), knowledge	Comp (36)	CO (answers and review)	2 x 2 factorial	81 (U)	paired t-test of means	IS knowledge affects problem solving; domain knowledge is helpful in solving demanding tasks	not applicable

Abbreviations: CO: Correctness, Comp: Comprehension, DSD: Data Structure Diagram, EER: Extended Entity-Relationship Model, (K)OOM: (Kroenkes) Object Oriented Model, MT: Modeling Time, N: Total number of participants, PEU: Perceived ease of use, SOM: Semantic Object Model, Spec: Specification, ST: solution Time, ORM: Object Role Model, OMT: Object Modeling Technique, RDM: Relational Data Model, T: Type of participants

constitutes an independent or an extraneous variable is, to some extent, a matter of the researcher's decision (contingent on the research question, the availability of participants, costs etc.). This decision requires knowledge on (at most) all the factors that affect the outcomes of an experiment. For experiments on understandability in computer science, this knowledge is provided by Fig. 1.

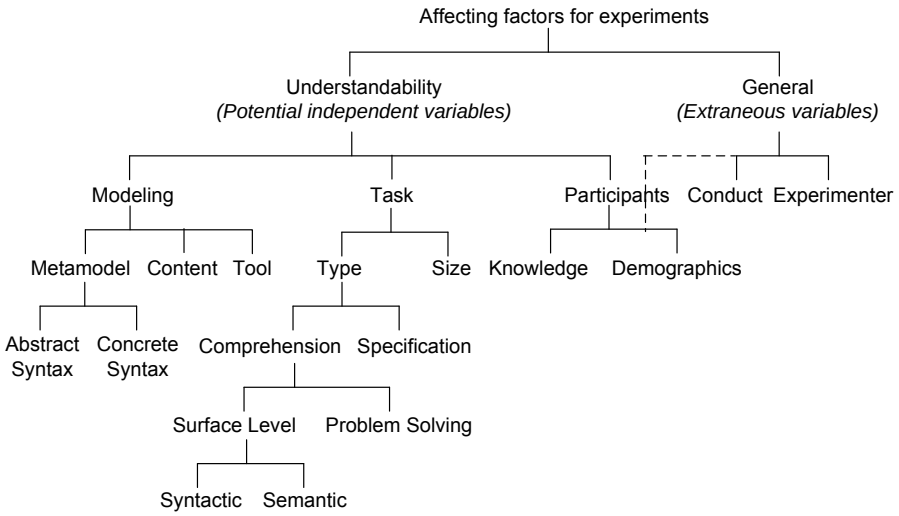

Fig. 1: Affecting factors in experiments on metamodel understandability.

It can be distinguished between factors that affect the outcome of any experiment (*general* affecting factors) and factors with a known influence on understandability; see Fig. 1. In the field of behavioral sciences (to which cognitive psychology belongs), the following *general affecting factors* are acknowledged:

- The *conduct* of the experiment, comprising:
  - The *experimental situation*, namely the location (noise, room temperature), the time of day and the equipment (failures, calibration) [9].
  - *Position effects*: Performance depends on the timely distance of a task from the start of the experiment (e.g., fatigue, getting bored, learning) [21].
  - *Carry-over effect*: The performance achieved in some task depends on whether or not some other task has been done before [28].
- The *experimenter*: His/her ability to instruct participants; his/her *bias* (expecting a particular outcome can distort the experimenter's behavior or data gathering) [26].

These general affecting factors are not causally related to the dependent variables, but distort the experimental results and, thus, are extraneous variables. In contrast, in investigating metamodel understandability, the following affecting factors – related to modeling, participants and task - are potential independent variables (see Fig. 1):

Both the metamodel's *abstract syntax* (e.g., the number [3] or type [7] of constructs) and its *concrete syntax* (graphical vs. textual notation; e.g., [14]) affect understandability. Metamodels cannot be tested in isolation, but only by applying them to some *content*. The content should be 'informationally equivalent' [23], i.e., it must be

possible to model this content by any of the investigated metamodels, and the content should be comparably difficult. Finally, the *tool* used to create or present models (e.g., its navigation or dynamic layout capabilities) influences understandability.

Among the affecting factors, *participants* play an intermediate role: Their *demographic* characteristics (e.g., age, gender) affect any experiment [1] and, thus, also understandability. For example, the participants' age is treated as an independent variable in MIS research [15]. *Knowledge* comprises experience and skills related to domain and metamodel as well as general mental abilities. Domain knowledge distorts results on metamodel understandability as it enables inferences [23]. Metamodel knowledge is usually provided in preparing the participants for the experiment.

Tasks in experiments on understandability can be characterized by their type and size. As Table 2 indicates, the task *types* used are comprehension or specification (defined in Section 1), which agree to the dependent variables cognitive psychology suggests (see Section 4.3). Comprehension tasks can be subdivided into surface-level understanding and problem-solving tasks [17]. In *problem-solving tasks,* participants are requested to determine whether and how certain information can be retrieved from an artifact created by applying the metamodel. In contrast, *syntactic* surface level understanding tasks refer to the constructs of the metamodel and their relationships (e.g., 'How many attributes describe the entity type ORDER?'), whereas *semantic* tasks assess the understanding of the *contents* described (e.g., 'Every employee has (a) a unique employee number, (b) more than one employee number.') [17]. An influence of the size of some task, e.g., the complexity of the database described by some metamodel, is generally assumed, but it was only marginally significant in [14].

Depending on the decision of the researcher, a potential independent variable is either systematically manipulated or becomes an extraneous variable. Extraneous variables decrease the *internal validity* of experiments, i.e., the degree to which the variation of the dependent variables can be attributed to the independent variables (rather than to some other factor) [28]. Consequently, extraneous variables must be controlled, which is main constituent of experimental design (see Section 4.2).

4.2 Experimental Design

An *experimental design* can be regarded as a general plan for (types of) experiments that joins independent variables and control techniques for extraneous variables. The main *control techniques* are removing, constancy and randomization [26], [28]; they should be applied in the following order:

1. *Remove* the extraneous variable (EV), especially if it is related to the experimental situation (e.g., use a quite room).
2. If the EV cannot be removed, its influence on the dependent variable is known and the sample is small, keep the EV constant. *Constancy* guarantees that all conditions are identical except for the manipulation of the independent variable, but reduces the *external validity* of the experiment, i.e., its generalizability [26].
3. If sample size does not matter and the influence of some irremovable EV on the dependent variable is not surely known (e.g., gender), must be neutralized (e.g., position or carry-over effects) or should be equated (e.g., age, knowledge), *randomize* the EV. Randomization increases the external validity of experiments.

Table 3. Summary of Experimental Designs

Design	**Between-subjects**	**Within-subjects**	**Block (Matched)**	**Factorial**
No. of IV (levels)	1 (n)	1 (n)	1 (n)	m > 1 (n)
Groups	n	1	n	m × n
Pro:	No carry-over effects	• Simple • Small samples • Constancy of individual characteristics	• Precise • No carry-over effects • Individual differences balanced	Interactions between IV can be examined
Contra:	• Unequal groups possible • Large samples	• Carry-over effects • Experimenter bias	• Effort • Matching factor must exist	• Large samples • Difficult to interpret for m > 3
EV Control	Randomization	Constancy	Constancy and Randomization	Randomization
Statistical test procedures				
Metric DV	♦: independent t ∗: F-test, ANOVA	♦: paired t-test of means ∗: MANOVA		MANOVA
Ordinal DV	♦: Mann-Whitney U ∗: Kruskal-Wallis H	♦: Wilcoxon signed rank test (matched) ∗: Friedman's χ^2		-
Nominal DV	♦/∗: χ^2 contingency test	♦: Sign test, McNemar's test of change ∗: Cochran's Q-test		-
Sample Size ♣	1-t: n_i = 20 [50] 2-t: n_i = 25 [60]	1-t: N = 11 [23] 2-t: N = 15 [35]	see between-subjects	2-t only, m = 3: n_i = 20 [50]

♣ To detect a large [a medium] effect (see Table 1) with $(1-\beta) = 0.8$ and $\alpha = 0.05$.

The *experimental design* to be chosen depends on (1) the number of independent variables and (2) the control technique. Table 3 summarizes typical experimental designs and their (dis-) advantages (for details, see [9], [26], [28]). Experimental design and the dependent variables determine the statistical test procedures for evaluation (see Table 3). For each statistical procedure, an effect size measure exists (see Table 1). The sample size required to detect a small, medium or large effect for a given experimental design and statistical test procedure can be calculated by power analysis (e.g., [9], [10]); the resulting recommendations are given in Table 3.

4.3 Affected Factors

The *dependent variable* is the one on which the effect of the independent variable is measured. Behaviorism, the origin of experimental research in psychology, requires the dependent variable to refer to observable behavior [1]. Thus, 'perceived ease of use' (even though applied, see Table 2) is not an acceptable dependent variable. Instead, the following measures of behavior are common [26]:

1. *Frequency*, e.g., the number of correct answers or solved problems.
2. *Selection*, e.g., which of several answers is chosen.

3. *Response* latency (or response time), which is concerned with how long it takes for a behavior to be emitted, e.g., how quickly a participant reacts.
4. *Response duration*, i.e., the length of time some behavior occurs (e.g., how long a participant deals with a task).
5. *Amplitude*, measuring the strength of response.

The dependent variables in experiments on metamodel understandability (see Table 2) use these measures as follows: Solution time refers to response latency and modeling time to response duration. If correctness is verified by multiple-choice questions (e.g., [17]), it is based on the measure 'selection', whereas numbers of correct answers are a measure of frequency.

Thus, the dependent variables in experiments on understandability in computer science are well-grounded in cognitive psychology. Completeness could be achieved by measuring amplitude, which, however, is mainly common in neuroscience [1], and by using selection of some metamodel from a list in specification tasks.

5 Conclusion

Missing comparability of the integrated studies is a major reservation about meta-analysis [27]. But, comparability of heterogeneous experiments can be achieved by methodologically equalizing differences among experiments [13] – provided that the differences are known. In other words, sound meta-analysis is possible if all variables and (for EV) their control techniques are reported. The taxonomies provided by the framework (see Section 4) help researchers to compile such lists; further advances can be achieved by web-publishing them (and the related experimental studies) as well as by tool support for the experiments on understandability. A simple open-source tool called `notate` already exists (http://sourceforge.net/projects/notate). It has been successfully applied in experiments on understandability [24] and can be extended to cover the complete framework of Section 4.

In contrast to the narrow view of MIS research, extensibility and flexibility are major requirements for a framework to investigate understandability in computer science, since the nature of language understanding in general still is an open research question in cognitive psychology [1]. Workshops are an appropriate place to exchange experience in this field and to advance the framework proposed here.

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Empirical comparison of two class model normalization techniques *Obstacles and questions* ⋆

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Abstract. Designing accurate models is a true challenge for model driven engineering approach. We are currently exploring techniques derived from Formal Concept Analysis (FCA) theory for finding possible class, association, attribute or method generalizations in models with the aim of improving their abstraction level. Using four models, we compare classical FCA approach to Relational Concept Analysis (RCA) which allows to discover more subtle generalizations. Interestingly, expected combinatorial explosion does occur in all cases when using RCA, making it a feasible solution in a special range of models. The study highlights several difficulties, including the need for costly and subjective human intervention in assessing or filtering the results.

1 Introduction

We are involved for 11 years in several projects with an industrial partner, France Télécom R&D, dealing with the general problem of assessing and improving the quality of the abstraction level of a class model. By class model we refer to UML structural class models, Ecore models as well as Java or C++ programs. A small part of our work is dedicated to abstraction metrics [1] while the main effort is put on developing theory [2,3], methodology [4,5], algorithms [6] and software tools [4,5] for improving abstraction level. We are currently involved in a project which is concerned with Model Driven Engineering (MDE) approach in two ways: we use the MDE spirit and technologies for developing a generic tool, based on data input/output metamodels and on a configuration metamodel; the purpose of the tool, which is improving abstraction level of models, deals with the core material of MDE, namely models. Our approach is based on Formal Concept Analysis (FCA) and a derived data analysis method called Relational Concept Analysis (RCA). RCA helps discovering more accurate generalizations in models, unattainable by FCA, but the counterpart is that many non relevant generalizations can be found at the same time. In this paper, we study four models, mining generalizations using classical FCA as well as RCA. Results show a

⋆ France Télécom R&D has supported this work (CPRE 5326).

combinatorial explosion for RCA applied to the two Ecore models, but a reasonable result size for Java models. In the last case, the gain in obtaining relevant, more subtle, generalizations is not ruined by the necessity of mining these interesting generalizations into a huge amount of artifacts. In Section 2, we describe the research problem and the studied solutions. Section 3 presents the empirical comparison which was conducted, as well as the difficulties encountered. We conclude by a discussion in Section 4.

2 Clustering techniques for Class Model Normalization

Problem description One effect of the lack of abstraction in class models is the introduction of duplicated elements (e.g. attributes, parts of methods). This occurs because of the iterative way of building software and its constant evolution. Table 1 gives an insight of the number of duplicated attributes names (identifiers) in four class models that will be used in the case study. UML2 and Docbook are two metamodels designed with Ecore, Apache Common Collections (ACC) and Minjava are written in Java. Those name duplications do not necessarily imply redundant declarations since two attributes can have the same name and different meanings. However, it gives an indication on the actual number of duplicated attributes. More generally, we would like to improve the level of abstraction of

	Docbook	UML2	Minjava	ACC
#Classes	40	246	29	250
#Attributes	183	615	340	544
#Attrib. name duplications	161	319	63	373

For a given identifier `I` duplicated `n` times, we count `n` duplications (and not one).

Table 1. Attribute name (identifier) duplications in four class models

class models: adding generalizations of operations factoring out common code in their body, adding generalizations of attributes because of common or close name and compatible types (with a common semantically close super-type) or adding generalizations of associations in UML because their ends and part of their description can be generalized. As a consequence of these generalizations, new classes are introduced, and they often highlight new abstract concepts useful in next steps of evolution, for reusing and easy maintenance. Among existing proposals for finding generalizations, we study more precisely derived solutions from Formal Concept Analysis field.

Studied solutions Formal Concept Analysis [7] is a clustering method that classifies a set of entities described by attributes. More formally, let $K = (E, A, R)$ be a formal context. E is a set of entities, A is a set of attributes and R the `own` relation, with $R \subseteq E \times A$. A sample formal context is shown at the right of Figure 1.

In this context, entities (UML classes) are the rows and attributes (UML properties) are the columns. A *concept* is a set of entities that share several attributes. It can be considered as an abstraction of these entities. More formally, a concept is a pair (X, Y) with $X \subseteq E$, $Y \subseteq A$ and $X = \{e \in E | \forall y \in Y, (e, y) \in R\}$ is the extent (covered entities), $Y = \{a \in A | \forall x \in X, (x, a) \in R\}$ is the intent (shared attributes). These definitions ensure maximal factorization of attributes, and in the context of class model, avoid property and method duplications.

The concepts can be organized in a specialization lattice: a concept c_1 is lower than a concept c_2 if the intent of c_2 is included in the intent of c_1. The specialization lattice ensures, in the context of class model normalization, that inheritance or specialization links respect property/method sets inclusion and refinement. A sample lattice corresponding to the context of Figure 1 is shown at the left of Figure 2 (intents are the upper part of the labels, extents are the lower part).

Three steps are required to apply formal concept analysis on a class model. First, the class model is converted into a formal context (Figure 1), which encodes the ownership (by contrast with [8] approach that mainly encodes access in formal contexts). Second, a concept lattice is built, according to the formal

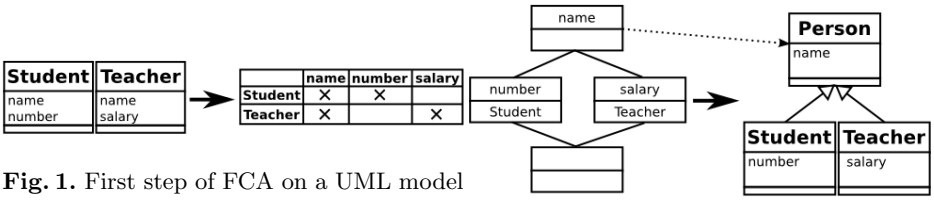

Fig. 1. First step of FCA on a UML model [9]

Fig. 2. Second step of FCA on a UML model

context. This concept lattice will contain concepts that represent the existing entities (and thus the classes) of the formal context, and new concepts that will lead to the creation of new classes. The last step (Figure 2-right) is to build a class model according to the concept lattice. It is clear that the output class model is normalized whereas the input class model was not. This normal form is called *attribute lattice factored form* in [10].

Formal Concept Analysis is powerful to distribute attributes in a class hierarchy, but is unable to deal with relational descriptions. As an example, let us consider the class model in the left of Figure 3. The same conversion and application of FCA on this model, as previously described, would lead to the creation of the model shown in the right of Figure 3. The resulting model, even if it is in normal form, could still be improved with a new property with type Person, introduced in the class Person, and redefined by the *friends* and *colleagues* properties.

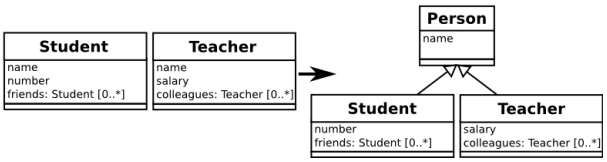

Fig. 3. Limitations of FCA on a UML model

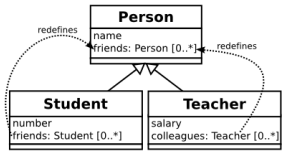

Fig. 4. RCA result on a UML model

Relational Concept Analysis [2,3] is an extension of FCA. It is designed to take into account entities described by attributes and by inter-entity links. In RCA, instead of having just one formal context, there is one formal context for each kind of entities. Then these formal contexts are filled out with other contexts that show relations between entities coming from one context and entities coming from another context (which can be the same). More formally, a Relational Context Family (RCF) is a pair $F = (K, L)$ where K is a set of formal contexts where $K_i = (E_i, A_i, R_i)$ and L a set of relational contexts, $L_i = (E_a, E_b, R_i)$ with $R_i \subseteq E_a \times E_b$. Figure 5 shows the relational context family corresponding to the class model at the left of Figure 3.

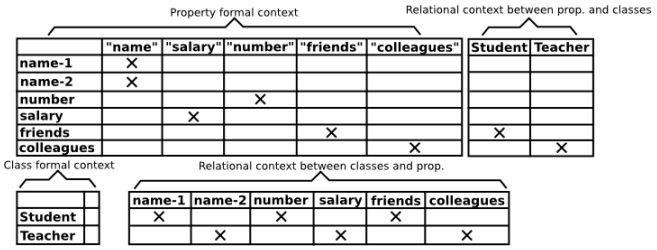

Fig. 5. Applying RCA on a UML model: produced contexts

An iterative lattice construction is applied on the relational context family. A concept lattice is built for each formal context K_i of the Relational Context Family. The discovered concepts of these lattices are injected as new entities in the RCF, and new lattices are built. This iterative construction stops whenever for each category of entities, the lattices built while performing two successive steps are isomorphic. The set of lattices produced after each step of the process is called a Concept Lattice Family (CLF). The class model in Figure 4 has been produced from the contexts of Figure 5.

3 An empirical comparison of Class Normalization techniques

In this section, we develop our view on an empirical study we began to carry out. We examine four facets: how a precise hypothesis we would like to check is formulated, how the experiment is prepared, how it is conducted and finally how results are evaluated.

3.1 Formulating hypothesis

As presented on small examples, FCA and RCA are very attractive techniques. From a theoretical point of view, it can be shown that RCA finds relevant generalizations (abstractions) that are not obtained by FCA. From a practical point of view, RCA is intrinsically much more combinatorial than FCA and it seems more difficult to fine-tune and control the huge set of produced generalizations. When the initial model contains long paths of associations, some inferred generalizations can have poor semantics and often mean something like (for a class) "class whose instances are linked to instances that are linked to instances that are linked .... to instances of" a given initial class of the model.

The question we would like to answer is formulated as follows: "Comparing generalizations produced by RCA versus those produced by FCA, and considering the effort needed for parameterizing and using results of FCA/RCA, is RCA an interesting improvement in practice?". This question is still too general, as several parameterizations are possible for FCA and RCA, depending of what we decide to encode in the tables (formal contexts for FCA and relational context family for RCA). This can be considered as part of the preparation of the experiment (reducing the hypothesis).

3.2 Preparation of the experiment

Preparing the experiment in our context involved choosing class models on which we could test, choosing some procedures to make the data usable or more relevant, choosing the part of models to consider because many elements can be abstracted, and choosing the gauges for evaluating results.

In previous experiments with industrial models from France Telecom R&D, data were confidential and it was impossible even for us to see them and we just could access to partial informations: partial examples of built abstractions and partial results. Now, after several years, these models are no more confidential but people who designed the models are no more in charge of the projects and have no time to devote to new experiments. The problem we face here is the obsolescence of data.

Choosing data To evaluate our class model normalization approach, we carried out an experiment on four open source class models. Two of them, UML [11] and Docbook [12], are design models written in Ecore. The two others, Apache Commons Collections (ACC) [13] and Minjava [14] (author: J.R. Falleri), are implementation models, obtained by reverse-engineering on Java code. UML stands for the UML 2.0 meta-model. Docbook is a meta-model of the Docbook

language. Apache Commons Collections is a Java library that extends the Java collections. Minjava is a Java reverse engineering tool that analyses Java bytecode and produces an Ecore compliant Java model conforming to a simple Java meta-model. Open source class models have the advantage of being available by everyone. Designers may change or may be too busy to discuss with experimentalists about the models, but at last, in industrial context this is often the same situation.

Choosing configurations We restricted the experiment, for a first study, to parts of models composed of classes and attributes (properties in UML terms):

1. Basic FCA configuration (*FCA1*): it corresponds to the one in [10], that generates a class and a property context and analyses the attribute ownership to discover super-classes, based on attribute names.
2. Enhanced FCA configuration (*FCA2*): same as the previous configuration, but using information specific to the input language (*e.g.* static keyword in Java, cardinality in Ecore) to avoid incorrect generalizations.
3. Enhanced Properties configuration (*RCA*): a RCA configuration that generates a class and a property context and analyses the attribute ownership and the attribute type to discover super-classes and redefined properties.

Choosing gauges To understand the results of the application of FCA (resp. RCA) to our sample models, we use the produced lattice (resp. Concept Lattices Family). We classify the concepts of these lattices into three disjoint categories. $ExistingConcepts$: for elements that were already present in the input class model; $NewConcepts$: for elements created during the RCA process; $MergeConcepts$: for the merge of existing elements from the input model.

The $ExistingConcepts$ set is not really interesting since it contains only concepts representing the input entities. The $NewConcepts$ set is very interesting. It contains the concepts that may introduce new useful elements (abstractions of existing ones) in the class model. The $MergeConcepts$ set is also interesting, since it contains the elements from the source model that have been considered similar and therefore have led to the creation of the new elements. To present the result of our case study, we choose to use the two following quantities: N, the number of new elements *i.e.* $|NewConcepts|$; M, the number of merges *i.e.* $|MergeConcepts|$.

Of course, the previous quantities show how the different configurations of the RCA process behave, but are unable to show the quality of these results. Metrics are a way of assessing quality, but they are not so easy to use: based on current inheritance metrics from [15,16], it has been shown in [17] that inheritance metrics (associated with size metrics) are useful in measuring software stability, but don't really help in detecting concrete design problems.

In [18], the case study uses a structural metric to analyze the result of FCA application on real world class hierarchies. The chosen metric, called *M2* is derived of the *M1* metric introduced in [19]. This metric measures redundancy and inheritance quality. Basically, *M2* is a weighted sum of the number of attributes

and the number of inheritance links. To defavor the use of multiple inheritance, for a given class the inheritance links count as double after the first one. The lower metric *M2* is, the better the class model is designed.

Unfortunately, this metric can lead to wrong analysis of the class model. If we imagine an output class model where a super-class has been found but is not correct (for instance because of homographs), the *M2* metric will still consider this output model as better than the input one. Moreover, this metric is not compatible with the use of redefined properties or methods. If we use the class model shown in Figure 3, the metric *M2* will be 24 for the input model, 22 for the output model without attribute redefinition and 26 for the output class model with attribute redefinition. This clearly shows that this metric is unable to correctly measure the quality of a class model design when attribute redefinition is used.

Results from FCA/RCA on class model could be assessed using recent proposals and results on specialization quality measurement [1,20]. But in this first experiment, four simple, specific metrics have been introduced based on human analysis:. *cn*: number of concepts included in the *NewConcepts* set that are considered as correct; a rate is obtained with $cnr = cn/|NewConcepts|$; *cm*: number of concepts included in the *MergeConcepts* set that are considered as correct; a rate is obtained with $cmr = cm/|MergeConcepts|$.

3.3 Experimenting

During Java model building, we faced to the presence of the standard Java API library and had to decide whether looking for abstraction in the model combined with the Java API, or inside the model only. When building our sample models, we finally chose to restrict the extraction of Java entities to the program itself, and blocked the extraction of the Java standard library (except base types). So when a Java class introduces an attribute typed by a class included in the standard Java API (for instance a *LinkedList*), the attribute appears as not typed in the resulting Java model. The output of this phase is the set of results, in the expected form if no problems for measuring have been found. For our experiments, results are presented in Figures 6, 7, 8, 9 and Tables 10, 11.

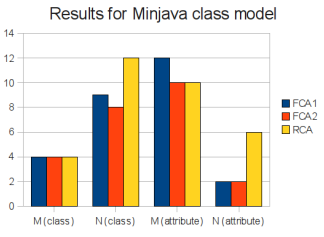

Fig. 6. Results for Minjava class model

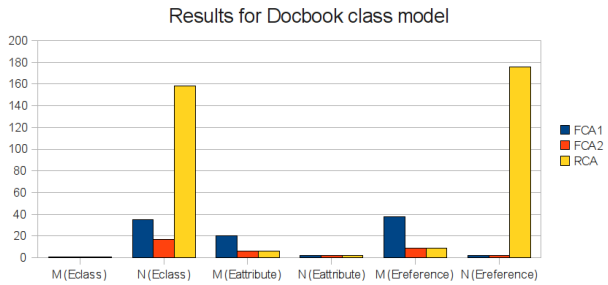

Fig. 7. Results for Docbook class model

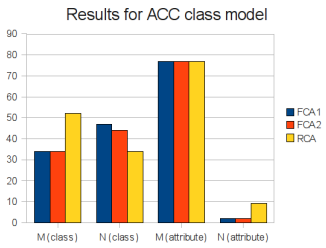

Fig. 8. Results for ACC class model

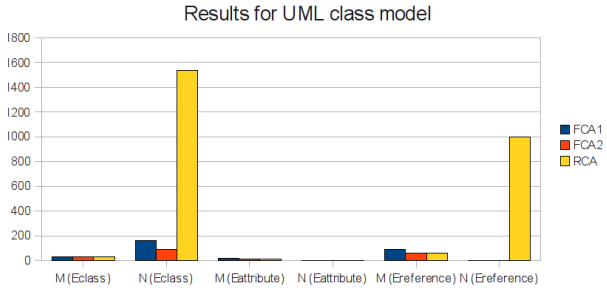

Fig. 9. Results for UML class model

	FCA1	FCA2	RCA
$\|MergeConcepts\|$	12	10	10
cm	10	10	10
cmr	0.83	1	1
$\|NewConcepts\|$	2	2	6
cn	0	0	0
cnr	0	0	0

Fig. 10. Attribute results for Minjava

	FCA1	FCA2	RCA
$\|MergeConcepts\|$	4	4	4
cm	3	3	3
cmr	0.75	0.75	0.75
$\|NewConcepts\|$	9	8	12
cn	5	5	5
cnr	0.56	0.63	0.42

Fig. 11. Class results for Minjava

3.4 Evaluating

Quantitative results shown in Figures 6, 7, 8, 9 are not sufficient to conclude on quality and relevancy of FCA/RCA, but they confirm that in some data (Java software here: ACC, MinJava) feasibility of FCA and RCA is assessed, combinatorial explosion does not occurs and results can be manually explored; in other cases (Ecore models: Docbook, UML2) the number of abstractions created by FCA remains reasonable, while abstractions created by RCA explodes, *e.g.* 1534 new classes are created by RCA for the UML2 metamodel using 246 initial classes and 615 initial properties. This last result is rather depressing and highlights the need for adapted filters. Because we face to the problem of the gold-seeker: looking for a nugget in a heap of uninteresting rocks.

Results of cn and cm metrics on Minjava are shown in Tables 10 and 11. Concept correction has been assessed by the designer of Minjava. These results confirm what was expected from the quantitative results. *FCA1* is the configuration that produces most incorrect merges and *RCA* produces most incorrect new concepts. On the other hand, new concepts produced by *RCA* could not have been created using a FCA configuration and can contain useful concepts. However it is necessary to find a way to analyse those new concepts in a semi-automatic way, because they are too numerous to be analysed by hand.

4 Discussion

We have shown several facets of an on-going empirical study about FCA and RCA techniques. Now we would like to open the discussion, based on the lessons learned during this first approach and the raised questions.

Place of humans is crucial: to decide which data are used, to evaluate quality of results. For three models we have only quantitative results which, at the end, only give indications of the number of built abstractions. For the (small-size) MinJava model, we have an evaluation which takes into account the judgment of one of us who is the designer of MinJava. This allows us to give a *precision* measure as it is commonly used in the information retrieval field: the fraction of the relevant retrieved abstractions among the retrieved abstractions. This interesting measure is difficult to compute for large models and when we do not have good knowledge of their semantics. It is also a subjective question. A second measure coming from information retrieval field is the *recall* measure which gives the fraction of the relevant retrieved abstractions among the relevant abstractions. This is even more difficult to obtain because finding all relevant abstractions in a model needs many different designers to have a kind of consensus.

Reproducibility of the study is always a challenge: in our case, it is ensured by the use of open source software MinJava for Java models, Ecore tools for Ecore models, the declarative configuration of which entities are taken into account, in available files thanks to a Model Driven Engineering approach [9].

Concerning evaluation, besides precision and recall measure, we could use comparisons between the number of duplications (Fig. 1) and the number of added abstractions. But, beyond these technical considerations, we are aware of the fact that we have (partially) evaluated a first level of quality. A more difficult, second level, of evaluation would consist of measuring: The effort achieved to build the abstractions and (manually) filtering them; What we gained in terms of software quality (readability, extensibility, maintainability).

To conclude, what could help us in our quest:

- sharing problems: organizing challenges like other domains do as the KDD Cup (http://www.sigkdd.org/kddcup/); This could guarantee relevancy of problems themselves;
- sharing data: stable benchmarks, different versions of models; an example of that in MDE domain is the zoo of metamodels (http://www.eclipse.org/gmt/am3/zoos/atlantEcoreZoo/); simulation (randomly generated data) can also be a solution in some special cases but is difficult to control; What is still missing is a benchmark of models, because in our experiment we had to build some of them by transforming source code, thus adding an interpretation step;
- sharing methodologies: Using metrics, using human skills, defining filtering techniques to restrict evaluation in relevant zones, etc.
- sharing results: repositories of results for given benchmarks to confront experiences; making possible the publications of negative results to avoid bias and begin again unuseful experiences.

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Assessing the Power of A Visual Notation - Preliminary Contemplations on Designing a Test -

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Abstract. This paper reports on preliminary thoughts which have been conducted in designing an empirical experiment to assess the comprehensibility of a visual notation in comparison to a textual notation. The paper sketches shortly how a corresponding hypothesis could be developed. Furthermore, it presents several recommendations that aim at the reduction of confounding effects. It is believed that these recommendations are applicable to other experiments in the domain of MDE, too. Finally, the paper reports on initial experiences that have been made while formulating test questions.

1 Introduction

Although modeling does not imply visualization, people often consider the visual representation of models to be a key characteristic of modeling. One reason to this could be that modeling techniques such as State Machines or Petri-Nets are often taught and explained with help of circles and arrows rather than in terms of mathematical sets and functions. Apart from that, other kinds of modeling, e.g. data modeling with help of Entity-Relationship-Diagrams, make heavy use of visual representations, although the same concepts could be specified in a purely textual manner, too.

However, let alone the impression that visual representations are considered very appealing by a broad range of developers, customers, maintainers, students, etc., a scientific question would be if visual representations actual yield any extra benefit to software development, maintenance, or teaching, etc.

Driven by a personal belief of the authors that this extra benefit exists, this paper reports on preliminary thoughts which have been conducted in designing an empirical experiment. The goal of this empirical experiment is to assess (such a "soft" property as) the "comprehensibility" of a visual notation in comparison to a textual notation.

This paper does not formulate a concrete hypothesis. Instead, it conducts general contemplation about hypotheses that are concerned with the evaluation of "comprehensibility". In particular, the paper presents several recommendations that aim at the reduction of confounding effects while running the test. It is suggested that these recommendations should be obeyed in other experiments in the domain of MDE, too. Furthermore, the paper reports on experiences that have been made while formulating the test questions for a test on comprehensibility.

The paper is structured as follows: In section 2, the process to define a hypothesis is outlined. In sections 3 and 4, a couple of considerations are presented in order to

reduce confounding effects. In section 5, problems are presented which have been encountered while formulating test questions. Section 6 presents some related work. And section 7 concludes the paper.

2 Defining the Goal of the Experiment, and What to Measure?

When designing an controlled experiment, everything is subordinate to the overall assumption, or hypothesis, that is going to be tested. Usually, the development of the test will require to repeatedly reformulate (refine) the hypothesis since the initial hypothesis turned out not to be (as easily) testable (as presumed). (A possible reason for this could be, for example, that it is overly hard, or impossible, to reduce the impact of confounding effects or to find suitable questions; cf. sections 3, 4 and 5.)

2.1 Experiment Definition

A fist step could be to define the experiment in general. When comparing visual vs. textual notations, this could be done as follows (using the experiment definition template suggested by [10]):

The goal of the study is to analyze visual and textual program specifications (i.e. diagrams versus code), with the purpose of evaluating their effect on the "comprehensibility" of the information shown. The quality focus is the perception speed and completeness and correctness with which all relevant information is apprehended. The perspective is that of teachers and program managers, who would like to know the benefit that visual notations can bring to their work (i.e. teaching students in computer science or developing software). The context of the experiment is made up of artificial/sample code snippets and their corresponding diagrams (= objects) as well as undergraduate and graduate students (= subjects).

2.2 Hypothesis Formulation

According to [4], a scientific hypothesis meets the following three criteria:

- A hypothesis must be a "for-all" (or rather a "for-all-meeting-certain-criteria") statement. This means in particular that the hypothesis must be true for more than a singular entity or situation.
- A hypothesis must be (able to be reformulated as) a conditional clause (of the form "whenever A is true/false, this means that B is (also) true/false").
- A hypothesis must be falsifiable. That means that, in principle, it must be able to find an entity or situation in which the hypothesis is *not* true.

Furthermore, for practical reasons, [1, 5] suggest to base the hypothesis on observable data. That is, in the (possibly reformulated) conditional clause, the value of one observable data (called "the dependent variable") must be specified to depend on the value of one other observable data (called "the independent variable") in a consistent way. The hypothesis is falsified if at least one example can be found where this dependency is not satisfied.

A starting point to find a hypothesis for the experiment outlined in section 2.1 could be the following:

When investigating program specifications, a visual representation X (as compared to a textual representation Y) ***significantly facilitates*** *comprehension of information Z.*

Following the aforementioned criteria, the above hypothesis is a scientific hypothesis because it can be rephrased as "whenever a program specification is represented using a visual notation X, it is easier to comprehend (with respect to information Z) than an equivalent representation using a textual notation Y". In this statement, the possible values (treatments) of the independent variable (factor) are "visual/not visual" and the possible values of the dependent variable are "easier to comprehend/not easier to comprehend". The claimed dependency would be "visual → easier to comprehend". The statement could be falsified by showing that visual notation X is not easier to comprehend than textual notation Y (with respect to information Z).

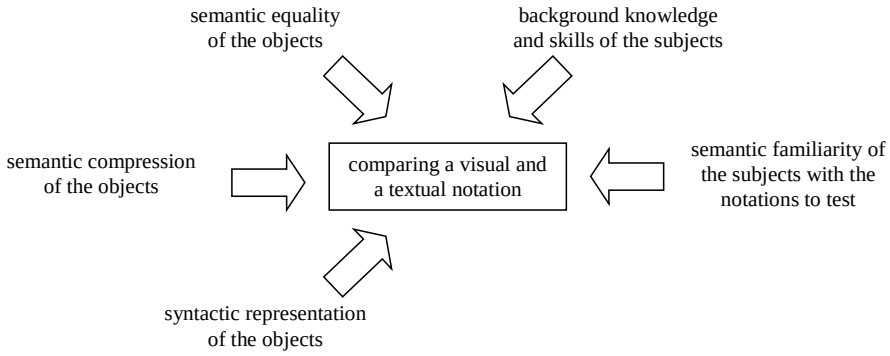


Fig. 1. Confounding impacts.

2.3 Variable Selection

Turning a the preliminary idea of a hypothesis into a testable hypothesis which is thoroughly rooted on objectively observable data is a challenging task in developing an empirical test. For example, since comprehensibility by itself is difficult to observe, another variable must be found whose values are considered to inherently depend on the level of comprehension of a tester. A commonly accepted variable measuring the level of comprehension, for example, is "correctness", i.e. the number of correct answers[1] given to a (test) questions (cf. [8, 7, 6]). However, as pointed out by [7], correctness is only one facet of comprehensibility. Another variable is "comprehension speed", e.g. the number of seconds that the subjects looked at the object (or maybe even "easy to remember", i.e. the number of times that the subjects

[1] if the correct answer consists of multiple elements, it could be some mean of precision and recall [2] (cf. [6]).

looked at the objects; cf. [7]). The inherent effect of the variable that is of interest on the variable that is measured must be substantially elucidated (and defended) in the discussion on the (construct) validity of the test.

The only factor (independent variable) in the experiment would be "kind of presentation" with the treatments (values) {visual, textual}.

One of the big challenges when investigating the casual dependencies between the (dependent and independent) variables is to reduce confounding impacts (see Fig. 1) as much as possible, and thus to maximize the validity of the experiment (cf. [10]). Otherwise, the "true" dependency could possibly be neutralized (at least, in parts), or might even be turned into its reciprocal direction (in the worst case).

In the following sections, some means are presented which should be taken in order to improve the validity of an experiment comparing a visual and a textual notation. The authors believe that these means are general enough to be applied to other evaluating experiments in the domain of MDE approaches, too.

3 Preparing Objects – Ensuring Construct Validity (I)

Construct validity refers "to the extent to which the experiment setting actually reflects the construct under study" [10]. In particular, this means to ensure that the objects of the experiment which are given to the subjects in order to perform the tests represent the cause well (i.e. a visual vs. a textual representation, in this case).

3.1 Semantic Equality

One obvious, yet carefully to ensure, requirement is to compare (visual and textual) representations that have equal semantics, only. It would be illegal and meaningless to compare any two representations with different semantics.

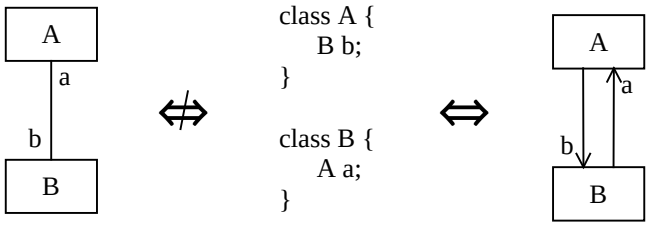


Fig. 2. Ensure semantic equality.

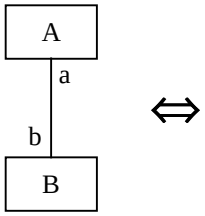
Fig. 2 shows an example. It would be illegal to compare the visual representation on the left with the textual representation in the middle since they mean different things. The bidirectional association between classes `A` and `B` in the UML model in the left of Fig. 2 denotes that two instances of class `A` and `B` are related to each other such that the instance of class `A` can navigate to the instance of class `B` via property `b`, while at the same time the instance of class `B` can navigate to the instance of class `A` via property `a` (meaning `a = a.b.a` is always true). The Java program code in the

middle of Fig. 2, however, does not imply that an instance of class `A` which is associated with an instance of class `B` (via its property `b`) is the same instance which that associated instance of class `B` can navigate to via its property `a` (meaning `a = a.b.a` does not need to be true).

Hence, in an empirical test comparing the performance[2] of visual vs. textual representations of associations, it would be more appropriate (in fact, obligatory) to compare the textual representation in the middle of Fig. 2 with the visual representation in the right of Fig. 2. Now, the semantic meaning of one notation is equally represented in the other notation, and comparing the results of their individual performance is valid[3].

3.2 Equal Degree of Compression

Apart from semantic equality, the expressions being compared need to be expressed at an equal degree of compression (here, the degree of compression shall refer to the degree with which semantic information is condensed into one language construct). Otherwise, "better" performance of one notation could be induced by the fact that one notation uses a "higher" compression (e.g. one language construct of that notation conveys the same semantic information than four language constructs of the other notation) rather than that it uses a "better" representation.



⇔

```
class A {
    B b;
    B getB() { return b; }
    void setB(B b) { this.b = b; b.a = this; }
}

class B {
    A a;
    A getA() { return a; }
    void setA(A a) { this.a = a; a.b = this; }
}
```

Fig. 3. Do not test expressions of unequal degree of compression.

Fig. 3 gives an example. Other than in Fig. 2, the Java code now contains extra lines which states that an instance of class `A` which is associated with an instance of class `B` (via its property `b`) must be the same instance to which that associated instance of class `B` can navigate via its property `a` (meaning `a = a.b.a` is always true). Hence, the Java expression in the right of Fig. 3 now equates to the semantics of the UML expression in the left of Fig. 3.

[2] in this paper, "performance" refers to "the notation's ability to be read and understood" rather than computation speed.

[3] note that asserting the semantic equality of two notations is not trivial. For example, there is no general agreement on how a UML class diagram should be transformed into Java code.

If – in a test – the UML expression should actually yield "better" results than the Java expression now, it is unclear (and highly disputable) whether the "better" performance is due to the visual representation or due to the higher degree of compression (i.e. the fact that we need to read and understand four method definitions in the Java code as compared to just one association in the UML diagram).

3.3 Presenting Objects

Apart from equal semantics and equal degree of compression, the expressions have to be appropriately formatted, each to its cleanest and clearest extent. This is because the authors estimate that disadvantageous formatting of expressions could have a negative impact on the test outcome, whereas advantageous formatting of expressions could improve the test results.

Fig. 4 gives an example. In the left part of Fig. 4, the Java code has been formatted in a way which is tedious to read. In the right part of Fig. 4, the UML representation has been formatted disadvantageously. With expressions formatted like this, it is assumed that the respective notation is condemned to fail in the performance test.

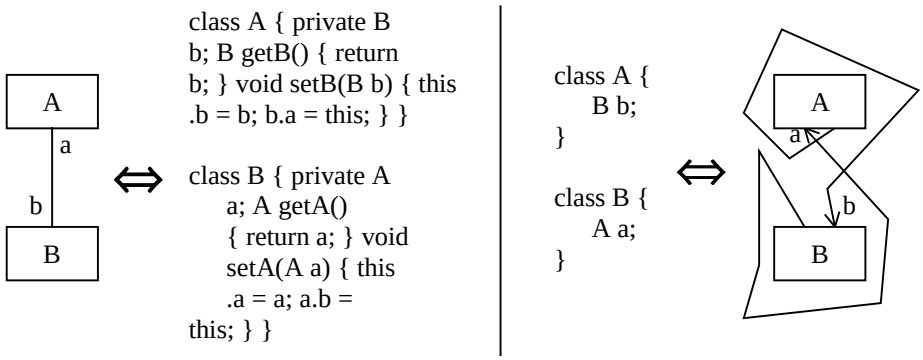


Fig. 4. Format expressions to their cleanest and clearest extent.

Unfortunately, there usually is no (known) optimal solution for the formatting task. Therefore, expressions should be formatted clearly and consistently following some strict and predefined guidelines (e.g. some formatting guidelines such as the [9]). It is important to keep in mind, though, that even though uniform guidelines are used to format the expressions, the effects of those formatting guidelines on the test outcomes are unclear. Moreover, the effects may even be different for each notation. Consequently, the (unknown) impact of formatting guidelines on the test results needs to be respected in the discussion of the (construct) validity of the test.

Likewise, syntactic sugar is to be avoided. That means, all means that are not related to the semantics of the underlying notation, such as syntax highlighting in textual expressions, or different text formats and different line widths in visual expressions, should not be used. Syntactic sugar (fonts, line width, colors, etc.) are likely to draw the attention of the testers to different parts of the expressions and thus may confound the pure comparison between their visual and textual representation.

Evaluating the impacts of formatting, fonts, line width, and colors on the comprehensibility of a notation is an interesting test of its own. However, that test should focus on the comparison of different style guidelines for *one* notation rather than on the comparison of (different) guidelines for different notations.

4 Preparing Subjects – Ensuring Internal Validity

To ensure internal validity, it must be ensured that a relationship between a treatment and an outcome results from a causal relationship between those two, rather than from a factor which has not been controlled or has not been measured (cf. [10]). In particular this means how to "treat", select, and distribute the subjects such that no coincidental unbalance exists between one group of testers and another.

4.1 Semantic Familiarity

The imperative necessity of comparing semantically equivalent "expressions" (see section 3.1) is complemented with the necessity that testers are equally trained in, and familiar with, both notations. Otherwise, i.e. if the testers of one notations are more experienced with their notation than the testers of the other notation with their notation, a "better" test result of the former notation could be induced by the fact that its testers have greater experience in using/reading it rather than by the fact that it is actually "better" (in whatsoever way). This is particularly probable whenever the performance of *new* notations shall be evaluated in contrast to *existing* ones.

One way to control the knowledge of the tested notations is to look for testers that are not familiar with both notations, and have them take a course in which they learn the notations to test. This approach seems particularly practicable in academia – even though the test results will usually assert the performance of "beginners", and thus make extrapolation to the performance of "advanced" software developers in industrial settings difficult (which does not mean that assessing the benefits of visual notations for "beginners" isn't worthwhile and interesting). This problem represents a threat to the external validity of the experiment (cf. [10]).

The goal of teaching the notations to novices is to ensure that the testers of each notation attain similar knowledge and skill with their notation. The challenge here is to defined what it means that testers are "equally familiar" (i.e. equally knowing and skilled) with their notations. It also needs to be investigated how the knowledge and skills of an individual tester with his/her notation can be actually assessed (so that we can decide afterwards whether or not "equal familiarity" has been reached). Another challenge is how "equal familiarity" can be achieved by a teaching course in a timely and didactically appropriate manner (e.g., what is to be done if a particular group of testers encounters unforeseen comprehension problems with their notation).

The knowledge and skill test could occur prior to the actual performance test, or intermingled with the performance test (in the latter case, some questions test the knowledge and the skills of the testers, while other questions test the performance of the notations). If the knowledge and skill test reveals that the semantic familiarity of the testers with their notation is extremely unbalanced (between the groups of testers), the test outcome must be considered meaningless.

5 Measuring Outcomes – Ensuring Construct Validity (II)

Once the hypothesis is sufficiently clear, the next challenging step is to formulate questions that are suitable to test the hypothesis and to find a test format that is suitable to poll the required data. This is another facet of construct validity, according to which the outcome of the test needs to represent the effects well (cf. [10]).

In this section, considerations and experiences are presented that have been made in designing a test evaluating the comprehensibility of a visual notation.

5.1 Test Format, and How to Measure?

Multiple Choice tests (when carefully designed; cf. [3]) are considered to be a good and reliable way to test the knowledge of a person, in particularly in comparison to simple True/False tests. Hence, Multiple Choice tests would have a higher construct validity with respect to the correctness of comprehension than True/False tests. A question format with free answer capabilities would be more realistic (and thus would increase the external validity of the experiment; cf. [10]). However, such short-answer test is much more laborious because it requires manual post-processing in order to detect typos and/or semantically equivalent answers.

When it comes to measuring the response time, it is important to discriminate between the time to find the answer in the expression and the time to understand the question. This is because if testers need 30 sec. to understand a question and then 10 sec. to find the answer in the textual expression and just 5 sec. to find the answer in the visual expression, it makes a difference whether 40 sec. are compared to 35 sec., or 10 sec. to 5 sec. Not to discriminate between the time to find an answer and the time to understand a question is only valid, if the ratio is reciprocal, i.e. if the time to understand a question is negligible short in comparison to the time to find the answer.

If the test outcome consists of more than one data, it is a big challenge to define how the outcomes can be combined in order to obtain a meaningful interpretation. In this case, for example, it needs to be decided how "correctness of answers" and "response time" can be combined to indicate a "level of comprehension". One option would be to disregard all incorrect answers, and consider the response time of correct answers, only.

5.2 Volatile (Time) Measurements – Problems of A First Test Run

Preliminary and repeated test runs of a test evaluating simple analysis of one textual expression[4] (with the same person) have shown that the measured time needed to answer the question (exclusive of the time needed to understand the question; cf. section 5.1) is rather short (in average ~10 sec.) and varies tremendously (3 sec. to 30+ sec., even for same or similar questions!). It seems as if the measured time is heavily confounded by some external factor (maybe slight losses of concentration). This is problematic because due to the short (average) response time, even the slightest disturbance (of about 1 sec.) could confound the measured (average) time significantly (e.g. by one tenth, in this case).

[4] in another case than association relationships

Another problem was to strictly discriminate between the time to find the answer in the expression and the time to understand the question (which, again, was essential due to the short (averaged) response time). The testers were required to explicitly flip to the expression once they have carefully read (and understood) the question (which was shown first). As it turned out, however, testers sometimes realized that they have not fully understood the question after they have already flipped to the expression. As a result, the measured response time was partly confounded.

It is currently being investigated how the problem of high variation in measurements can be tackled. One option would be to pose questions that are more difficult to answer, and thus takes more time. This will only work, though, if the confounding effects do not grow proportionally. Another option would be to repeat the test countless times (with the same person and similar questions) in order to get a more reliable average response time. A big problem of this approach is to ensure that the testers will not benefit from learning effects in the repeated tests.

A promising solution to properly discriminate between the time to find the answer in the expression and the time to understand the question has been found in [7].

6 Related Work

In 1977, Shneiderman et al. [8] have conducted a small empirical experiment that tested the capabilities of flow charts with respect to comprehensibility, error detection, and modification in comparison to pseudo-code. Their outcome was that – statistically – the benefits of flow charts was not significant. Shneiderman et al. did not measure time, though.

This was determined to be inevitable by Scanlan [7]. Scanlan formulated five hypotheses (e.g. "structured flow charts are faster to comprehend", "structured flow charts reduce misconceptions", to name just the two which are closely related to this paper). Scanlan's test design is very interesting: Scanlan separated comprehension (and response) time of the question from comprehension time of the expression. To do so, testers could *either* look at the question *or* look at the expression (an algorithm, in this case). This is an interesting solution for the aforementioned problem of separating comprehension time and response time (see section 5.1). Scalan's outcome was that structured flow charts are beneficial.

7 Conclusion

This paper has presented preliminary thoughts which have been conducted in designing an empirical experiment to assess the comprehensibility of visual notations in comparison to textual notations. The paper has discussed shortly how a corresponding hypothesis could be developed. Furthermore, it has presented several recommendations that aim at the reduction of disturbances in the measured data, which are considered to be helpful for other experiments in the domain of MDE, too. Finally, the paper has reported on initial experiences that have been made while formulating the test questions.

It needs to be emphasized that this paper presents preliminary considerations rather than sustainable outcomes. On the contrary, each of the presented contemplations could be subject of an empirical evaluation of itself (e.g. whether or not advantageous formatting really has an positive effect on comprehensibility). Also, decisions need to be made about how to execute the test (e.g. how textual and visual expressions are shown to the testers, if they can use zooming or layouting functions, etc.) . The authors plan to pursue the considerations presented here and, ultimately, come up with a test design. Getting there will require many (self-)tests before finally a test design will be found that is capable to assess the specified hypothesis reliably.

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Embedded System Construction – Evaluation of Model-Driven and Component-Based Development Approaches

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Abstract. Model-driven development has become an important engineering paradigm. It is said to have many advantages, such as reuse or quality improvement, over traditional approaches, even for embedded systems. Along a similar line of argumentation, component-based software engineering is advocated. In order to investigate these claims, the MARMOT method was applied to develop several variants of a small micro-controller-based automotive subsystem. Several key figures, like model size and development effort were measured and compared with two main-stream methods: the Unified Process and Agile Development. The analysis reveals that model-driven, component-oriented development performs well and leads to maintainable systems and a higher-than-normal reuse rate.

Keywords: Exploratory Study, Embedded, Model-Driven, Components

1 Introduction

Embedded software design is a difficult task due to the complexity of the problem domain and the constraints from the target environment. One specific technique that may, at first sight, seem difficult to apply for the embedded domain, is modeling and Model-Driven Development (MDD) with components. It is frequently used in other engineering domains as a way to solve problems at a higher level of abstraction, and to verify design decisions early. Component-oriented development envisions that new software can be created with less effort than in traditional approaches, simply by assembling existing parts. Although, the use of models and components for embedded software systems is still far from being industrial best practice. One reason might be, that the disciplines involved, mechanical-, electronic-, and software engineering, are not in sync, a fact which cannot be attributed to one of these fields alone. Engineers are struggling hard to master the pitfalls of modern, complex embedded systems. What is really lacking is a vehicle to transport the advances in software engineering and component technologies to the embedded world.

Software Reuse is currently a challenging area of research. One reason is that software quality and productivity are assumed to be greatly increased by maximizing the (re)use of

(part of) prior products instead of repeatedly developing from scratch. This also stimulated the transfer of MDD and CBD [12] techniques to the domain of embedded systems, but the predicted level of reuse has not yet been reached. A reason might be that empirical studies measuring the obtained reuse rates are sparse. Studies, such as [7] or [8] examined only specific aspects of reuse such as specialization or off-the-shelf component reuse, but did not provide comparative metrics on the method's level. Other empirical studies that directly focus on software reuse either address non-CBD technology [14], or they focus on representations on the programming language-level [15]. Unfortunately, there are no studies in the area of MDD/CBD for embedded systems.

This paper shortly introduces the MARMOT system development method. MARMOT stands for *Method for Component-Based Real-Time Object-Oriented Development and Testing*, and it aims to provide the ingredients to master the multi-disciplinary effort of developing embedded systems. It provides templates, models and guidelines for the products describing a system, and how these artifacts are built. The main focus of the paper is on a series of studies in which we compare MARMOT, as being specific for MDD and CBD with the RUP and Agile Development to devise a small control system for an exterior car mirror. In order to verify the characteristics of the three development methods, several aspects such as model size [13] and development effort are quantified and analyzed. The analysis reveals that model-based, component-oriented development performs well and leads to maintainable systems, plus a higher-than-normal reuse rate, at least for the considered application domain.

The paper is structured as follows: Section 2 briefly describes MARMOT, and Sections 3, 4, and 5 present the study, discuss results and address threats to validity. Finally, Section 6 presents a brief summary, conclusions drawn, and future research.

2 MARMOT Overview

Reuse is a key challenge and a major driving force in hardware and software development. Reuse is pushed forward by the growing complexity of systems. This section shortly introduces the MARMOT development method [3] for model-driven and component-based development (CBD) of embedded systems. MARMOT builds on the principles of KobrA [1], assuming its component model displayed in Fig. 1, and extends it towards the development of embedded systems. MARMOT components follow the principles of encapsulation, modularity and unique identity that most component definitions put forward, and their communication relies on interface contracts (i.e., in the embedded world these are made available through software abstractions). An additional hardware wrapper realizes that the hardware communication protocol is translated into a component communication contract. Further, encapsulation requires separating the description of what a software unit does from the description of how it does it. These descriptions are called specification and realization (see Fig. 1).

The specification is a suite of descriptive (UML [11]) artifacts that collectively define the external interface of a component so that the component can be assembled into or used by a system. The realization artifacts collectively define a component's internal realization. Following this principle, each component is described through a suite of models as if it was an independent system in its own right.

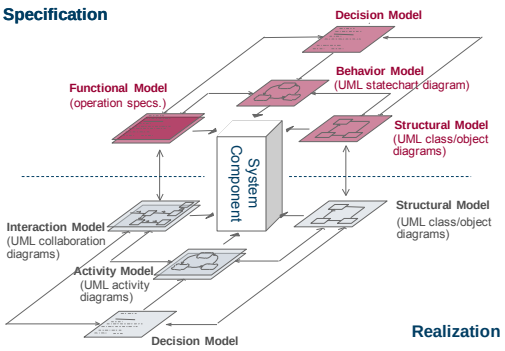

Fig. 1. MARMOT component model.

The fact that components can be realized using other components, turns a MARMOT project into a tree-shaped structure with consecutively nested abstract component representations. A system can be viewed as a containment hierarchy of components in which the parent/child relationship represents composition. Any component can be a containment tree in its own right, and, as a consequence, another MARMOT project. Acquisition of component services across the tree turns a MARMOT project into a graph. The four basic activities of a MARMOT development process are composition, decomposition, embodiment, and validation as shown in Fig. 2.

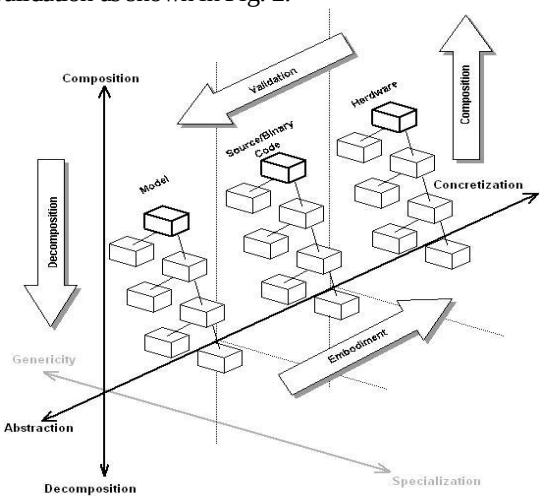

Fig. 2. Development Activities in MARMOT.

Decomposition follows the divide-and-conquer paradigm, and it is performed to subdivide a system into smaller parts that are easier to understand and control. A project always starts above the top left-hand side box in Fig. 2. It represents the entire system to be built. Prior to specifying the box, the domain concepts must be determined, comprising descriptions of all relevant domain entities such as standard hardware components that will appear along the concretization dimension. The implementation-specific entities determine the way in which a system is divided into smaller parts. During decomposition,

newly identified logical parts are mapped to existing components, or the system is decomposed according to existing components. Whether these are hard- or software is not important since all components are treated in a uniform way, as software abstractions.

Composition represents the opposite activity, which is performed when individual components have been implemented or reused, and the system is put together. After having implemented some of the boxes and having some others reused, the system can be assembled according to the abstract model. The subordinate boxes with their respective super-ordinate boxes have to be coordinated in a way that exactly follows the component description standard introduced above.

Embodiment is concerned with the implementation of a system and a move towards executable representations. It turns the abstract system (i.e., models) into concrete representations that can be executed. MARMOT uses refinement and translation patterns for doing these transformations. MARMOT supports the generation of code skeletons and can thus be regarded as a semi-automatic approach.

Validation checks whether the concrete representations are in line with the abstract ones. It is carried out in order to check whether the concrete composition of the embedded system corresponds to its abstract description.

3 Description of the Study

In general, empirical studies in software engineering are used to evaluate whether a "new" technique is superior to other techniques concerning a specific problem or property. The objective of this study is to compare the effects of MARMOT concerning reuse in embedded system development to other approaches such as the Unified process and agile development.

The study was organized in three runs (i.e., one run per methodology). All runs followed the same schema. Based on an existing system, documentation subjects performed a number of small projects. These covered typical project situations such as maintenance, ports to another platform, variant development, and reuse in a larger context. The first run applied MARMOT. The second run repeated all projects but used a variation of the Unified process, specifically adapted for embedded system development. The third run, applying an agile approach, was used to validate that modeling has a major impact and to rule out that reuse effects can solely be obtained at the code level. Metrics were collected in all runs and were analyzed in order to evaluate the respective research questions.

3.1. RESEARCH APPROACH

Introducing MDD and CBD in an organization is generally a slow process. An organization will start with some reusable components, and eventually build a component repository. But they are unsure about the return on investment gained by initial component development plus reuse for a real system, and the impact of the acquired technologies on quality and time-to-market. This is the motivation for performing the study and asking questions on the performance of these techniques.

Research Questions. Several factors concerning the development process and its resulting product are recorded throughout the study in order to gain knowledge about using MDD and CBD for the development of small embedded systems. The research

questions of the case-study focus on two key sets of properties of MDD in the context of component-oriented development. The first set of questions (Q1-Q4) lead to an understanding of basic and/or general properties of the embedded system development approach:
Q1: Which process was used to develop the system? Each run of the study used a different development approach (i.e., MARMOT, Unified Process, and Agile). These are compared in terms of different quality attributes of the resulting systems.
Q2: Which types of diagrams have been used? Are all UML diagram types required, or is there possibly a specific subset sufficient for this domain?
Q3: How were models transferred to source code? Developers typically work in a procedural setting that impedes the manual transformation of UML concepts into C [10].
Q4: How was reuse applied and organized? Reuse is central to MDD with respect to quality, time-to-market, and effort, but reuse must be built into the process, it does not come as a by-product (i.e., components have to be developed for reuse).

The second set of questions (Q5-Q9) deals with the resulting product of the applied approach (i.e., with respect to code size, defect density, and time-to-market).

Q5: What is the model-size of the systems? MDD is often believed to create a large overhead of models, even for small projects. Within the study, model size follows the metrics as defined in [13].
Q6: What is the defect density of the code?
Q7: How long did it take to develop the systems and how is this effort distributed over the requirements, design, implementation, and test phases? Effort saving is one promise of MDD and CBD [12], though, it does not occur immediately (i.e., in the first project), but in follow-up projects. Effort is measured for all development phases.
Q8: What is the size of the resulting systems? Memory is a sparse resource and program size extremely important. MDD for embedded systems will only be successful if the resulting code size, obtained from the models, is small.
Q9: How much reuse did take place? Reuse is central for MDD and CBD and it must be seen as an upfront investment paying off in many projects. Reuse must be examined between projects and not within a project.
Research Procedure. MDD and CBD promise efficient reuse and short time-to-market, even for embedded systems. Since it is expected that the benefits of MDD and CBD are only visible during follow-up projects [5], an initial system was specified and used as basis for all runs. The follow-ups then were:
R1/R2 Ports to different hardware platforms while keeping functionality. Ports were performed within the family (i.e., ATMega32) and to a different processor family (i.e., PICF). Implementing a port within the same family might be automated at the code-level, whereas, a port to a different family might affect the models.
R3/R4 Evolving system requirements by (1) removing the recall position functionality, and (2) adding a defreeze/defog function with a humidity sensor and a heater.
R5 The mirror system was reused in a door control unit that incorporates the control of the mirror, power windows, and door illumination.

3.2. PREPARATION

Methodologies. The study examines the effects of three different development methods on software reuse and related quality factors. In the first run, we used the MARMOT method that is intended to provide all the ingredients to master the multi-disciplinary effort of developing component-based embedded systems. In the second run we followed an adapted version of the Unified Process for embedded system development [4] (i.e., RUP SE). RUP SE includes an architecture model framework that supports different perspectives. A distinguishing characteristic of RUP SE is that the components regarding the perspectives are jointly derived in increasing specificity from the overall system requirements. In the third run, an agile process (based on Extreme Programming) [9], adapted towards embedded software development, was used.

Subjects of the study were graduate students from the Department of Computer Science at the University of Kaiserslautern (1st run) and the School of IT at the International University (2nd and 3rd run). All students, 45 in total (3 per team/project), were enrolled in a Software Engineering class, in which they were taught principles, OO methods, modeling, and embedded system development. Lectures were supplemented by practical sessions in which students had the opportunity to make use of what they had learned. At the beginning of the course, subjects were informed that a series of practical exercises were planned. Subjects knew that data would be collected and that an analysis would be performed, but were unaware of the hypotheses that were being tested. To further control for learning and fatigue effects and differences between subjects, random assignment to the development teams was performed. As the number of subjects was known before running the studies it was a simple procedure to create teams of equivalent size.

Metrics. All projects were organized according to typical reuse situations in component-based development, and a number of measurements were performed to answer the research questions of the previous sub-section:

Model-size is measured using the absolute and relative size measures proposed in [13]. Relative size measures (i.e., ratios of absolute measures) are used to address UMLs multi-diagram structure and to deal with completeness [13]. Absolute measures used are: the number of classes in a model (NCM), number of components in a model (NCOM), number of diagrams (ND), and LOC, which are sufficient as complexity metrics for the simple components used in this case. NCOM describes the number of hardware/software components, while NCM is represents the number of software components. These metrics are comparable to metrics such as McCabe's cyclomatic complexity for estimating the size/nesting of a system. Code-size is measured in normalized LOC. *System size* is measured in KBytes of the binary code. All systems were compiled using size optimization.

The *amount of reused elements* is described as the proportion of the system which can be reused without any changes or with small adaptations (i.e., configuration but no model change). Measures are taken at the model and code level.

Defect density is measured in defects per 100 LOC, whereby defects where collected via inspection and testing activities.

Development effort and its distribution over development phases are measured as development time (hours) collected by daily effort sheets.

Materials. The study uses a car-mirror control system that moves a mirror horizontally and vertically into the desired position. Positions can be stored/recalled to support driver profiles. The simplified version of this study controls two servos via potentiometers, and

indicates movement on a LCD. A replication package is available from the authors.

For each run, the base system documentation was developed by the authors of this paper. The reason was that we were interested in the reuse effects of one methodology in the context of follow-up projects. Using a single documentation for all runs would have created translation and understanding efforts. Therefore, reasonable effort was spent to make all three documents comparable concerning size, complexity, etc. This is supported by the measures of each system.

4 Evaluation and Comparison

In the context of the three experimental runs, a number of measurements were performed with respect to maintainability, portability, and adaptability of software systems. Tables 1, 2, and 3 provide data concerning model and code size, quality, effort, and reuse rates. Table columns denote the project type[1].

Table 1. Results of the First Run (MARMOT)

		Original	R1	R2	R3	R4	R5
LOC		310	310	320	280	350	490
Model Size (Abs.)	NCM	8	8	8	6	10	10
	NCOM	15	15	15	11	19	29
	ND	46	46	46	33	52	64
Model Size (Rel.)	$\frac{NumberofStateCharts}{NumberofClasses}$	1	1	1	1	0.8	1
	$\frac{NumberofOperations}{NumberofClasses}$	3.25	3.25	3.25	2.5	3	3.4
	$\frac{NumberofAssociations}{NumberofClasses}$	1.375	1.375	1.375	1.33	1.3	1.6
Reuse	Reuse Fraction(%)	0	100	97	100	89	60
	New (%)	100	0	3	0	11	40
	Unchanged (%)	0	95	86	75	90	95
	Changed (%)	0	5	14	5	10	5
	Removed (%)	0	0	0	20	0	40
Effort (h)	Global	26	6	10.5	3	10	24
	Hardware	10	2	4	0.5	2	8
	Requirements	1	0	0	0.5	1	2
	Design	9.5	0.5	1	0.5	5	6
	Implementation	3	1	3	0.5	2	4
	Test	2.5	2.5	2.5	1	2	4
Quality	Defect Density	9	0	2	0	3	4

First Run Porting the system (R1) required only minimal changes to the models. One reason is that MARMOT supports the idea of platform-independent modeling (platform specific models are created in the embodiment step). Ports to different processor families (R2) are supported by MARMOT's reuse mechanisms.

[1] Project types are labeled following the scheme introduced in section 3 (e.g., "Original" stands for the initial system developed by the authors as a basis for all follow-up projects, "R1" – Port to the ATMEGA32 microcontroller (same processor family), "R2" – Port to the PIC F microcontroller (different processor family), "R3" – Adaptation by removing functionality from the original system, "R4" – Adaptation by adding functionality to the original system, and "R5" – Reuse of the original system in the context of a larger system.

Concerning the adaptation of existing systems (R3 and R4), data show that large portions of the system could be reused. In comparison to the initial development project the effort for adaptations is quite low (26hrs vs. 3/10hrs). The quality of the system profits from the quality assurance activities of the initial project. Thus, the promises of CBD concerning time-to-market and quality could be confirmed.

Interestingly, the effort for the original system corresponds to standardized effort distributions over development phases, whereby the effort of follow-ups is significantly lower. This supports the assumption that component-oriented development has an effort-saving effect in subsequent projects.

Porting and adapting an existing system (R1-R4) implies that the resulting variants are highly similar, which explains why reuse works well. It is, therefore, interesting to look at larger systems that reuse (components of) the original system (i.e., R5). 60% of the R5 system was reused without requiring major adaptations of the reused system. Effort- and defect density are higher than those of R1-R4, due to additional functionality and hardware extensions. However, when directly compared to the initial effort and quality, a positive trend can be seen that supports the assumption that MARMOT allows embedded systems development at a low cost but with high quality.

Table 2. Results of the Second Run (Unified Process)

		Original	R1	R2	R3	R4	R5
LOC		350	340	340	320	400	500
Model Size (Abs.)	NCM	10	10	10	8	12	13
	NCOM	15	15	15	11	19	29
	ND	59	59	59	45	60	68
Model Size (Rel.)	$\frac{NumberofStateCharts}{NumberofClasses}$	1.5	1.5	1.5	0.72	1.33	1.07
	$\frac{NumberofOperations}{NumberofClasses}$	4	3.5	3.5	3.25	3	3.46
	$\frac{NumberofAssociations}{NumberofClasses}$	2.5	2.3	2.3	2.5	2.16	1.76
Reuse	Reuse Fraction(%)	0	100	94	88	86	58
	New (%)	100	0	6	11	14	42
	Unchanged (%)	0	92	80	70	85	86
	Changed (%)	0	4	15	6	15	14
	Removed (%)	0	4	5	24	0	41
Effort (h)	Global	34	8	12	5.5	13	29
	Hardware	10	2	4	0.5	2	8
	Requirements	4	1	1	1.5	3	4
	Design	12	1	2	1	4	7
	Implementation	5	2	3	1.5	2	6
	Test	3	2	2	1	2	4
Quality	Defect Density	8	1	2	0	3	4

The **Second and Third Run** replicated the projects of the first run but used different development methods. Interestingly, the results of the second run are quite close to those of the first. However, the Unified Process requires more overhead and increased documentation, resulting in higher development effort. Ironically, model-size seems to have a negative impact on quality and effort. Interestingly, the mapping of models to code seems not to have added additional defects or significant overheads.

Although the amount of modeling is limited in the agile approach, it can be observed that the original system was quickly developed with a high quality. However, this does not hold for follow-up projects. These required substantially higher effort than the effort

required for runs 1 and 2. A reason might be that follow-ups were not performed by the developers of the original system. Due to missing documentation and abstractions, reuse rates are low. In contrast, the source-code is of a good quality.

Table 3. Results of the Third Run (Agile)

		Original	R1	R2	R3	*R4*	R5
LOC		280	290	340	300	*330*	550
Model Size (Abs.)	NCM	14	15	15	13	*17*	26
	NCOM	5	5	5	4	*7*	12
	ND	3	3	3	3	*3*	3
Model Size (Rel.)	$\frac{NumberofStateCharts}{NumberofClasses}$	0	0	0	0	*0*	0
	$\frac{NumberofOperations}{NumberofClasses}$	3.21	3.3	3.3	3.15	*3.23*	4.19
	$\frac{NumberofAssociations}{NumberofClasses}$	3.5	3.3	3.3	3.46	*3.17*	2.57
Reuse	Reuse Fraction(%)	0	95	93	93	*45*	25
	New (%)	100	5	7	7	*55*	75
	Unchanged (%)	0	85	75	40	*54*	85
	Changed (%)	0	14	15	40	*36*	10
	Removed (%)	0	1	10	20	*10*	5
Effort (h)	Global	18	5	11.5	6	*13.5*	37
	Hardware	6	2	4	1	*2*	8
	Requirements	0.5	0	0	0.5	*1*	1
	Design	2	0	0	1	*1.5*	3
	Implementation	7	2	5	2	*6*	18
	Test	2.5	1	2.5	1.5	*3*	7
Quality	Defect Density	7	0	2	1	*5*	7

5 Threats to Validity

The authors view this study as exploratory. Thus, threats limit generalization of this research, but do not prevent the results from being used in further studies.

Construct Validity. Reuse is a difficult concept to measure. In the context of this paper it is argued that the defined metrics are intuitively reasonable measures. Of course, there are several other dimensions of each concept. However, in a single controlled study it is unlikely that all the different dimensions of a concept can be captured.

Internal Validity. A maturation effect is caused by subjects learning as the study proceeds. The threat to this study is subjects learned enough from single runs to bias their performance in the following ones. An instrumentation effect may result from differences in the materials which may have caused differences in the results. This threat was addressed by keeping the differences to those caused by the applied method. This is supported by the data points as presented in table 1, 2, and 3. Another threat might be the fact that the studies were conducted at different institutes.

External Validity. The subjects were students and are, therefore, unlikely to be representative of software professionals. However, the results can be useful in an industrial context for the following reasons: Industrial employees often do not have more experience than students when it comes to applying MDD. Furthermore, laboratory settings allow the investigation of a larger number of hypotheses at a lower cost than field studies. Hypotheses supported in the laboratory setting can be tested further in industrial settings.

6 Summary and Conclusions

The growing interest in the Unified Modeling Language provides a unique opportunity to increase the amount of modeling work in software development, and to elevate quality standards. UML 2.0 promises new ways to apply object/component-oriented and model-based development techniques in embedded systems engineering. However, this chance will be lost, if developers are not given effective and practical means for handling the complexity of such systems, and guidelines for applying them systematically.

This paper shortly introduced the MARMOT approach that supports the component-oriented and model-based development of embedded software systems. A series of studies was described that were defined to empirically validate the effects of MARMOT on aspects such as reuse or quality in comparison to the Unified Process and an agile approach. The results indicate that by using MDD and CBD for embedded system development will have a positive impact on reuse, effort, and quality. However, similar to product-line engineering projects, CBD requires an upfront investment. Therefore, all results have to be viewed as initial. This has led to the planning of a larger controlled experiment to obtain more objective data.

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Towards Quality-Driven Model Transformations: A Replication Study

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Abstract. Commonly, there are several ways to transform a source model into a target model. These alternative target models may have the same functionality but can differ in their quality attributes. One of the key challenges of an automated transformation process is to identify the transformations that will produce a target model with the desired quality attributes. In this paper, we present a replica of a controlled experiment to investigate the selection of alternative transformations to obtain UML class models from a Requirements Model. This is a concrete instantiation of a wider domain-independent approach for quality-driven model transformation. Specifically, we focus on a set of transformations related to structural relationships between classes (association, aggregation and association class) and the understandability quality attribute. Although, some results could be foreseen even by a superficial analysis of the alternatives, the goal of this work is to use experimentation to gather empirical evidence about which alternative transformation produces the UML class model that is the easiest to understand. The empirical results support the original results showing that there is a tendency to favor the use of association relationships to drive these transformations when understandability is chosen.

Keywords: Model transformations, MDA, Software Quality, Requirements, UML class model, Empirical Software Engineering.

1 Introduction

Model-Driven Architecture (MDA) is an emerging approach to software development. It promotes the use of models and model transformations as the primary artefacts to be built and maintained. In essence, an MDA development process transforms a Platform-Independent Model (PIM) into one or more Platform-Specific Models (PSM), which are then transformed into code (Code Model – CM). Therefore,

in this context, a model is no longer simply a means for describing software, but rather an essential piece of the software development process. Consequently, the quality of the models built throughout this process is of great significance since these models will determine the quality of the software product that is finally deployed.

Usually, in an MDA development process there are several ways to transform a source model into a target model. Alternative target models may have the same functionality but differ in their quality attributes. One of the key challenges for an *automated transformation process* is to identify which transformations will produce a target model with the desired quality attributes (e.g., understandability, modifiability).

In the last few years, some approaches that deal with quality in Model-Driven Engineering (MDE) have been proposed [15] [14] [12] [13] [8] [9]. One disadvantage of these approaches is that the practical applicability of model transformations is often reported based on the intuition of the researcher. As pointed out by Czarnecki and Helsen [4], there is a lack of controlled experiments to fully validate the observations made by the researchers in the field of MDE. Therefore, more systematic approaches to ensure quality in MDE processes are needed.

In this paper, we present a replica of a controlled experiment to investigate the selection of alternative transformations to obtain UML class models from a Requirements Model [5] [6]. This work is part of a project on *quality-driven model transformations* whose overall goal is the definition of a quality metamodel to drive the selection of alternative model transformations according to different quality attributes.

Specifically, we focus on a sub-set of transformations that are related to structural relationships between classes (association, aggregation, and association class) and the 'understandability' quality attribute. These transformations have been selected because the determination of structural relationships has a great impact on the UML class model. Understandability has been selected since it is recognized as the main quality attribute that influences maintainability. A UML class model must be understood before any desired change to it can be identified, designed or implemented.

The goal of the experimentation is to gather empirical evidence about which alternative transformation produces the UML class model that is the easiest to understand. The empirical evaluation of the best transformation is particularly important when the transformations are automatically applied, which is the main reason for adopting MDA [11].

This paper is organized as follows. Section 2 gives an overview of our approach to transform a Requirements Model into UML class models. This section also shows the analysis of alternative model transformations. Section 3 describes the design of the original experiment and its replica to empirically validate the selection of alternative transformations with regard to the understandability. Section 4 presents the data analysis and the interpretation of the results. Finally, section 5 presents our conclusions and future work.

2 Transforming Requirements into UML Class Models

Following an MDA development approach, it is important to ensure quality in every step of the development process. In this context, automated model transformation plays a key role for success. We propose to empirically validate model transformations with regard to quality attributes and use this information to drive the selection of alternative transformations. A quality attribute is a measurable physical or abstract property of an entity (e.g., conceptual model) [7].

A model transformation is executed taking a transformation definition as input. A transformation definition contains transformation rules that relate constructs in the source model to constructs in the target model. We use another input for the transformation process that is the definition of the quality attributes together with the corresponding empirical evidence gathered from controlled experiments. This information will feed the transformation process with the criteria to choose the alternative transformation that maximizes the selected quality attribute. The rationale of this approach is to be able to automatically select the alternative transformation that an experienced software developer would select if the transformation process were manually applied. In this section, we present a concrete application of the quality-driven model transformation approach to transform software requirements into UML class models (see Fig. 1).

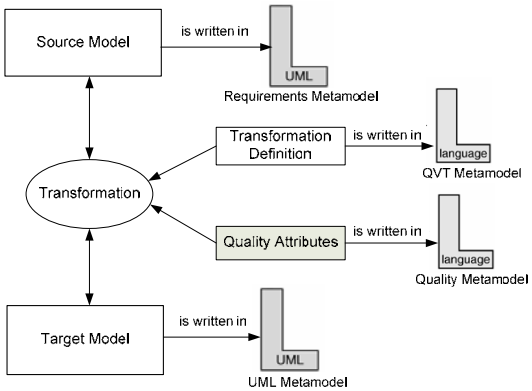


Fig. 1. The quality-driven model transformation approach

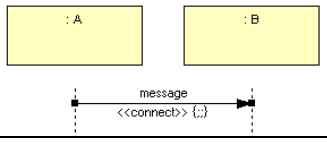
The Requirements Model [5] [6] defines the structures and the process followed to capture the software requirements following an MDA approach. It is composed of a *Functions Refinement Tree (FRT)* to specify the hierarchical decomposition of the system functionality, a *Use Case Model* to specify the system communication and functionality, and *Sequence Diagrams* to specify the required object-interactions necessary to realize each Use Case. This Requirements Model is supported by a Requirements Engineering TOol – RETO (http://reto.dsic.upv.es). Once the Requirements Model has been specified, a conceptual model including a UML class model can be obtained by applying a set of transformations from the Transformation Rules Catalog (TRC) [5]. These transformation rules establish traceability relationships between the Requirements Model and the UML class models.

The application of a Transformation Rule (TR) implies that a certain structural pattern match in the Requirements Model and that a resultant structure in the UML class model can be generated while establishing a traceability relationship between them. Some transformations are easy to apply once the transformation pattern has been matched (a *one-to-one* relation). For example, the generation of classes for the UML class model is based on the analysis of participating classes in all the Sequence Diagrams. However, other transformations are not easy to identify or apply for three main reasons: the complexity of the transformation pattern, the non-disjoint condition pattern of the transformation, and the multiple valid representation of a conceptual model for a given requirement pattern.

2.1 Analyzing Alternative Transformations

Following, we explain with examples some transformations where multiple alternatives arise because of non-disjoint condition patterns and multiple possible representations of the same pattern. Fig. 2 shows the Sequence Diagram used to specify the necessary object interactions to realize the Use Case *Create Insurance* of a Car Rental system. This Use Case represents the creation of a car *Insurance* policy that must be bought from an *Insurance Company* and assigned to the *Car* before using the car for rentals. The actor *Administrator* initiates the interaction (message 1). After introducing the necessary data and checking the existence of the corresponding car (messages 2 to 4), a new *Insurance* object is created (message 5). In addition, an *Insurance Company* object and a *Car* object must be connected to the newly created *Insurance* policy object (messages 6 and 7).

After analyzing the requirement specification provided by the previous Sequence Diagram, the analyst of the system could possibly determine that the partial Class Model that best represents this requirement is defined by two association relationships that relate the newly created class *Insurance* (message 5) to the *InsuranceCompany* and *Car* classes as shown in Fig. 3(a). This partial Class Model can be obtained by applying twice the following transformation rule TR15 (Association) from the Transformation Rule Catalog to the messages 6 and 7:

TR15 (Association): *For every message between two classes labeled with the stereotype «connect», THEN an association relationship between these classes will be generated.*	
Pattern condition:	**Result:**
	

4

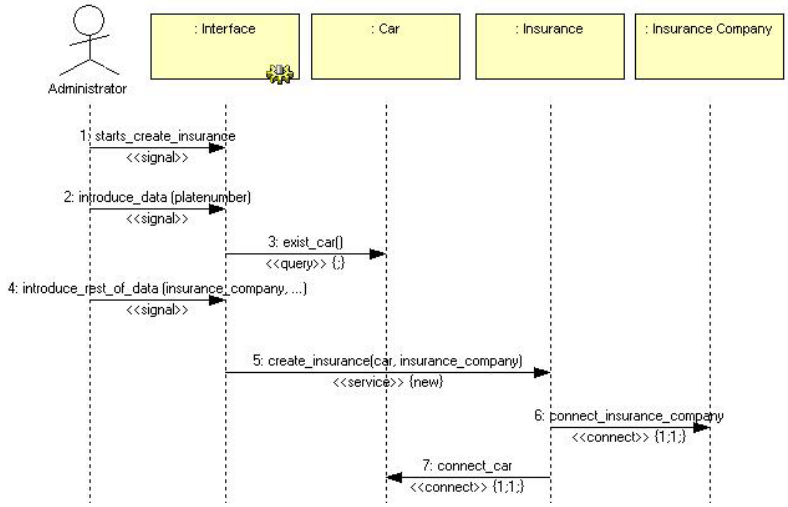
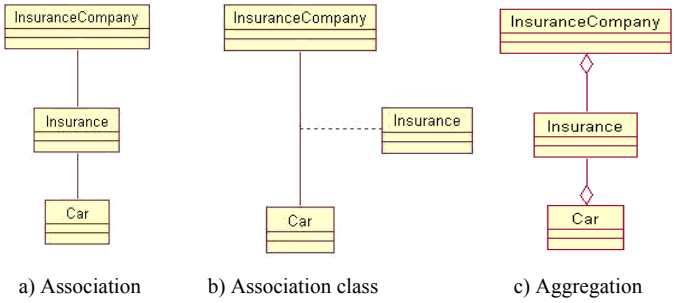


Fig. 2. Sequence Diagram showing the required interactions for the Use Case *Create Insurance*

As an alternative solution to the same requirement specification, the analyst could prefer a solution that uses an association class (named *Insurance*) to relate the *Car* rented with the *Insurance Company* as shown in Fig. 3 (b). This partial Class Model can be obtained by applying the transformation rule TR39 (AssociationClass) to the messages 5, 6 and 7. Finally, another possibility is the definition of two aggregation relationships, one between *Insurance Company* and *Insurance* classes and another between *Car* and *Insurance* classes as shown in Fig. 3 (c). This partial Class Model can be obtained by applying twice the transformation rule TR28b (Aggregation) to the messages 6 and 7.



a) Association b) Association class c) Aggregation

Fig. 3. Two partial Class Models for the Car Rental system

An important issue derived from these examples is that different alternative structural relationships can be derived from the analysis of the requirements specifications described using Sequence Diagrams. And what is more important is the fact that the part of the information used in the Sequence Diagrams for deriving these

structural relationships follows the same pattern: a message with the stereotype *new*, followed by two messages with a stereotype *connect*.

In the examples, we have shown that TR15, TR39, or TR28b transformation rules could be applied, depending on the interpretation of the analyst if the transformation process is performed manually. In an automated transformation process, the information provided by the stereotyped Sequence Diagrams is not sufficient to automatically determine which structural relationship of a Class Model best realizes the specified requirement. Because these alternatives exist, it is the human analyst who should decide which alternative better represent a solution in the corresponding problem domain. In this work, considering that alternatives exist, we use controlled experiments to discover which alternative transformation maximizes a given quality attribute of the resulting target model (e.g., understandability).

3 A Controlled Experiment and its Replica

In a previous work [1], we presented a controlled experiment to determine which of the transformation rules for structural relationships: association (A1), aggregation (A2), or association class (A3), obtained the easiest to understand UML class model. The results show a slight tendency to favor the transformations related to associations (A1). However, as Basili et al. [2] suggested relevant and credible results can only be obtained by replicating the experiments. In other words, single studies rarely provide definitive answers. Therefore, we carried out an internal strict replication (changing only the subjects) to corroborate the findings. To provide an overall view of the experimentation, we explain both the original experiment and the replica.

3.1 Planning

The participants in the original experiment were 39 fourth-year students in Computer Science at the Universidad Politécnica de Valencia, who were taking part in the second Software Engineering course. We took a "convenience sample" (i.e., all the students in the class). The subjects had six month of experience in modeling with UML and three years of experience in the OO paradigm. The subjects that participated in the replica were a different group of 37 students from the same course.

The independent variables for the experiments were the transformation rules for structural relationships between classes (A1, A2 and A3). The dependent variable was understandability. The experimental material and tasks consisted of:

- 9 Sequence Diagrams from three different case studies (a car rental system, a hotel management system, and a singer contest system), with 3 UML class models each. These were obtained by applying the alternative transformation rules. The material used is available at: www.dsic.upv.es/~einsfran/experiment.
- Each Sequence Diagram had a questionnaire attached consisting of 6 Yes/No questions to test the subjects' understanding of the Sequence Diagrams. The effectiveness of the subjects in answering the questionnaires (number of correct

answers by number of answers) was used to exclude those observations that did not fulfill a minimum level of quality.

- Each of the three UML class models had a questionnaire attached (with 6 questions) for assessing which UML class model was best understood by the subjects. In addition, the subjects had to write down the starting and ending times for completing the questionnaires. For this purpose we used a wall clock. From this understanding task, we obtained three measures for understandability:
  Understandability Time, which reflects the time, in seconds, that the subjects spent answering each questionnaire (calculated by the difference between the ending time and the starting time). Each subject completed 4 questionnaires detailing 3 alternatives (A1, A2 and A3). Three understandability time measures (A1Time, A2Time and A3Time) were obtained.
  Effectiveness, which reflects the correctness of the answers (number of correct answers by number of answers). Three understandability effectiveness measures (A2Effec, A2Effec and A3Effec) were obtained.
  Efficiency, which reflects the correctness of the answers by time (number of correct answers by understandability time). Three measures for understandability efficiency (A2Effic, A2Effic and A3Effic) were obtained.
- The final task of each test asked the subjects which of the three alternative UML class models best reflected the problem modeled in the Sequence Diagram. It is a subjective measure (*Alternative Selected*) based on the subjects' perception.

The following hypotheses were formulated:

- **$H1_0$**: The use of different alternative transformations does not affect the Understandability Time (A1Time, A2Time and A3Time). $H1_1=\neg H1_0$
- **$H2_0$**: The use of different alternative transformations does not affect the Understandability Effectiveness (A1Effec, A2Effec and A3Effec). $H2_1=\neg H2_0$
- **$H3_0$**: The use of different alternative transformations does not affect the Understandability Efficiency (A1Effic, A2Effic and A3Effic). $H3_1=\neg H3_0$
- **$H4_0$**: There is no correlation between the Alternative Selected and the means of objective Understandability variables (Understandability Time, Effectiveness, and Efficiency). $H4_1= \neg H4_0$

We selected a balanced within-subject design, i.e., each subject received the same experimental material.

3.2 Execution

Both the original experiment and the replica started with an introductory session in which we reviewed the main concepts of the Requirements Model (e.g., the notation of Sequence Diagrams). The goal of the experiment was not disclosed to the subjects. Then, we showed an example of the experimental material, which was similar to the material they would use during the execution of the experiment.

Each subject was assigned all the material, with the nine tests (balanced within-subject design). The models were assigned in different order to limit learning effects. We showed them how to develop the experimental tasks, and they had a maximum of two hours to complete all the tasks.

After the experiment took place, we collected the experiment data, which consisted of a table of 351 rows (9 models x 39 subjects) and 9 columns (A1Time, A2Time, A3Time, A1Effec, A2Effec, A3Effec, A1Effic, A2Effec, A3Effec). The replica had the same structure, but with 331 rows (9 models x 37 subjects). In both samples, we performed a "data cleaning", excluding the observations that were not complete because the subjects had not written down the time or because the subjects did not select the best alternative. All the questions were answered in each questionnaire, thereby assuring the completeness of the performed tasks. We also excluded the observations that had a value of effectiveness of 50% or less for each Sequence Diagram. We considered that if the level of correct answers was low in relation to the Sequence Diagram, the subjects had not really understood the model, and they would probably not perform well in the following tasks, so we discarded them. Therefore, the final data for testing the hypotheses were 325 observations for the original experiment and 293 for the replica.

4 Data Analysis and Interpretation

The following statistical analyses were performed to analyze the data: (1) a descriptive study was done to characterize the variables Alternative Selected, Understandability Time, Effectiveness, and Efficiency; (2) Hypotheses H1, H2, and H3 were tested using an ANOVA test with repeated measures; (3) Hypothesis H4 was tested using the Spearman correlation coefficient.

We used SPSS to carry out the data analyses presented in this study. The transformation most selected by the subjects was A1, i.e., the subjects believed that the use of associations obtained the easiest to understand UML class model. Association class (i.e., alternative A3) was the transformation that was least selected, which reveals that it could be the least appropriate transformation.

The descriptive statistics we carried out for Understandability Time, Effectiveness and Efficiency suggest the following:

- **Original Experiment.** On average, the subjects spent less time performing the tasks related to alternative A2; however, the difference with the others is not very significant (≈ 8 seconds for A1 and A3). The subjects were more effective and efficient performing the tasks related to alternative A1; however, the difference in effectiveness with the other alternatives is not very significant.
- **Replica.** The measures related to A1 have the best values, which means that, on average, the subjects spent less time and were more effective and more efficient performing the tasks related to the class model that was obtained via associations.

In summary, the descriptive statistics show a slight tendency in favor of A1, the transformation based on associations. Surprisingly, the difference between the minimum and maximum time values is significant. This may be due to the fact that the subjects were novice modelers.

To test the hypotheses presented in section 3.1, we carried out an ANOVA for repeated measures, which is the appropriate statistical test for analyzing the collected data [10]. Due to space constraints, we will briefly present the main findings obtained through the ANOVA for each data sample:

- **Original Experiment.** We can reject hypotheses $H1_0$ ($p = 0.0002$), $H2_0$ ($p = 0.0001$), and $H3_0$ ($p = 0.0005$), with a significance level = 0.05. This means that the use of different alternative transformations really affects understandability time, effectiveness, and efficiency.
- **Replica.** We can reject $H1_0$ ($p = 0.0028$) and $H2_0$ ($p = 0.0003$), which means that the use of one alternative or another does not affect efficiency but does affect time and effectiveness when the subjects understand the class models.

When planning the experiment, we designed it in such a way as to alleviate the threats to the internal validity.

One limitation to the external validity (i.e., the generalization of the findings) of this study is the fact that the three alternative transformation rules cannot be applied simultaneously to all modeling situations. For instance, to establish an association class relationship (A3), at least one «service/new» message and two «connect» messages are needed in the source model. The goal of this experimentation was to gather empirical evidence for the specific case when the three alternative transformations could be applied to obtain a relationship between classes. We are aware that, more alternatives may be possible to represent structural relationships between classes. More experimentation is needed to validate these other combinations. Another limitation to the external validity might be the use of students as experimental subjects. However, the students who participated in the experiment can be considered to be representative of novice users of conceptual modeling approaches. To increase external validity, the current study needs to be replicated using experienced practitioners.

5 Conclusions and Future Work

This paper has presented an analysis of alternative model transformations and how controlled experiments can be used to provide useful information to guide the selection of transformations in an automated transformation process. In particular, we presented the results of an experiment to gather evidence about which alternative transformation produces the UML class model that is easiest to understand.

The main findings obtained from the experimentation are the following: (a) the transformation that was most selected in the original experiment and the replica was the association transformation (A1); (b) the results of the replica confirm the results of the original experiment for effectiveness. The subjects were more effective when they understood the class models with association relationships; (c) the fact that the hypothesis related to efficiency could not be confirmed in the replication has no great impact on our approach since the transformations are automatically executed in a Model Management framework (MOMENT) [3].

These results show that there is a slight tendency to favor the use of association relationships as part of an automated transformation process. A possible reason for this could be that this relationship has less semantic strength than the other kinds of relationships. When an aggregation relationship is chosen instead of an association relationship, analysts know that they are defining a part-of relationship. In the case of an association class, it is possible to represent almost the same relationship using two

association relationships. Although the results obtained can be quite obvious the important point is the systematic approach presented to validate this 'obvious' results. That the association relationship is more understandable than the aggregation relationship or association classes is something that almost all the people can say but until this moment no one has the data to confirm that result but merely by intuition.

These preliminary results provided empirical evidence that can be further used to define a domain-independent quality metamodel to drive the execution of model transformations. Nevertheless, more replication is needed for building a body of knowledge. We plan to replicate this experiment with practitioners. Future work also includes the evaluation of the remaining transformations of the Transformation Rules Catalog taking into account other quality attributes (e.g., usability, modifiability).

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Analyzing the Influence of Certain Factors on the Acceptance of a Model-based Measurement Procedure in Practice: An Empirical Study

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Abstract. Full automatic software measurement from conceptual models is now accepted by academics, although take-up of these model-based measurement procedures in practice by software practitioners has been slow. To encourage acceptance in industry, an acceptance model for measurement procedures is proposed, identifying a set of factors that influence perceived usefulness and perceived ease of use when a user employs a measurement procedure. Analyzing the results of an empirical study carried out with software engineering academics, we find which factors have an influence on other factors. Using regression analysis, certain factors are identified that affect perceived usefulness and ease of use, and which in turn will affect intention to use.

1. Introduction

Although software measurement is recognized as a key element of engineering science, it has not yet been widely accepted in practice by software practitioners. The Software Engineering Measurement and Analysis (SEMA) group at the Software Engineering Institute (SEI) concluded from a series of explorative studies carried out from 2004-2005 [1] that there is still a significant gap between the current and desired state of software measurement. One of reasons for this is that there are no programs that use measures and empirical evidence to assess the practical relevance of such programs.

Nowadays, with the appearance of the model-driven development process, several approaches have arisen which allow for full-automatic software measurement of specific artifacts developed at early stages and in particular contexts [2][3][4][5][6]. However, the question is whether these model-based measurement procedures would be accepted in practice.

According to Cooper and Zmud [7], acceptance is one of the stages in the diffusion of technological innovations, and is defined from an employee perspective; an organization's personnel are induced to commit to Information Technology application usage. Acceptance must not be confused with adoption; which is defined

as a stage where negotiations are started in relation to the decision to adopt the innovation and mobilizing of organizational and financial resources for doing so [7].

The acceptance of technology has been investigated in a number of different fields [7][8][9]; however, in the software measurement field there are few papers on this subject in the literature.

Umarji and Emurian [10] focus on the evaluation of the likelihood of acceptance of a metrics program. Their model takes as input organizational culture, and the nature of the metrics program. Gopal et al. [11] researched the influence of institutional factors on the assimilation of metrics in software organizations. They also identified a set of determinants for metrics program success [12]. These determinants are divided into organizational and technical variables.

Our proposal focuses on a model-based measurement procedure relating to acceptance from a software practitioner's perspective. A number of models exist for evaluating the acceptance of new techniques and technology, in particular the Technology Acceptance Model (TAM) [14]. The Method Evaluation Model (MEM) [21], which uses the same TAM constructs, was the first to be applied in the context of Functional Size Measurement (FSM) procedures ([3], [17]). From preliminary results obtained with MEM, a theoretical model was defined, which includes a set of factors that affect practitioners' perceptions, perceptions that will determine the user's intention to use the model-based measurement procedures [13].

The aim of this paper is to analyze the influence of these factors on acceptance of RmFFP in practice, using the regression analysis technique. RmFFP is a measurement procedure designed to automatically estimate the functional size of object-oriented applications generated in an MDA environment

This paper is structured as follows: Section 2 introduces an acceptance model for model-based measurement procedures, Section 3 shows how an initial empirical study is carried out to analyze the causal relationships of the model, and finally, our conclusions are given and further work is suggested.

2. Evaluating the acceptance of measurement procedures

In order to define our model for evaluating acceptance of model-based measurement procedures; we use the same TAM constructs, but which have been redefined in the following way [13]:

- **Perceived Ease of Use:** the extent to which a person believes that using a particular measurement procedure would be free of effort.
- **Perceived Usefulness:** the extent to which a person believes that a particular measurement procedure will be effective in achieving intended objectives.
- **Intention to Use:** the extent to which a person intends to use a particular measurement procedure.

In addition, we identified the following factor types:

- **Intrinsic Factors** related to the intrinsic nature of software measurement procedure; these correspond to quality and tangibility of results, and the minimum number of actions required for calculating the measure using a measurement procedure.

- *Quality of results:* extent to which a person believes that the results of using a measurement procedure are accurate and convertible.
- *Tangibility of results:* extent to which a person believes that the results of using a measurement procedure are observable and understandable.
- *Minimum actions:* extent to which a person believes that using a particular measurement procedure would obtain results with the minimum number of actions required.

- **Extrinsic Factors** that do not depend on the measurement procedure in itself; these correspond to the experience and job relevance of the software practitioner.
  - *Job relevance:* extent to which an individual believes that a measurement procedure is applicable and relevant to his or her job.
  - *Experience:* knowledge or skill gained in the use of measurement procedures over a period of time.
- **External factors** that depend on the organization as a whole. These factors include where the business follows trends in the market based on advertising and marketing or peer company use, or the maturity level of an organization, or has business priorities giving rise to time or cost constraints.

The causal relationships hypothesized between the TAM constructs and factors of the model are shown in Figure 1. In the next section, we present an empirical study to analyze these causality relationships.

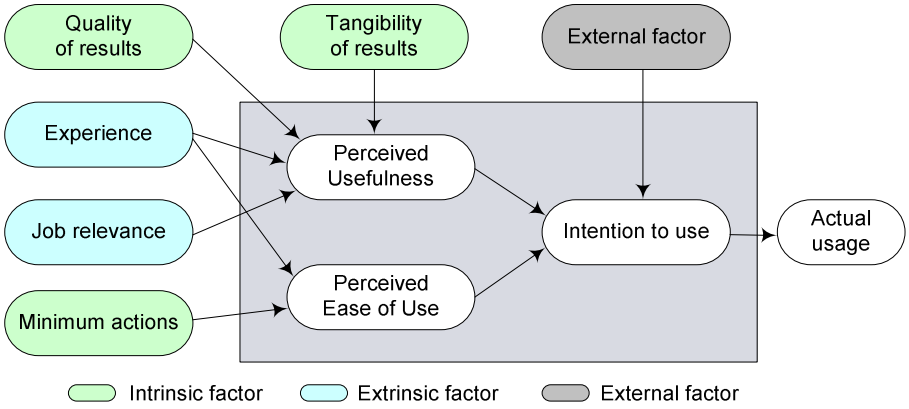

Figure 1. Acceptance model for model-based measurement procedures

3. Analyzing causality relationships in the Acceptance of RmFFP

RmFFP is a functional size measurement procedure designed on the basis of the COSMIC standard method, which has been approved by ISO/IEC 19761 [20]. RmFFP was proposed in order to automatically estimate the functional size of object-oriented systems generated in an MDA environment [5]. The object to be measured is the functional requirements specification obtained using the OO-Method requirements model [18].

This procedure starts with the definition of the *measurement strategy*, which includes the purpose, the scope, and the measurement viewpoint. The scope of RmFFP comprises the functionality to be included in a particular measurement. The measurement viewpoint corresponds to the 'analyst' viewpoint, which will focus on a requirements specification (object of interest).

Then, RmFFP starts a *mapping phase* to identify the significant primitives of the Requirements Model that contribute to the system's functional size according to the concepts of the COSMIC [20]. We defined sixteen mapping rules whose principal purpose is to reduce misinterpretation about the COSMIC generic concepts and to facilitate the automation of the RmFFP procedure. For instance, each use case is identified as a functional process; each message of the sequence diagram is identified as a data movement type, etc. The main outcome of this phase is the identification of data movements that are fundamental components of COSMIC.

Once the data movements have been correctly identified, we proceed with the *measurement phase*, whose purpose is to produce a quantitative value that represents the software functional size of a requirements specification. To do this, we apply the measurement function, which consists of assigning a numerical value of 1 Cfsu (Cosmic Functional Size Unit) to each data movement. We defined four rules to add together these quantified data movements. To do this, we used the relationship types between use cases to calculate the size of the functional processes (use case) and the size of the entire system

3.1 Planning: Case study

In order to define the goal of our empirical study, we used the Goal/Question/Metric (GQM) template [15], which is described as follows:
To analyze the Acceptance Model proposed ***for the purpose of*** evaluating RmFFP ***with respect to*** their acceptance in the practice ***from the viewpoint*** of the researcher ***in the context of*** software engineering professors using a measurement procedure for requirements specifications.

From this goal, the following research questions were addressed by this study:
RQ1: is perceived usefulness of the RmFFP measurement procedure really influenced by certain intrinsic factors?
RQ2: is perceived usefulness of the RmFFP measurement procedure really influenced by certain extrinsic factors?
RQ3: is perceived ease of use the RmFFP measurement procedure really influenced by certain intrinsic factors?
RQ4: is the intention to use really a result of the perceptions experienced by the subjects using the RmFFP measurement procedure?

Selection of subjects. The subjects were 20 professors from various Peruvian universities. They were enrolled in the United Nations summer school on "Advanced Techniques in Software Development", February - March 2007. The careful selection of participants was based on academic qualifications, teaching or industrial experience, technical background, and specific interest in software engineering. The

empirical study was organized as a part of the "Measurement and Software Quality" course given during the summer school.

Variables and Hypotheses. Using the framework proposed by Juristo and Moreno [16], we identified three types of variables:

- **Response variables:** variables that correspond to the outcomes of the empirical study. For this study, we considered certain factors and constructs of the Model as response variables: Perceived Ease of Use (PEOU), Perceived Usefulness (PU), Intention to Use (IU), Job Relevance (JR), Quality of Results (QR), Tangibility of Results (TR), and Minimum Actions (MA). We omitted the extrinsic factor: "experience" and the external factors, which will be considered in further studies. As these outcomes should be measurable, we used a 5-point Likert scale format.
- **Factors:** variables that affect the response variable. In our study, this variable corresponds to the Models-based Measurement Procedures, and as single treatment: the RmFFP procedure [5]
- **Parameters:** variables that we do not want to influence the experimental results: level of practitioner's experience using a measurement procedure; complexity of conceptual models to be measured.

The following hypotheses regarding the research questions were considered:
H1: Perceived Usefulness is determined by the quality of results of the RmFFP measurement procedure.
H2: Perceived Usefulness is determined by the tangibility of results of the RmFFP measurement procedure.
H3: Perceived Usefulness is determined by job relevance using the RmFFP measurement procedure for the software practitioner.
H4: Perceived ease of use is determined by the minimum number of actions required using the RmFFP measurement procedure.
H5: Intention to use is determined by usefulness perceived.
H6: Intention to use is determined by perceived ease of use.

3.2 The Collection Data Method

First, we gave an introduction on how to apply the RmFFP measurement procedure by means of illustrative examples. Finally, we verified the knowledge learned by the participants by working through an assigned application. The time used for the training session was 4 hours distributed over two days. Then, each subject used the RmFFP measurement guide to measure a requirements specification of a Car Rental application with thirty-five use cases. The time allowed for this task was unlimited.

Finally, each subject was asked to complete a specially-designed survey to evaluate RmFFP acceptance. The time allowed for this task was also unlimited.

Instrumentation. A survey instrument[1] was designed to measure the response variables, with twenty closed questions. These questions consisted of 6 items used to measure PEOU; 2 items to measure PU; 3 items to measure IU; 4 items to measure JR; 2 items to measure QR; 1 item to measure TR; and 2 items to measure MA. Table 1 presents the four items used for the job relevance factor.

Table 1. Items formulated for measuring the job relevance factor

Construct	Description	Items
Job relevance	It is possible for a measurement procedure not to be perceived as useful even though the procedure provides accurate results, possibly because the use of the measurement procedure is not relevant for the job type of the software practitioner concerned.	1. Using the measurement procedure, the performance of my job will improve. 2. The use of the measurement procedure is relevant for my job. 3. Using the measurement procedure could increase the effectiveness of the development of my tasks. 4. I would use a measurement procedure, if I had to manage a software project

Responses to the instrument were based on a 5-point Likert scale ranging from (1), strongly disagree, to (5), strongly agree. The order of the items was randomized and some questions negated to avoid monotonous responses.

We also used a set of training materials, such as: a set of instructional slides on RmFFP procedure; an example of the application of RmFFP, and a measurement guide.

3.3 Data Analysis and Interpretation

As we can see in Figure 1, the intention to use a measurement procedure is influenced by perceptions of usefulness and ease of use; which can be influenced by certain type of factors. We identified several relationships, which were defined above in the six hypotheses (H1-H6). In this section, we analyze them by applying the regression analysis technique.

H1: Quality of results → Perceived usefulness. The regression equation resulting from the analysis is: *PU = 2.376 + 0.477*QR*.

The regression had a high significance level ($p < 0.01$), which means that H1 was confirmed. The determination coefficient ($R^2 = 0.316$) showed that 31.6% of the total variation in perceived usefulness can be explained by variation in quality of results.

H2: Tangibility of results → Perceived usefulness. The regression equation resulting from the analysis is: *PU = 3.208 + 0.236*TR*.

[1] http://www.dsic.upv.es/~nelly/survey2.pdf

The regression had a null significance level ($p > 0.1$), which means that H2 was not confirmed.

H3: Job Relevance → Perceived usefulness. The regression equation resulting from the analysis is: *PU = 2.86 + 0.348*JR*.

The regression had a medium significance level ($p < 0.05$), which means that H1 was confirmed. The determination coefficient ($R^2 = 0.186$) showed that 18.6% of the total variation in perceived usefulness can be explained by variation in job relevance.

H4: Minimum actions → Perceived ease of use. The regression equation resulting from the analysis is: *PEOU = 2.733 + 0.314*MA*.

This regression had a null significance level ($p > 0.1$), which means that H4 was not confirmed.

H5: Perceived usefulness → Intention to use. The regression equation resulting from the analysis is: *ITU = 1.628 + 0.577* PU*.

The regression had a medium significance level ($p < 0.05$), which means that H5 was confirmed. The determination coefficient ($R^2 = 0.166$) showed that 16.6% of the total variation in intention to use can be explained by variation in perceived usefulness.

H6: Perceived ease of use → Intention to use. The regression equation resulting from the analysis is: *ITU = 2.881 + 0.298* PEOU*.

The regression had a null significance level ($p > 0.1$), which means that H6 was not confirmed.

Table 2 below summarizes the regression analysis results in terms of the predictive power (R^2) and significance level of the model (p), and the confirmation of the casual relationships.

Table 2. Regression analysis results

Causal hypotheses	Predictive power	Significance. level*	Confirmed?
H1: QR → PU	31.6%	High	Yes
H2: TR → PU	--	Null	No
H3: JR→ PU	18.6%	Medium	Yes
H4: MA→ PEOU	--	Null	No
H5: PU → IU	16.6%	Medium	Yes
H6:PEOU→IU	--	Null	No

Note that three hypotheses out of six were confirmed using a regression analysis (H1, H3, and H5). This means, that the perceived usefulness is determined by the quality of results, and by the job relevance using RmFFP for the software practitioner. In addition, the intention to use RmFFP is determined by the perceived usefulness.

3.4 Validity evaluation

It is important to ensure that the obtained results are valid, we present the more important threats related to our empirical study in Table 3.

* Null: $\alpha > 0.1$, Low : $\alpha < 0.1$, Medium: $\alpha < 0.05$, High: $\alpha < 0.01$, Very high: $\alpha < 0.001$

Table 3. Type of threats to the validity of the results obtained in our empirical study

Type of threats	Description
Conclusion validity	• *Random heterogeneity of subjects:* All the subjects selected for the empirical study had approximately the same level of background. We are aware that this homogeneity reduces the external validity of our empirical study. • *Reliability of measures:* We are aware that the measures based on perceptions are less reliable than objective measures, since it does not involve human judgment. However, to diminish this threat, we carried out a reliability analysis on the survey used, which is explained below.
Construct validity	• *Inadequate pre-operational explanation of constructs:* To ascertain whether the constructs are sufficiently defined, and, hence the experiment is sufficiently clear, we conducted a reliability analysis on the survey, calculating reliability using the Chronbach alpha technique. The generic value obtained was 0.85 indicating that the items included in the survey are reliable. However, a design adjustment on the questions corresponding to the constructors PU, MA and QR would be required for further empirical studies, since their corresponding Cronbach alpha values were lower than 0.7 ([19]).
Internal validity	• *Instrumentation:* This is the effect caused by the artefacts used in the study execution. The requirements specification of the Car Rental System was reviewed; and the measurement guide was verified in advance with a small group of people in order to improve its understandability.
External validity	• *Interaction of selection and treatment:* This is the effect of not having a representative population in the experiment with which to generalize. In our case, we are aware that more studies with a larger number of subjects would be appropriate to reconfirm the initial results obtained.

4. Conclusions and further work

This paper provides a brief introduction to a theoretical model to evaluate the acceptance of measurement procedures from an individual perspective. The model includes three types of factors that influence perceptions of usefulness and ease of use (intrinsic, extrinsic and external factors). An empirical study has been carried out to verify causal relationships that include the intrinsic and extrinsic factors. The analysis

shows that perceived usefulness is influenced by the job relevance of the people that use a measurement procedure. However, with respect to intrinsic factors, only the quality of results could affect the perception of usefulness. Perceived ease of use cannot be determined by the minimum actions factor. Furthermore, the results show that the intention to use a measurement procedure can be influenced more strongly by perceived usefulness than by perceived ease of use.
We plan to make further adjustments to the questions on the survey to improve the reliability of certain constructs, such as PU, MA, and QR. In addition, we are aware that further experimentation with industry practitioners will be appropriate in order to reconfirm these initial results. Finally, as further empirical studies, we also intend to consider the influence of software practitioners' experience on the acceptance of model-based measurement procedures.

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Towards a generic framework for empirical studies of Model-Driven Engineering

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Abstract. The goal of this paper is to introduce a work-in-progress approach that intends to formalize and facilitate empirical studies in Software Engineering in general and in Model-Driven Engineering in particular. The main idea is to use a detailed model of software that makes explicit the different intermediate models used at the different levels of abstraction, their different quality characteristics together with their relationships. The expected benefits of using such an explicit modeling is illustrated though five examples for which empirical studies are designed (but not yet conducted) on basis of that approach.

1 Introduction

Though Model-Driven Engineering techniques are very in vogue in academic world, their introduction into industry seems very slow. One of the suggested reasons is the difficulty to convince decision makers of Model-Driven Engineering advantages in terms of qualities and consequently regarding return on investment. Indeed, such argumentation necessitates empirical evidence.

Some major problems in conducting empirical studies in MDE are related to the use of classical quality models. In this paper, we claim that considering software as a single product with a list of quality characteristics (maintainability readability, efficiency...) is too rough to be used as basis for empirical studies in MDE. Instead, we suggest the use of a detailed model of the software products that makes explicit the different intermediate products (the different interrelated models) together with their relationships. The ultimate goal is to elaborate a framework to help design empirical studies in MDE. The remainder of this paper is organized as follows: Section 2 presents some related works our approach relies on and complements as well as the remaining problems we intend to address. Section 3 describes the framework and the approach themselves while Section 4 describes some examples of use.

2 Related work and Issues

The effort described here relies on research linked to both quality measurement and empirical software engineering. The basis of empirical studies in software engineering can be found in [28, 18]. Software measurement has witnessed too many metric proposals to cite them here but also benefits from generic theoretical works like [9, 16] while quality assessment benefits from numerous quality models — notably McCall's [25, 19], Boehm's [3, 2], FURPS [15], ISO [17], Dromey's [8]. Unfortunately those works do not take *explicitly* into account design-related quality. Software design quality still benefits from its own research. For instance, [6] summarizes many object-oriented design measures and studies the relationships between them and software quality. We can find publications, notably [13, 14, 12, 11, 10, 20, 21], that focus on how to estimate the quality of *models*. Finally, a generic approach to evaluate design is also proposed in [7].

Nevertheless we still identify some difficulties and limitations that appear when conducting empirical investigations in Software Engineering in general and in Model-Driven Engineering in particular.

To begin with, software measurement methods in general lack clarity about what they really measure. Theoretically, measurement process consists in quantifying relevant features (attributes) of a product (entity) in order to estimate another feature (quality) that is not directly quantifiable [9]. Practically, the entity supposed to be measured is very frequently not defined in a precise way and roughly called "software". If this view is sufficient for some empirical studies focusing on the quality of the software product as a whole, investigating Model-Driven Engineering necessitates more. In fact, since Model-Driven Engineering copes with different models and handles them as distinguished products, the qualities of the different models have to be clearly distinguished.

Moreover, the attribute supposed to be measured is frequently unclear. Numbers produced by applying a measurement method on a given model are sometimes used to estimate or predict different attributes without any clarification — e.g., a complexity measurement method used as size measurement. This can lead to incongruous use of software metrics and therefore to a completely wrong, or at least biased, quality assessment [16].

Model-Driven Engineering deals with a succession of models, from the more abstract ones to the code. Each model corresponds to a given abstraction level and has its own concerns about quality. But, as any model produced during software development is a part of an overall workflow, the inner quality of a given model is as important as the preservation of this quality through subsequent transformation steps leading to the final product. As a consequence, it can be unclear whether a quality criteria of a given model from a given development step actually improves or worsens the quality of the overall software product.

Also, Model-Driven Engineering is based on model *transformations*. However, research focuses are usually put on the preservation of semantic properties — the correctness of a transformation is defined on that basis. Traditional semantics approaches only encapsulate the "functional" aspects while empirical studies are needed to estimate a larger set of software quality attributes — including

various non-functional aspects. Whatever quality model and vocabulary is used quality attributes include not only the functionality but also other attributes which are based on cognitive sub-characteristics — e.g., understandability, modifiability,... — related to the syntax. Empirical investigation in Model-Driven Engineering should thus take this specific type of qualities into account.

Finally, software development is mainly a matter of information transmission between people and transformation through different levels of abstraction and viewpoints [7]. Nevertheless, almost every effort relating to quality does not address such aspects — i.e. how easy the transformation process of a given model will be. Model-Driven Engineering also supposes that most of the models should be generated through automated — or at least very systematic — transformations. Investigating the preservation of the quality attributes all along the transformation process trough empirical studies should thus also imply to question the quality of these transmissions and transformations.

3 An approach to improve empirical studies of quality

This section introduces a framework we are elaborating with the intent to address the previously cited issues. The basic idea is to use model of software that makes explicit the various models used in the development as well as their relationships in terms of quality. Section 3.1 deals with the model of the software while Section 3.2 is concerned with the quality characteristics.

3.1 Modeling the software product(s).

Our framework relies on an *explicit* representation of software as a *complex* and *composite* product. In the followings we introduce a generic structure of software (see Figure 1) we claim to be sufficient to illustrate our approach. Our generic model introduces two levels of decomposition. The first level of decomposition focuses on software as the collection of outcomes of a multifaceted process involving many distinct activities. As a matter of fact, software life cycle models intend to organize those activities in a structured and rational manner [26] and the outcome of these activities is mostly composed of models — it is even arguable that this outcome is *exclusively* composed of models if "model" is defined as *just a representation*. In order to be as general as possible and to cover most life cycle models, we use a generic structure which divides the process into three categories of activities: Requirement Engineering, Architectural Design and Implementation where each category includes all the (sub)activities involved in the production of three *global artifacts*: the Requirements, the Design and the Source Code. Even though the process behind a software life cycle is not necessary linear — as shown notably in [4, 5, 24, 22] —, each *global artifact* relies on another previously produced one (except for the Requirements).

The second level of decomposition logically focuses on the internal structure of those composite *global artifacts*. Each of them is in fact made of one or more specialized elements referenced to as *elementary artifacts* in the framework. The

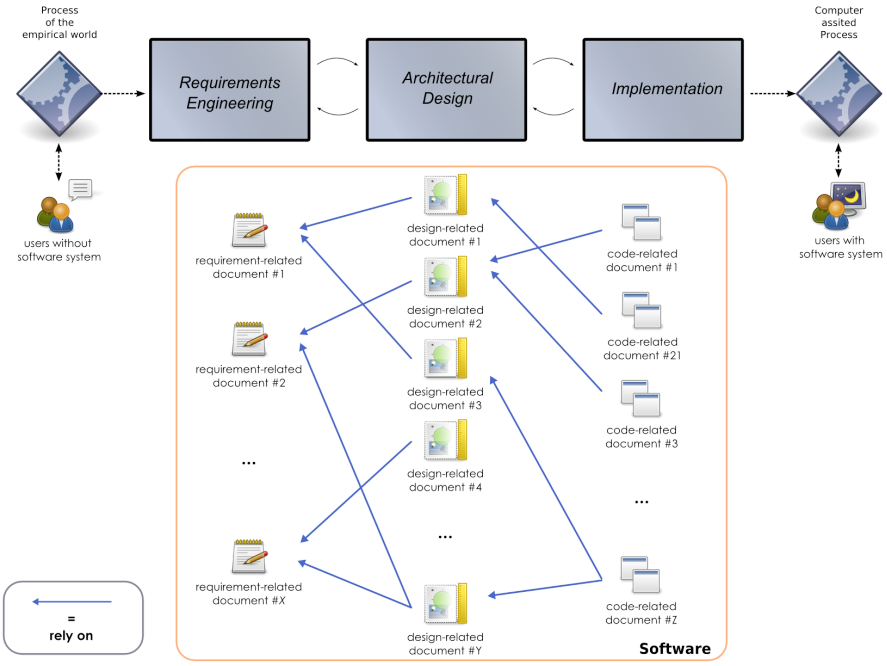

Fig. 1. Software as a composite and complex artifact

word *elementary artifact* means here any self sufficient piece of information comprised in a *global artifact* (e.g. a diagram, a structured text, a list of items in a text, a file, etc.). Each *elementary artifact* has a type (e.g., static structure diagram, dynamic structure diagram, source file, etc.), is written in a given language (e.g., UML, java, etc.), with a given level of abstraction and can be requirement-, design-, or code-related. Moreover, the granularity of the decomposition is variable so that it is possible to define *elementary artifacts* with more or less important scope. Finally, each element is part of an interconnected network of dependencies partially inherited from the dependencies of the composite *global artifacts*. At this level of our investigation, such a generic model seems to be sufficient to support the approach.

3.2 Modeling the software characteristics.

The main use of this view is to support a refined definition of the different quality characteristics, to relate each of them to the adequate product (elementary artifact or composite one) and to express and study relationships between them. The final aim is to build a quality model that is more flexible and adequate for model-driven approaches than the existing ones. Practically, as the quality of any type of elements can almost certainly be evaluated through a set of proposed "quality characteristics" — as hinted notably in [12, 11, 10]—, a powerful

quality model could be built by defining a list of characteristics assigned to each element then by determining the "influence" relationship between attributes. At this point, we can illustrate the "influence relationship" with the following partial example based on our practice, and common sense.

Each entry of the table below has the following structure:

Globalartifact.TypeOfElementaryartifact.Characteristic, where

- **Globalartifact** is either R(equirements), D(esign) or C(ode).
- **TypeOfElementaryartifact** is : NFu stands for non functional requirements, Fu for functional requirements, Struc for structural aspect, Behav for behavioral aspects, Run for aspects involved at runtime or "*" which means that any type of *Elementary Artifact* is involved. The type is used to categorize the elementary artifacts according to the role they play in the development. Not every type is present in every *global artifact*.
- **Characteristic** is either Main(tainability), Usab(ility), Func(tionality), Effi (ciency) [17] or Pres(ence) — since the presence of a given artifact can be considered as quality-related information in this approach as we will see in Section 4. A characteristic could be meaningful for only a subset of a type of elementary artifact but this preliminary attempt intends to be as generic and simple as possible.
- Each cell contains either N(no influence expected) or I (some influence expected)

Table 1. Expected Influence relationships

	...	D.Struc.Main	D.Behav.Main	C.Run.Func	C.Run.Effi	C.*.Main
R.NFu.Usab	...	*N*	*I*	*N*	*I*	*N*
R.Fu.Usab	...	*I*	*N*	*I*	*N*	*N*
R.*.Pres	...	*I*	*I*	*I*	*I*	*I*
D.Struc.Main	...		*N*	*I*	*I*	*I*
D.Behav.Main	...	*N*		*I*	*I*	*I*
D.*.Pres	...	*I*	*I*	*I*	*I*	*I*
...	...	...	...	...	...	...

The table intends to give a new formulation of the questions to be dealt with through empirical studies. It shows the plausible expectations in terms of influence at a very general level. That is, its content represents a set of plausible

hypotheses to explore through empirical investigations, each of them involving going to a lower level (subtype of element artifact, quality sub-characteristic and internal metrics). The table is far from being final at this point and each "path" can be questioned. Also, this current table is only focusing on very generic characteristics so that each user of the framework can expand, propose and study new relationships or refine existing ones with different characteristics or other classifications of artifacts. So it is only through the massive use of the proposed method within the community of Empirical Software Engineering that a consensual and widely accepted table of this kind could be achieved. We believe that such table would be a more helpful basis to conduct empirical studies in model-driven engineering than usual quality models.

4 Some typical scenarios of use

The novelty of the framework introduced above lies in the particular point of view adopted. It is more flexible in the sense that it allows to get *inside* Model-Driven process and investigate the influence of various artifacts on other ones. Then this approach supports more specialized investigation such as the relevance of a particular transformation in comparison with another.

The first step in order to use the framework is to make *explicit* the dependencies between studied artifacts — e.g., *elementary artifact* c1 is produced thanks to d1 and d2. Then the idea is to consider these variables as "typed" variables where each "type" (requirement-, design, code-related) has its own set of meaningful quality characteristics. Finally, the table of influences is used to express the hypotheses about the characteristics involved and their relationships. Besides being an innovative way to support empirical studies in general, the present approach particularly suits Model-Driven Engineering for two main reasons. First, most of elementary artifacts, are supposed to be models. Since elementary artifacts are almost the primary form of expression of our framework and models are the one of MDE, the two approaches should be compatible. Secondly, our approach is designed to address the transformation of information, which is a core concept of any model-driven approach.

The remainder of this Section illustrates the use of the framework on five hypothetical experiments and highlights the benefits of the framework in those situations. The structure of the cases is inspired by the GQM paradigm [1, 27].
Case 1

- **Goal.** Study the impact of the presence of a design pattern P on the maintainability of the produced code.
- **Question.** Let d1, d2 be 2 class diagrams, where d2 satisfies the same set of requirement-related *elementary artifacts* REQ1 than d1, but d1 uses a design pattern P while d2 does not; c1 & c2 are two pieces of java program produced from d1, d2, respectively, through a transformation T. Is c2 more maintainable than c1?

- **Metrics.** To study the maintainability of the code, the experimenter could use the effort needed to complete a maintenance task as a metric. This is consistent with classical quality models which propose to decompose maintainability into sub-characteristics which are mainly measurable through effort. Though the choice of such measurement method is questionable by itself, it could still be used as an approximation.
- **Discussion.** This experiment could be achieved without the support of the framework but would miss some benefits. With our framework, this experiment can be expressed as the investigation of the influence of a structural design-related *elementary artifact* on the maintainability of a code-related *elementary artifact*. This path is present in the table of expected influences (Table 3.2), which means that the experiment seems relevant. Moreover, the use of the framework highlights the fact than design patterns are *part of the design* and not the code, a view that is not always admitted.

Case 2

- **Goal.** Study the impact of one design characteristic on the same characteristic at the code level. For instance the goal could be to investigate whether a focus on the maintainability at the design level does not produce more complex code and therefore impact negatively the global maintainability of the software product.
- **Question.** Let d1, d2 be 2 diagrams at the design level, where d1 is more maintainable than d2, and c1 & c2 be two pieces of java program produced from d1 & d2, respectively, through a transformation T. Is c2 more maintainable than c1?
- **Metrics.** This situation illustrates perfectly the case where the present approach complements and is nurtured by other related works when it comes to selecting the adequate metrics. Indeed, we can use and integrate to the framework the research that has been done regarding the maintainability of UML diagrams and how to evaluate it thanks to internal metrics [12, 11]. For the code maintainability, see Case 1.
- **Discussion.** This case is almost similar to the first one but illustrates how the framework allows us to further investigate software quality. While the first case was about the influence of a technique on a quality characteristic, this case proposes to confront the *quality characteristics* of two entities and see how they are related. Without the framework and its specific point of view, this experiment — the investigation of the influence of the maintainability of a design-related *elementary artifact* on the maintainability of a code-related *elementary artifact* — would need an extra descriptive effort to show that maintainability does not apply to the same entity on both sides of the relationship.

Case 3

- **Goal.** Study the impact of one design characteristic on another characteristic at the code level. For instance the goal could be to investigate whether a

focus on the maintainability all along the design process does not impact negatively the efficiency of the software code.
- **Question.** Let d1, d2 be 2 design models where d1 is more maintainable than d2 and c1 & c2 be two pieces of java program produced from d1, d2, respectively, through a transformation T. Is c2 more efficient than c1?
- **Metrics.** The same principles as in Case 2 apply for d1 and d2. Metrics related to efficiency could be specifically designed for the experiment or taken from ISO standards.
- **Discussion.** This case is set at the same level than Case 2 : the aim is to study internal mechanisms of software quality by questioning the relationship between two quality characteristics. The framework helps give sense to this experiment : though maintainability and efficiency do not seem to be related in any way when applied to the same entity, the framework allows us express the fact that a relation of cause and effect exists between the two *elementary artifacts* and probably between their respective quality characteristics. In this context, the legitimacy of the question is clearer.

Case 4

- **Goal.** Study the benefits of a given transformation methodology (or tool) regarding the preservation of a given quality characteristic. For instance the goal could be to question whether a transformation T preserves, improves or worsens the complexity of the software code.
- **Question.** Let DES be a collection of design-related *elementary artifacts* (e.g., class diagrams & statecharts & sequence diagrams) and let C1, C2,. . . , Cn be different source code — and thus *global artifacts* — produced by the transformations T1, T2,. . . ,Tn, respectively. Is C1 more complex than C2,. . . , Cn?
- **Metrics.** McCabe's number could be used to assess complexity according that some precaution is taken [23].
- **Discussion.** This case illustrates how the framework can support investigations about the quality of the engineering process. In previous cases, the transformation process was fixed and the studied variable was an *elementary artifact*. Here, we fix the design-related *elementary artifacts* and investigate how various transformations provide various level of quality at the code level.

Case 5

- **Goal.** Determine the relevant level of abstraction — requirement-, design- or code-related — and the suitable models involved where it would be meaningful to take a given characteristic into account — e.g., security.
- **Question.** Let $< R1, D1, C1 >$, $< R2, D2, C2 >$ and $< R3, D3, C3 >$ be three software products; the non-functional requirement of security is modeled by an adequate elementary artifact since the requirement level in $< R1, D1, C1 >$, since the design level in $< R2, D2, C2 >$ and in the code in $< R3, D3, C3 >$, which is the most secure product?
- **Metrics.** Security metrics are not proposed here but could be designed specially for the experiment.

– **Discussion.** This case illustrates how the framework can express questions about the "temporal" impact of the introduction of some *elementary artifacts*. It also shows how the particular point of view chosen in this approach allows to make a quality characteristic out of a very simple attribute like the presence of absence of a given artifact.

5 Conclusion and future work

The approach introduced in this paper is a first and currently evolving attempt to address some limitations of software quality assessment. Though relying on a very simple and light mechanism, the main benefit brought by this framework is to force the experimenter to adopt a more accurate and flexible view of software. The examples given in Section 4 are just some of the possible experimentations that could benefit from it. In each case, the use of "typed variables" clarifies the "dimension" involved and allows to avoid ambiguity about what the experimenter expects to address. Section 4 also illustrates how basic attributes like the presence of a given *elementary artifact* become valuable quality criteria when software is considered from this point of view — i.e., as a composite artifact resulting from a network of interconnected pieces of information.

As an early work, the approach naturally lacks experimental data to confirm all the benefits we expect. The next step of the development will be to apply the framework to an actual empirical study in the context of a student development project. The table of expected influences also need experimental confirmation, but should eventually constitute a valuable reference for anyone interested in further transversal investigations about the internal mechanisms of software quality.

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