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New Perspectives on the System Usage Construct

Andrew Burton-Jones

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NEW PERSPECTIVES ON THE SYSTEM USAGE CONSTRUCT

BY

ANDREW BURTON-JONES

A Dissertation Submitted in Partial Fulfillment of the Requirements for the Degree
of
Doctor of Philosophy
in the Robinson College of Business
of
Georgia State University

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Acceptance

The dissertation was prepared under the direction of the candidate's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Business Administration in the Robinson College of Business Administration of Georgia State University.

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Abstract

New Perspectives on the System Usage Construct

By

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July 2005

Committee Chair: Dr. Detmar Straub

Major Department: Computer Information Systems

Information systems are designed to support human and organizational purposes. To achieve their ends, information systems must be used. Although this may seem to be self-evident, there are many aspects of systems usage that are not so, and yet, in spite of this, there has been little intense conceptual scrutiny of this construct in past research.

The objective of this thesis, therefore, is to develop new in-depth perspectives for studying system usage. Drawing on critical realist assumptions and studies of research diversity, I explain how epistemological factors enable while ontological factors constrain the diversity of meanings of system usage, and I build on this reasoning to advance a systematic approach for conceptualizing and measuring system usage in an appropriate way for a given research context.

To demonstrate the approach and judge its usefulness, I carry out three empirical studies to test whether measures of system usage that are selected according to the proposed approach provide more explanatory power and lead to more coherent results in specific research contexts than other measures of system usage. Exploring the relationship between system usage and user task performance among 804 users of spreadsheet software, the experiments reveal support for the usefulness of the approach and demonstrate how it can enable researchers to conceptualize and measure system usage in an appropriate manner for a given research context.

Together, the conceptual approach and empirical studies contribute by: (1) providing a systematic way to conceptualize and measure system usage for a given study context, (2) revealing rich new directions for research on the nature of system usage, its antecedents, and its consequences, and (3) suggesting a new approach for construct development and investigation in IS research.

Chapter 1

Introduction[1]

1.1 Overview

The June 2003 issue of the *Harvard Business Review* included 14 letters to the editor debating the merits of Carr's article entitled "IT Doesn't Matter" (2003). A key theme of the letters was whether impacts from information technology (IT) stem from IT itself, or from how it is used. For many years, information systems researchers have debated the same question. Some emphasize the deterministic effects of IT, while others stress that its impacts stem from use in specific contexts (Markus 1994; Markus 2004; Robey and Sahay 1996).

Although it may be self-evident that IT impacts stem from use, there has been little in the way of exacting studies of the nature of system usage as a theoretical construct. At a nomothetic level, many have studied its antecedents (Compeau et al. 1999; Venkatesh et al. 2003). Others have studied its consequences (Gelderman 1998; Lucas and Spitler 1999). However, there has been scant attention as to the nature of usage itself (DeLone and McLean 2003). Studies of the antecedents to use have converged on highly predictive theories (Venkatesh et al. 2003), but the nature of usage typically escapes theoretical scrutiny in such studies. Studies of the consequences of use report weak results and have recently called for more research to understand how to conceptualize and measure the usage construct (Chin and Marcolin 2001).

Perhaps the most detailed understanding of system usage comes from idiographic researchers. They show how similar users can use IT in different ways (Barley 1986; Robey and Sahay 1996) and how users employ IT unconventionally (DeSanctis and Poole 1994; Orlikowski

[1] Burton-Jones, A. "New Perspectives on the System Usage Construct," Working paper, Department of Computer Information Systems, Georgia State University, 2005.

1996). However, because of their meta-theoretical assumptions, they have rarely studied system usage as a research *construct*. Nor, until recently, have they studied its consequences, e.g., on performance (Orlikowski 2000).

Overall, past research on system usage is marked by two distinguishing characteristics:

- diverse conceptualizations of system usage
- disconnected conceptualizations of system usage

To illustrate the diversity of conceptualizations of system usage that exist in the literature, Table 1.1 summarizes 14 different measures of system usage and many minor variants that have been used at the individual level analysis.

Table 1.1: Diverse Measures of System Usage at an Individual Level of Analysis†

Broad Measure	Individual Measures	Used as Independent Variable	Used as Dependent Variable
System usage measured as the use of information from an IS			
Extent of use	Number of reports or searches requested	✓	✓
Nature of use	Types of reports requested, general vs. specific use	✓	
Frequency of use	Frequency of requests for reports, number of times discuss or make decision using information		✓
System usage measured as the use of an IS			
Method of use	Direct versus indirect		✓
Extent of use	Number of systems, sessions, searches, displays, reports, functions, or messages; user's report of whether they are a light/medium/heavy user.	✓	✓
Proportion of use	Percentage of times use the IS to perform a task		✓
Duration of use	Connect time, hours per week	✓	✓
Frequency of use	Number of times use system, daily/weekly	✓	✓
Decision to use	Binary variable (use or not use)		✓
Voluntariness of use	Binary variable (voluntary or mandatory)		✓
Variety of use	Number of business tasks supported by the IS	✓	✓
Specificity of use	Specific versus general use		✓
Appropriateness of use	Appropriate versus inappropriate use	✓	✓
Dependence on use	Degree of dependence on use	✓	✓

†This list was induced from a sample of 48 IS articles in major journals from 1977-2005 (see Appendix 5A).

Diverse conceptions of system usage should not be surprising, given that the system usage construct is:

- one of the longest standing constructs in IS research (DeLone and McLean 2003; Ginzberg 1978; Lucas 1978b)

- studied in many different subfields of IS research, including IS success (DeLone and McLean 1992), IS for decision making (Barkin and Dickson 1977), IS acceptance (Davis 1989) , IS implementation (Hartwick and Barki 1994), group support systems (Zigurs 1993), and practice perspectives on IT impacts (Orlikowski 2000)
- studied at many levels of analysis, such as the individual (Straub et al. 1995), group (DeSanctis and Poole 1994), and organizational levels (Devaraj and Kohli 2003).

Debates on the merits of research diversity generally conclude that *disciplined* diversity is desirable (Benbasat and Weber 1996; Landry and Banville 1992; Robey 1996; Weber 2003a). Is diversity within the system usage literature disciplined? The weight of evidence suggests that it is not. For example, at the individual-level of analysis, there are no accepted definitions of system usage (DeLone and McLean 2003; Trice and Treacy 1986), researchers rarely choose measures of usage based on theory (Chin 1996; Chin and Marcolin 2001), researchers rarely validate the system usage construct empirically (Igbaria et al. 1997), and researchers rarely justify their methods for measuring system usage (Collopy 1996; Straub et al. 1995). The situation is similar at other levels of analysis. As Zigurs (1993, p. 117) demurred after reviewing the group-level literature, "system usage is an example of a deceptively simple construct that needs to be looked at more carefully."

In addition to these problems, there is a marked disconnect among conceptions of system usage across levels. For example, Figure 1.1 illustrates markedly different conceptions of system usage at different levels of analysis. Although some diversity across levels should be expected, some cohesion should also be expected because system usage at higher levels of analysis must emerge from system usage at lower levels of analysis. In other words, groups and organizations can only "use" systems if individuals use them (Rousseau 1985). Thus, one might expect to see research describing how individual usage and collective usage are similar, how they are different, and how they affect each other. Despite calls to consider such multilevel issues (Harris 1994), such research has been absent in IS research (Chan 2000).

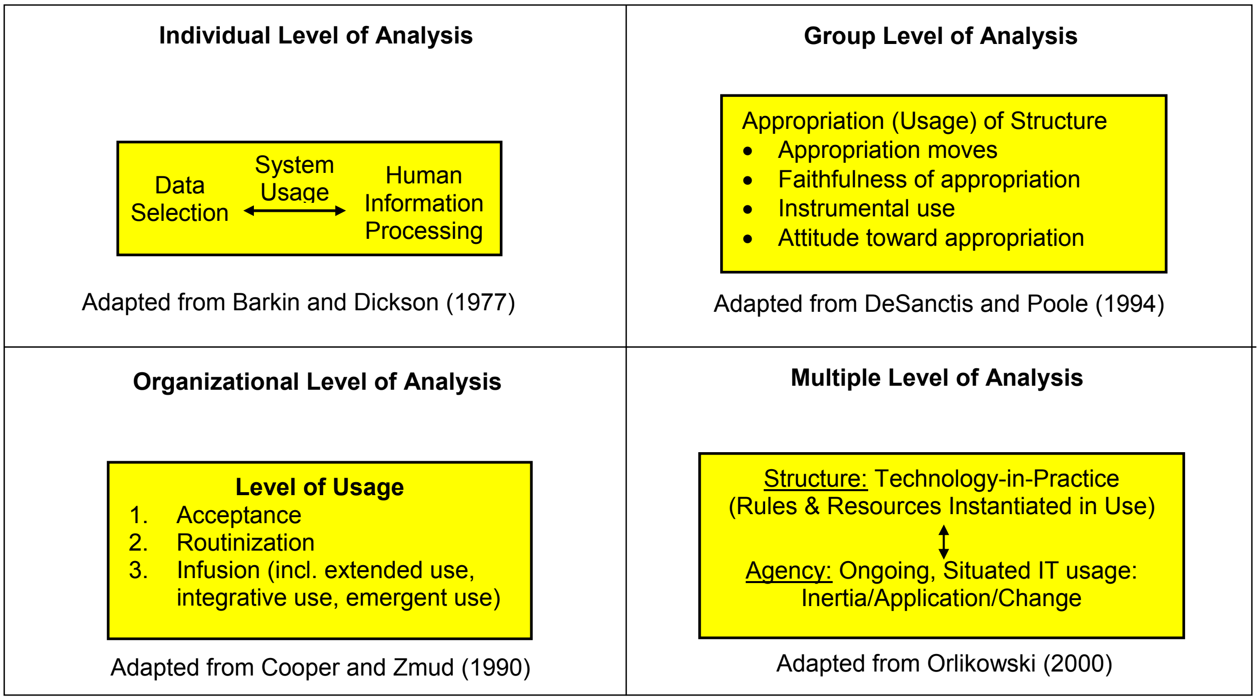

Figure 1.1: Conceptualizations of System Usage across Levels of Analysis

Overall, therefore, conceptions of system usage in IS research appear to lack disciplined diversity. This is unfortunate because a lack of disciplined diversity can make it difficult to achieve cumulative theoretical progress (Berthon et al. 2002). There is an important gap, therefore, in the literature: the need for a way to increase the *discipline* of conceptualizing and measuring the system usage construct while still enabling the generation of *diverse* conceptualizations of the construct.

This thesis addresses this gap by answering the following research question: *What principles can be used to conceptualize and measure system usage in an appropriate way for a given theoretical context?* To answer this question, my dissertation undertakes two tasks: (1) it advances an approach for conceptualizing and measuring system usage, and (2) it reports on empirical tests of the degree to which measures of system usage that are selected according to the proposed approach provide more explanatory power and lead to more coherent results in specific theoretical contexts than other measures of system usage.

The thesis contributes by: (1) clarifying the nature of system usage, (2) providing an explicit set of steps and principles that researchers can use to select or evaluate measures of system usage for a given theoretical context, (3) providing validated measures of system usage for specific theoretical contexts, and, more generally, (4) demonstrating how constructs in IS research can be conceptualized and measured in a diverse yet disciplined way. In terms of practical contributions, the approach advanced in this thesis can be tailored by organizations to help them select metrics of system usage that can predict and explain important downstream outcomes such as individual, workgroup, and organizational performance.

This chapter summarizes the dissertation. The chapter is structured as follows. Section 1.2 presents the scope of the investigation. Section 1.3 forwards a high-level framework for understanding how new perspectives of constructs can be generated. Sections 1.4 and 1.5 build upon this framework to advance the proposed approach for conceptualizing and measuring system usage. Section 1.6 describes three empirical studies that were carried out to test the usefulness of the proposed approach. Section 1.7 summarizes the chapter.

1.2 Scope of the Inquiry

Developing new perspectives on the system usage construct requires answering two questions:

- what is system usage and what can it be?
- what is the "system usage construct" and what can it be?

Both questions are essentially philosophical. The first is an ontological question, as it relates to the nature of a phenomenon in the world. The second is an epistemological question, as it relates to the nature of knowledge about a phenomenon in the world. Consequently, I propose that researchers could follow two general approaches to structure an inquiry into new perspectives on the system usage construct. First, researchers could examine system usage within *one* ontological and epistemological position. The aim would be to examine the possibility of different perspectives within one such meta-theoretical position. There have been very few examples of such research in IS. One example is Sabherwal and Robey's (1995) study in which they investigated the nature of IS developments projects from one set of ontological and epistemological assumptions but two forms of theory, "variance" theory and "process" theory.

The second approach would be to examine system usage from *multiple* ontological and epistemological assumptions. This would involve a multi- or meta-paradigmatic inquiry (Lewis and Grimes 1999). Such inquiries seek to cultivate diverse views of constructs by illuminating their various meanings across different ontological and/or epistemological positions (Lewis and Kelemen 2002). Only a few such studies have been undertaken in IS (Hirschheim et al. 1995; Jasperson et al. 2002; Mingers 2001; Trauth and Jessup 2000). For example, Jasperson et al. (2002, p. 427) describe how: "Power is…a complex phenomenon …[and] a metaparadigmatic approach can help authors understand, delimit, and carefully describe the conceptualization of power that they are adopting when studying IT…. [and] help surface anomalies and paradoxes."

I adopt the first of these approaches in this thesis. I do so because multi- or meta-paradigmatic inquiries are so expansive that they are arguably more suited to being carried out by a research team over a long-term research program (Jones 2004; Petter and Gallivan 2004). Arguably, a multi- or meta-paradigmatic approach will also be more effective if one has carefully investigated a phenomenon within each ontological and epistemological perspective first.

Adopting one set of ontological and epistemological assumptions alone is not sufficient to scope the thesis. Specifically, I restrict my investigation according to the following principles, which I define and discuss in turn below:

- *Meta-theoretical assumptions*: Critical realist
- *Research target*: Explanation
- *Form of theory*: Variance theory

1.2.1 Meta-Theoretical Assumptions: Critical Realist

This thesis adopts critical realist assumptions.[2] Critical realism is a meta-theoretical position that holds *realist* ontological assumptions and *relativist* epistemological assumptions (Archer et al. 1998; Bhaskar 1979). In other words, critical realists assume that all natural and social phenomena are part of the one "real" world, but, following constructivists, assume that the "true" nature of the world is unknowable and that all human knowledge of the world is inherently partial, fallible, and socially constructed (Hanson 1958; Kaplan 1998; Kuhn 1996).[3]

Are the meta-theoretical assumptions of critical realism accepted in the philosophy of science? This is hard to answer, because as McKelvey (2002) notes, "philosophers never seem to agree exactly on anything"(p. 757). Nonetheless, some do accept its assumptions. For example, Searle (1995) based his philosophy of social science on the same meta-theoretical assumptions. Likewise, Schwandt (1997) suggests that critical realism is a type of post-empiricism and he argues that the assumptions of post-empiricism are "roughly equivalent to the contemporary understanding of the philosophy of science" (p. 119).

Are the meta-theoretical assumptions of critical realism accepted in research practice? Although not all researchers agree on the merits of these assumptions (Klein 2004; Mingers 2004a; Monod 2004), some have explicitly acknowledged the importance of critical research assumptions in IS (Weber 1997), organization science (Azevedo 2002; McKelvey 2002), and, more broadly, in qualitative research in general (Miles and Huberman 1994). Moreover, there is significant evidence to suggest that critical realism has long been used *implicitly* in IS and organizational research. For example, it is the position that underpinned Cook and Campbell's

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[2] Other labels for critical realism are transcendental realism, evolutionary critical realism, constructive realism, and hypothetical realism (Archer et al. 1998; Bhaskar 1979; Bhaskar 1989; Brewer and Collins 1981; Messick 1989).
[3] *Epistemic* relativity does not imply *judgmental* relativity, the view that all judgments are equally valid (Mingers 2004b). Cook and Campbell (1979), for example, utilize evolutionary principles to explain why good research ideas are selected while others discarded over time.

(1979) classic work on research validity. Donald Campbell was a vigorous proponent of critical realism, and his research with many co-authors (Campbell and Fiske 1959; Cook and Campbell 1979; Webb et al. 2000) had a strong influence on notions of methods, constructs, measurement, and validity in both the quantitative and qualitative behavioral sciences (Azevedo 2002; Bickman 2000; Brewer and Collins 1981; Evans 1999; Messick 1989; Yin 1994). Consequently, there are likely many instances in IS and organizational research in which a practicing researcher is using principles derived from a critical realist perspective, such as Cook and Campbell's (1979) validity typology, without acknowledging that critical realist principles are being used (Moldoveanu and Baum 2002).

In short, critical realist assumptions appear to be an accepted set of assumptions in the philosophy and practice of social science. Certainly, critical realism is not the only meta-theoretical position, nor do all social scientists agree with it. Other meta-theoretical positions could be adopted, and a multi- or meta-paradigmatic inquiry would be a very useful way to understand the benefits of each position (Lewis and Kelemen 2002). Nonetheless, I submit that critical realism is sufficiently well accepted position for me to utilize it in this thesis.

1.2.2 Research Target: Explanation

Social science generally targets one or more of the following goals (Rosenberg 1995):

- to explain relationships among research constructs
- to understand the meaning and significance of people's actions and beliefs
- to emancipate individuals from domination, deceit, or delusion

In IS, researchers often associate these goals with different "paradigms," namely positivist, interpretive, and critical theory paradigms respectively (Orlikowski and Baroudi 1991).

In this thesis, I adopt a target of "explanation." I search for perspectives of system usage that will improve *explanations* of relationships between system usage and other phenomena, such as user performance. I do not imply that this goal is superior to the other two goals. In fact, there has been a series of important interpretive and critical theory studies of system usage over the last decade (Boudreau and Robey 2005; Ngwenyama and Lee 1997; Orlikowski 1996; Vaast and Walsham 2005). I merely submit that a thorough investigation of system usage from the perspective of achieving explanations is useful because it can directly assist researchers who share this goal, and it can indirectly assist those who strive for other goals by laying the groundwork for a later multi- or meta-paradigmatic inquiry (Lewis and Kelemen 2002).

Adopting a goal of *explanation* entails a particular view of constructs. If my goal was *understanding* or *emancipation*, I would focus on system usage as a "first-level construct," that is, it would refer to the concept(s) that people employ to understand their own use of systems (Lee 1991; Schutz 1962). However, because my goal is *explanation*, I focus on system usage as a "second-level" construct, that is, it refers to a concept that researchers "construct" to explain phenomena associated with people employing systems in reality (Lee 1991; Schutz 1962).

Philosophers of science disagree on what it means to "explain" (Kitcher 1998). Hovorka et al. (2003) outline five different types of explanation possible in social science:

- *Descriptive*: empirical/atheoretical knowledge regarding a phenomenon, e.g.:
    - X tends to occur in context Y, but not in context Z
- *Covering law*: a logical deduction involving a set of initial conditions and a law, e.g.:
    - X occurred, therefore Y occurred, according to law Z
- *Statistical relevance*: statistically significant relationships between facts, e.g.:
    - X and Y explain a significant amount of the variation in Z
- *Pragmatic*: an informative, context-specific answer to a why-question, e.g.:
    - X is a good explanation for Y because it explains why Y has a value of Z, not Z*
- *Functional*: Explanations that are defined in terms of desired end states. For example:
    - People do X to achieve Y

Of these types of explanation, the *descriptive* and *covering law* types appear unacceptable for this thesis because *descriptive* explanations do not offer the prospect of a very thorough explanation, while the covering law model is generally considered flawed (Hovorka et al. 2003). Of the remaining types of explanation, I use a combination of two (*statistical relevance* and *pragmatic*) that complement each other.

As outlined earlier, my thesis develops an approach for selecting measures of system usage that are appropriate for a given research context and I propose that when measures of system usage are selected according to this approach, "explanations" of the relationship between system usage and a downstream outcome will improve. I judge the merit of this proposition via both types of explanation:

- *Statistical relevance explanations:* I judge whether measures of system usage selected according to the proposed approach explain a significantly higher proportion of the variation in a downstream outcome than other measures of system usage
- *Pragmatic explanations:* I judge whether relationships estimated between system usage and downstream outcomes are more coherent (or theoretically interpretable) when usage measures are selected according to the proposed approach than when they are not.

In addition to these two types of explanation, another possible type is functionalist explanation. Markus (2004) recently pioneered such an explanation of system usage in the case of email use. For reasons of scope, I do not employ functionalist explanations in this thesis. However, future studies could extend the research in this thesis from such a perspective.

Given that I adopt a research target of *explanation*, some researchers might classify this research as "positivist." This would be misleading. Whether a critical realist study is "positivist" depends on how one defines "positivism." If positivism refers to pure "logical" positivism, then critical realist research does not support it because critical realism was created in direct opposition to logical positivism (Bhaskar 1979; Cook and Campbell 1979). Moreover, logical positivism has also long been rejected. As Suppe (1977) explained, since the late 1970's, all remaining philosophies of science have, literally, been post-positivist.

If pure positivism has long been rejected, why do so many researchers describe their work and the work of others as "positivist" (Dube and Pare 2003; Lee 1999; Myers et al. 2004; Straub et al. 2004b)? A simple answer is that working positivists are *not* pure positivists (Moldoveanu and Baum 2002). Weber (2003b) gives a good example of such a case:

> "My most vivid memory…was my surprise at the way positivism was being characterized…. I was a positivist, but I subscribed to none of the assumptions that my colleagues…alleged that I made…. [the] assumptions that are supposedly made by…adherents [of positivism] are outdated and misplaced ideas. Indeed, I believe positivists would dismiss some as ludicrous" (pp. iii, x).

Weber's case is useful to cite because he previously acknowledged that much of his research is based on critical realism (Weber 1997). Likewise, Moldoveanu and Baum (2002) suggest that most so-called positivists are likely critical realists or scientific realists.

In summary, it appears that many researchers who strive for a goal of explanation are labeled "positivists," yet they are not positivists, nor are they interpretivists or critical theorists. Arguably, many of these researchers are, in fact, critical realists.

Given this unfortunately ambiguous state of affairs, I stress that the research in this thesis is *not* pure positivist research. As Schwandt (1997) and Orlikowski and Baroudi (1991) note, critical realist research is more accurately labeled "post-positivist, post-empiricist" research. Even so, because many researchers who are incorrectly labeled by others as "positivists" are, arguably, critical realists, it might be reasonable to *informally* describe the work in this thesis as roughly conforming to the assumptions of "working positivists" in IS.

1.2.3 Form of Theory: Variance

Past research suggests that there are at least two well-accepted forms of theory: process and variance (Markus and Robey 1988; Mohr 1982; Poole et al. 2000). The constructs in such theories refer to different aspects of the world. In variance theory, research constructs refer to properties of phenomena, whereas in process theory, research constructs refer to events, sequences of events, or initial conditions (Poole et al. 2000). For example, in variance theory, the system usage construct could be conceived as a property that comes into being when a user employs a system in a task. A researcher could then measure values of this property over a period of time. In contrast, in process theory, system usage could be conceived as a sequence of events that occur when users employ a system in a task. A researcher could then measure system usage by measuring the occurrence of various sequences of these events over time.

In this thesis, I conceive of system usage in variance theory terms. I do so to scope the thesis and because it enables the thesis to directly contribute to a larger body of research (as most IS research utilizes variance theory) (Shaw and Jarvenpaa 1997). Past research has offered important process-oriented conceptions of usage (Hilbert and Redmiles 2000; Orlikowski 1996) and I believe that the approach advanced in this thesis could be extended in the future to support hybrid (i.e., process and variance) conceptions of system usage (Shaw and Jarvenpaa 1997).

1.3 Generating Perspectives of a Construct within Critical Realism

Although many so-called "positivist" researchers have arguably based their work on critical realist principles (Moldoveanu and Baum 2002), I agree with Mingers (2004b) that the full implications of a critical realist view have not been realized. I suggest that a critical realist view has two important implications for conceiving and measuring constructs in IS research:

- ***Principle of diversity****: All research constructs have multiple potential meanings.*
  This principle stems from epistemological relativism. Because constructs are social constructions, there can be legitimately different meanings of a construct at any time.
- ***Principle of constraint****: The number of potential meanings of a research construct is constrained by the nature of the real world phenomena it represents.*
  This principle stems from ontological realism. Because research constructs refer to real world phenomena, the number of potential meanings of a construct is necessarily limited because a construct must maintain a meaningful relation with its real world referent.

These principles suggest that in critical realism, the generation and investigation of constructs should embody a standard of disciplined diversity. Although other meta-theoretical assumptions might imply different standards, a standard of disciplined diversity is very useful. For example, in debating the merits of diversity in IS research, researchers have typically agreed that a standard of disciplined diversity is perhaps the most appropriate research ideal (Benbasat and Weber 1996; Landry and Banville 1992; Robey 1996; Weber 2003a).

Following past calls for principles to guide disciplined diversity (Robey 1996), this thesis advances a framework to help researchers consider how to achieve disciplined diversity when studying research constructs. Figure 1.2 illustrates the framework.[4] After outlining the framework, I use the principles in the framework to present an approach for conceptualizing and measuring the system usage construct in a way that realizes disciplined diversity.

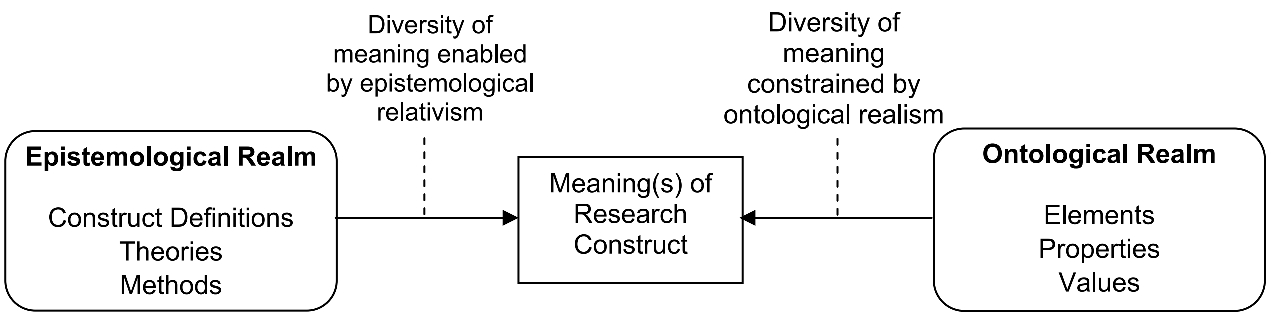


Figure 1.2: The Meaning of a Research Construct in Critical Realism

1.3.1 Epistemological Factors that Enable Diversity of Meaning

As Figure 1.2 shows, I build upon past research to suggest that three epistemological factors drive the diversity of meaning of research constructs: construct definitions, theories, and methods (Benbasat and Weber 1996; Robey 1996; Shadish et al. 2002).

The impact of each epistemological factor on the meaning of a construct is as follows:

1.3.1.1 Construct Definitions

Definitions establish the meaning of things (Antonelli 1998). Epistemological relativity allows alternative definitions and thus alternative meanings to co-exist. Different definitions can

[4] In this thesis, I only apply the principles of the framework to the system usage construct. Clearly, however, they could be applied to many other constructs in IS research, e.g., perceived usefulness (Davis 1989), task-technology fit (Goodhue 1995), effective IS security (Straub 1990), project escalation (Keil et al. 2000), and so on.

co-exist, for example, because: (1) they may be useful in different contexts, e.g., in simple vs. complex research contexts (an "instrumentalist" view), (2) they might occur within different theories (a "coherence" view), or (3) different researchers might simply name instances of the same phenomena differently (a "nominalist" view) (Monod 2004; Shadish et al. 2002).

1.3.1.2 Theories

Epistemological relativity assumes that the meaning of a construct depends on the theory in which it is embedded. For example, as Kuhn (1996) notes, identical terms can have vastly different meanings in different theoretical paradigms. Even within one paradigm, differences in theory can affect the meaning of constructs. For example, Cronbach and Meehl (1955) suggest that the meaning of a construct is determined partly by its internal structure (i.e., its make-up or composition) and partly by its relationships with the other constructs in its theoretical model. Likewise, Dubin (1978) argued that complex constructs should be decomposed into more specific subconstructs that are relevant in different theoretical models.

1.3.1.3 Methods

Researchers often cite physicists such as Heisenberg to stress that research methods influence construct measurements (Monod 2004; Weber 2003b). Campbell and Fiske (1959, p. 81) introduced this issue to the behavioral sciences, arguing that researchers cannot access true aspects (or "traits") of reality but can only measure "trait-method" units, i.e., "a union of a particular trait content with measurement procedures not specific to that content." Therefore, even when a construct has one definition and is located in one theory, the use of different methods for measuring the construct can vary the meaning of the construct measured.

1.3.2 Ontological Factors that Constrain Diversity of Meaning

As Figure 2 shows, I suggest that three ontological factors constrain the meaning of constructs: elements, properties, and values. Elements are "things" (e.g., a person) or parts of things (e.g., a person's mind); properties are attributes of elements (e.g., a person's intelligence); values define states of a property (e.g., a person's level of intelligence).[5] These are typically considered the three key constructs in variance-based ontology (Bunge 1977; Weber 2003c).[6]

The impact of these three ontological factors on the meaning of a construct is as follows:

1.3.2.1 Elements

A realist ontology implies that constructs have real world referents (Percival 2000). Typically, constructs refer to properties (Rossiter 2002). That is, researchers do not measure elements per se (e.g., a person), but rather properties of elements (e.g., intelligence) (Nunnally and Bernstein 1994). Elements constrain the number of meanings of a construct by limiting the types of properties a construct can refer to. For example, ontological theory suggests that individual elements (e.g., people) have intrinsic properties (e.g., individual cognition) while composite elements or "wholes" (e.g., collectives) also have emergent properties (e.g., collective cognition) (Bunge 1977; Weber 1997). This has an important implication for research because, as multilevel researchers have argued (Klein and Kozlowski 2000), if a researcher studies sets of individuals, s/he is necessarily constrained to investigating *intrinsic* properties, but when s/he is studying collectives, s/he can investigate intrinsic properties of individuals *as well as* emergent properties of collectives (Hofmann and Jones 2004; Morgeson and Hofmann 1999).

1.3.2.2 Properties

Critical realists employ tests of construct validity to assess whether a measure represents, fails to represent, or partially represents the intended property (Borsboom et al. 2004; Messick 1989). As no test can prove that a construct reflects a real world property, critical realists couple this test with others that judge whether measures yield coherent results (Cronbach and Meehl

[5] The term "value" is used here in its ontological sense (Bunge 1977), not in a moral or cultural sense.
[6] Some ontological theories are consistent with a "variable-oriented" view of the world (Bunge 1977), while others are more consistent with a "process-oriented" view of the world (Whitehead 1979).

1955; Embretson 1983; Westen and Rosenthal 2003). As Cook and Campbell (1979) note, by selecting constructs that pass such tests and discarding others, critical realists gradually build confidence over time that their constructs may approximate intended properties. Thus, for critical realists, the meaning of a construct is never fully relative, because to remain in use, its meaning must be tied, at least in part, to the nature of property it is intended to reflect (Messick 1989).

1.3.2.3 Values

The meaning of a construct is constrained by values because to reflect a real world property, measures of a construct must bear some similarity with the true value of the property. This has long been the concern of psychometricians who study measurement "scales" (Schwager 1991). Although the true scale of a property is unknowable (Nunnally and Bernstein 1994), tests have been developed to indicate when a construct scale might be misspecified (Viswanathan 2005). Multilevel researchers have also studied this issue to determine how to measure "collective constructs" when individuals within a collective have different values on a property. For example, researchers have developed ways to identify when a single value (e.g., an average) is an accurate reflection of the different values in the collective or when a pattern of values (i.e., a "configuration") would be a more accurate measure (Kozlowski and Klein 2000).

1.4 Generating New Perspectives on the System Usage Construct

A criticism of the preceding arguments might be that researchers have long known that the meaning of a construct is enabled and constrained by ontological and epistemological factors. Even if this criticism were correct, it does not follow that researchers have sufficiently *accounted* for these issues when studying constructs. If past research had accounted for these factors, this should be evident in extant conceptions of IS constructs. For example, we should be able to identify how past research has created diverse conceptions of constructs by systematically varying key epistemological factors while simultaneously ensuring that the constructs remain tied to the aspects of reality that they are intended to reflect.

I submit that there is little to no evidence that such epistemological and ontological factors have been accounted for in a systematic way in IS research. Certainly, in the case of system usage, there is a great diversity in conceptualizations and yet little-to-no justification that these conceptualizations actually reflect the intended aspects of system usage in reality. Although I am not the first to highlight this problem (Collopy 1996; Straub et al. 1995; Trice and Treacy 1986), extant research offers no systematic approach for accounting for these issues when studying system usage, or any other construct, in IS research. In other words, although critical realist assumptions have been used implicitly (often by so-called positivist researchers) for many years, the full implications of a critical realist view have not been acknowledged or addressed.

This thesis attempts to fill this gap in the literature by proposing an approach for conceptualizing and measuring the system usage construct. Figure 1.3 shows the approach. The rationale for advancing the approach is that critical realist assumptions imply that it is not possible to posit one "true" measure of system usage. However, I argue that it is beneficial to have a rigorous *approach* for conceptualizing and measuring system usage. Such an approach can offer a way to develop new perspectives of system usage that account for the epistemological and ontological factors discussed above and can provide a way to help improve disciplined diversity in the system usage literature (Benbasat and Weber, 1996, Robey 1996).

Table 1.2 explains which epistemological or ontological factors are addressed in each step of the proposed approach. As Table 1.2 shows, Chapters 2-5 of this thesis consist of individual papers that further explain each step of the approach, expand on the underlying principles, and demonstrate how each step can be carried out in the context of a given study.

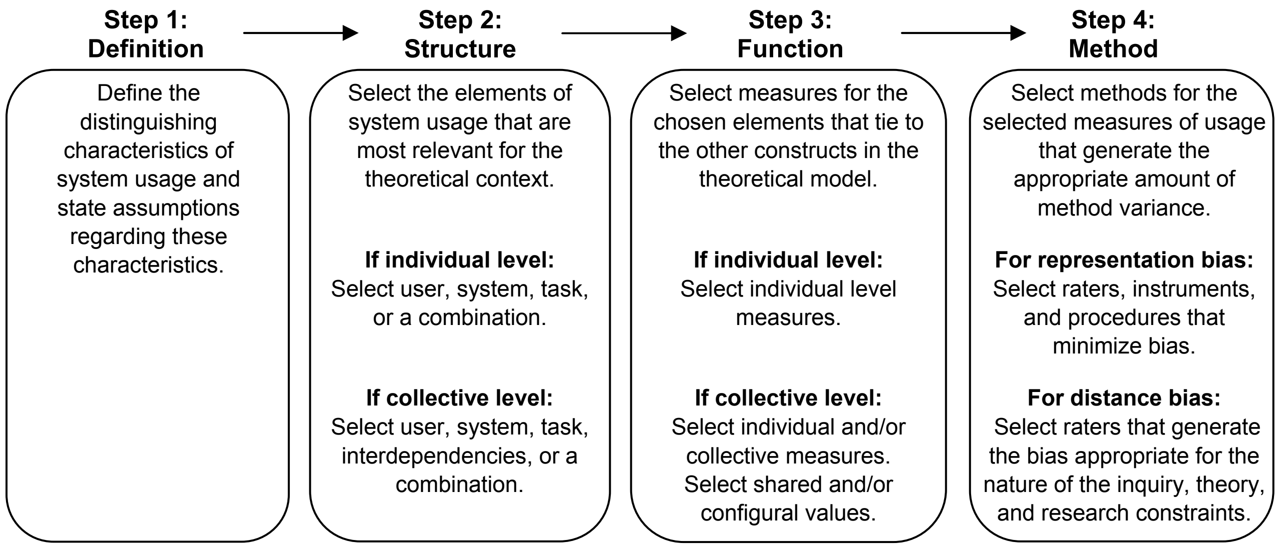


Figure 1.3: Approach for Conceptualizing and Measuring the System Usage Construct

Table 1.2: Scope of the Approach and the Organization of Thesis Chapters

		Steps of the Proposed Approach			
		1. Definition	**2. Structure**	**3. Function**	**4. Method**
Meta-theoretical factors that each step addresses (per Figure 1.2)	*Epistemological factor(s):*	Definitions	Theories	Theories	Methods
	Ontological factor(s):	Elements	Elements	Properties, Values	Properties
Chapter of the thesis in which each step is addressed	*Chapter:*	2	2, 3	2, 3, 4	5
	Description of how the chapter addresses the issue:	*Chapter 2* Initiates a new definition of system usage with associated assumptions.	*Chapter 2* Demonstrates how to select relevant elements of individual-level system usage for a given theory. *Chapter 3* Explains how the emergence of collective system usage depends on the presence of interdependencies.	*Chapter 2* Demonstrates how to select relevant properties of individual-level system usage for a given theory. *Chapter 3* Explains why configurations of values of collective system usage are important and how they can be studied. *Chapter 4* Demonstrates how to select relevant collective properties of system usage for a given theory.	*Chapter 5* Demonstrates how methods can be selected to appropriately account for method variance in measures of system usage for a given study.

1.5 Steps of the Proposed Approach

The following sections outline the steps of the proposed approach.

Step 1: Definition

The first step of the approach is to define the system usage construct and explicate its assumptions. Although other definitions could be constructed, I propose that system usage is an activity that involves three elements: (1) a user (i.e., the subject using the system), (2) a system (i.e., the object being used), and (3) a task (i.e., the function being performed). Support for the view that system usage involves these elements can be found widely in research on system usage in IS (Szajna 1993, DeSanctis and Poole 1994, Massetti and Zmud 1996), human computer interaction (John 2003), and computer supported cooperative work (Perry 2003). By drawing on these elements and recognizing that any system comprises many features (Griffith 1999), I define system usage as: *a user's employment of one or more features of a system to perform a task.*[7]

This definition has two implications. First, it provides a scope for what system usage can include. For example, it implies that system usage is related to, but is distinct from constructs such as IT adoption, information usage, faithful appropriation, and habits. A researcher may use system usage as a *proxy* for these constructs, but they are, nevertheless, different constructs.

Second, the definition refers to a broad "universe of content" (Cronbach 1971), only a *subset* of which may be relevant in any study. Individual and multilevel researchers suggest that constructs are defined partly by their internal *structure* and partly by their *functional* relationships with other constructs in a theoretical model (Cronbach and Meehl 1955; Morgeson and Hofmann

[7] My assumptions regarding each element of usage are as follows:

- A *user* is a social actor. This implies that users are individuals or collectives who are using a system to perform one or more aspects of their task(s) (Lamb and Kling 2003).
- A *system* is an artifact that provides representations of one or more task domains. This implies that the system offers features designed to support aspects of those task domains (DeSanctis and Poole 1994; Griffith 1999).
- A *task* is a goal directed activity performed by an individual or collective. This implies that task outputs can be assessed in terms of pre-defined task requirements (Zigurs and Buckland 1998).

1999). This implies a two-stage method for selecting system usage measures: (1) selecting relevant elements of usage (i.e., its *structure*), and (2) selecting measures of these elements that tie to the other constructs in a theoretical model (i.e., its *function*). These are the next two steps.

Step 2: Structure

This step involves selecting the relevant element(s) of system usage for a given study. It is important to recognize that the elements of system usage vary depending on the level of analysis. As noted above, I assume that "user" in the proposed definition could be an individual or a collective (e.g., a group or firm). The difference, according to multilevel theorists, is that collective phenomena emerge as a result of *interdependencies* among a collective's members (Morgeson and Hofmann 1999). This suggests that individual system usage comprises the elements in the definition (user, system, and task), whereas collective system usage comprises not only the sum of these elements for each individual in the collective, but also the interdependencies among individual users during use. In other words, collective usage is "more than the sum of its parts." Table 1.3 illustrates this reasoning. Models 1-3 show conditions in which both collective and individual usage exist, while Model 4 shows a condition in which only individual usage exists.

Once the possible elements of system usage are known, a researcher must select the *relevant* elements for a given study. As Table 1.4 shows, one could use measures of varying degrees of richness to capture this activity. Lean measures would attempt to capture the entire spectrum of usage activity in an omnibus measure such as use/non-use, duration of use, or extent of use. Although convenient, lean measures are not precise as they do not refer to the elements of usage that may be most relevant in a specific study context (Collopy 1996). In contrast to lean measures, rich measures step decisively into the nature of the usage activity. For example, as Table 1.4 shows, some researchers may be more interested in the extent to which a *system* is used, without capturing much of the user or task elements. Others may wish to add the *user* context by

Table 1.3: The Nature of Individual and Collective System Usage

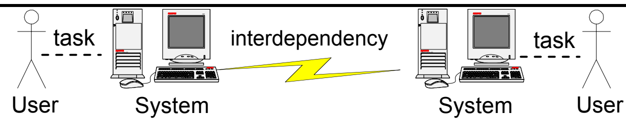
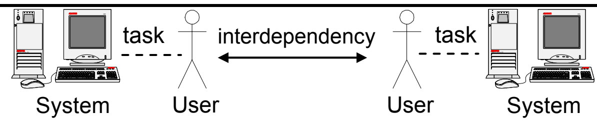
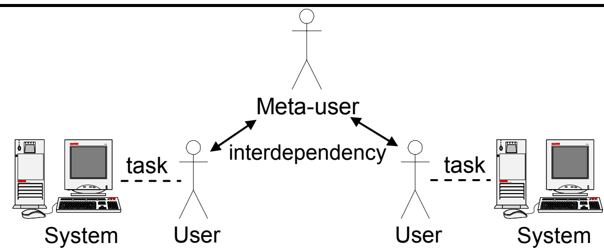
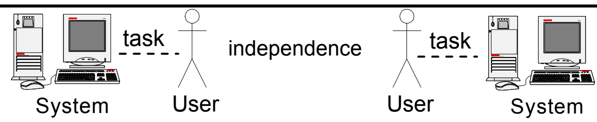
Model	Nature of Systems Usage Among Individuals	Type of Interdependency	Existence of Construct
1		Interdependency between users via their IT	Both individual and collective system usage exist
2		Interdependency between users who use IT	
3		Indirect interdependency between users who use IT (mediated by a "meta-user" e.g., a manager)	
4		No direct or indirect interdependency exists	Individual system usage exists (but collective system usage does not)

Table 1.4: Rich and Lean Measures of System Usage at an Individual Level of Analysis

Richness of measures	1. Very Lean	2. Lean	3. Somewhat Rich *(IS)*	4. Rich *(IS, User)*	5. Rich *(IS, Task)*	6. Very Rich *(IS, User, Task)*
Type	Presence of use	Extent of use (omnibus)	Extent to which the system is used	Extent to which the user employs the system	Extent to which the system is used to carry out the task	Extent to which the user employs the system to carry out the task
Elements measured*	**Usage**	**Usage**	**Usage** System User Task	**Usage** System User Task	**Usage** System User Task	**Usage** System User Task
Example properties examined in past literature	Use/ Non-use	Duration Extent of use	Breadth of use (number of features)	Cognitive absorption	Variety of use (number of subtasks)	None to date (Difficult to capture via a reflective construct)

measuring the degree to which a user employs a system and/or add the *task* context by measuring the degree to which an IS is employed in the task. At a collective level, researchers may wish to include user *interdependencies* during use. None of these approaches is inherently better. Rather, as Figure 1.3 states, researchers must identify the elements that best fit their theoretical context.

Step 3: Function

The Function step requires a researcher to examine his/her selected elements of system usage and select one or more properties of these elements that tie to the other constructs in his/her theory. As Table 1.4 shows, each element or combination of elements of usage can be associated with one or more properties. For example, Model 4 in Table 1.4 shows a case in which the researcher is interested in the *user* and *system* elements of usage and the table lists "cognitive absorption" as a property associated with these elements. Of course, "cognitive absorption" may or may not be relevant in a given study. The Function step simply requires a researcher to determine the relevant properties for his/her study's theoretical context.

Once a researcher has chosen the relevant properties, s/he must select *measures* to reflect them. The aim is to obtain content-valid, contextualized measures of usage, that is, measures that: (1) reflect the intended properties completely (i.e., no errors of omission) and (2) do not also reflect other unintended properties (i.e., no errors of inclusion) (Messick 1989).

A potential criticism of this step is that it could lead to a proliferation of measures that all refer to "system usage", but refer to different content, thus hindering cumulative progress. This criticism would be mistaken, because the different measures would still measure usage, just different *subsets* of the usage activity. I show this in Figure 1.4 by using subscripts to denote subtypes of usage relevant in different contexts. By encouraging researchers to justify the properties and measures they select based on theory, the approach should support cumulative progress.

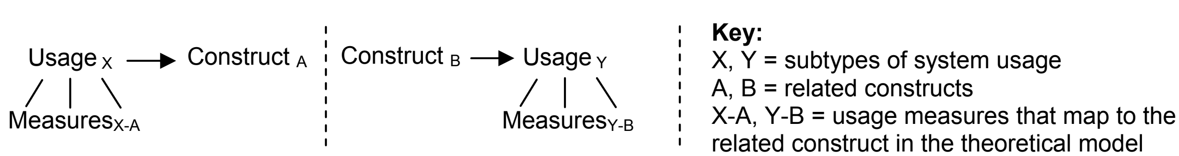


Figure 1.4: Contextualizing the System Usage Construct

For researchers studying collective system usage, the Function step has one additional requirement: to select an approach for measuring the "values" of collective system usage. On this issue, multilevel theorists differentiate between three types of collective properties (Kozlowski and Klein 2000):

- *Global properties*: properties that exist at the collective level with no counterparts at lower levels of analysis (e.g., group size)
- *Shared properties*: properties that emerge at the collective level due to the individual members having similar (i.e., "shared") values of the property
- *Configural properties*: properties that emerge at the collective level due to the individual members having a distinct pattern (i.e., "configuration") of values of the property.

Researchers can use different types of constructs to reflect each of these properties, with each construct utilizing a different approach for representing values (Kozlowski and Klein 2000):

- *Global construct*: measured by assigning a score that directly reflects the value of the global property (e.g., a number that reflects the size of a group)
- *Shared construct*: measured by assigning a score that reflects the common value of the property among individual members (e.g., a mean average)
- *Configural construct*: measured by assigning a score that reflects the pattern of levels of the property among individual members (e.g., variance, min-max, distance)

In this thesis, I argue that collective system usage is not a global property. This is because collectives (e.g., groups and organizations) can only "use" systems through the actions of their individual members. As a result, I suggest that researchers must measure the collective system usage construct either as a shared construct or a configural construct (Morgeson and Hofmann 1999). Various quantitative and qualitative techniques can be used for doing so. The proposed approach merely requires that researchers measure collective system usage in a way that enables them to: (1) faithfully reflect the true values of the intended properties, whether shared or configural (i.e., the ontological requirement) (Hofmann 2002), and (2) support a coherent theoretical relationship with the other constructs in the theoretical model (i.e., the epistemological requirement) (Doty et al. 1993).

Step 4: Method

The final step is to select one or more methods for obtaining the selected measures. To extend past research (Collopy 1996; Straub et al. 1995), Figure 1.5 illustrates two sources of method variance, distance bias and representation bias, that I propose researchers must consider when selecting methods.

Distance bias refers to an inaccuracy in measurement that occurs due to a rater's lack of access (or "distance") to the property. If a rater does not have access to the focal construct, their measures may be restricted to a *proxy* (i.e., another construct). A common example of distance bias occurs when a rater scores a property that belongs to a thing other than him/herself, e.g., an IS or another person. In this case, the individual may lack knowledge of the property being rated. Representation bias, in contrast, refers to an inaccuracy in measurement that is not due to the rater's lack of knowledge, but rather due to the rater not providing (i.e., "misrepresenting") his/her best estimate of the measure. Many reasons for representation bias are known, e.g., cognitive dissonance, social desirability, and yea/nay-saying (Podsakoff et al. 2003).

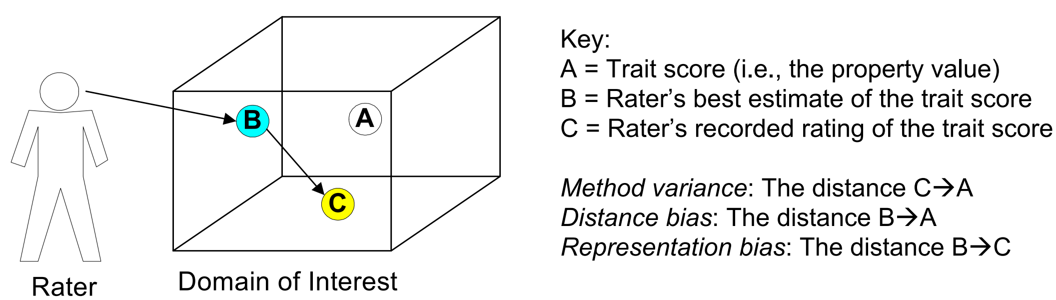


Figure 1.5: Two Sources of Method Variance: Distance Bias and Representation Bias[8]

[8] The term "trait score" in measurement theory is equivalent to the term "property value" in ontology, i.e., it refers to the true score/value of the real world trait/property. In this chapter, I use the terms "property" and "value" because I align the arguments with ontological theory (Bunge 1977; Weber 2003c). In Chapter 5, I use the terms "trait" and "trait score" because the arguments in that chapter are aligned with measurement theory (Campbell and Fiske, 1959).

The proposed approach suggests that researchers account for these two sources of bias differently. Specifically, the approach suggests that representation bias is unwanted and that researchers should attempt to minimize it wherever possible. For example, researchers might use multiple methods to triangulate on the scores from different raters (Campbell and Fiske 1959), under an assumption that "true lies at the intersection of independent lies" (Levins 1966, p. 423).

Whether distance bias is unwanted depends on the aims of a study. This is because a rating that reflects distance bias is not a true rating of the intended property, but it may be a valid rating of a relevant, albeit unintended property. For example, assume that a researcher asks a manager to complete a questionnaire regarding her subordinate's performance. If the manager has a mistaken perception of her subordinate's performance, her rating would be a biased rating of actual performance, but it would be a valid rating of her perception of the employee's performance, and this perception could have a real effect on how s/he interacts with the employee at work, i.e., it is a relevant property in the domain. Of course, even though it is a relevant trait in the domain, it may not be relevant for a particular researcher's study. To provide guidance, the proposed approach suggests that unintended properties measured by distance bias are irrelevant (and, thus, distance bias should be minimized) only if *all* of the following hold:

- the inquiry is confirmatory (i.e., aiming to confirm or test one model)
- the theory specifies a rater (e.g., perceived usefulness is "perceived" by the user)
- the desired rater is available (i.e., the rater specified in the theory can be used)

Where any of these three conditions do not hold, the approach suggests that distance bias is important, valuable, and should be investigated. Importantly, this could occur in many studies of system usage because:

- researchers need not necessarily adopt a confirmatory stance (Platt 1964)
- the system usage construct is rarely defined in terms of one particular rater
- practical contingencies in research often preclude a researcher from using the desired rater for every construct in a theoretical model

Overall, by highlighting the difference between distance bias and representation bias, and providing guidance for dealing with them in an appropriate manner in a given study, the proposed approach helps ensure that a researcher will obtain more accurate measures (i.e., the ontological requirement) and more relevant measures (i.e., the epistemological requirement) in a given study.

1.6 Research Design

The proposed approach, like any approach, cannot be tested for its "truth," only its usefulness (Cook and Campbell 1979). I test its usefulness by empirically investigating whether measures of system usage that are selected according to the approach yield better explanations than other measures of usage in specific theoretical models. The theoretical context I use for this empirical investigation is the relationship between system usage and user task performance, which past researchers have suggested is a context in which better measures of system usage are needed (Chin and Marcolin, 2001, DeLone and McLean, 2003).

1.6.1 Empirical Tests

Table 1.5 describes the empirical tests. As Table 1.5 shows, Chapters 2, 4, and 5 use data from free simulation experiments. Each experiment examines one or more steps of the proposed approach. Chapter 3 is a conceptual paper with no empirical test.

An experimental approach was appropriate because this is the first test of the proposed approach and experiments generally provide strong empirical tests by providing greater control of external influences and rival, confounding explanations (Calder et al. 1981; Greenberg 1987). A limitation of experiments is their generalizability. To minimize this limitation, both experiments examine a common task in practice: analysts' use of spreadsheets for financial analysis (Springer and Borthick 2004). This is a useful context for studying system usage because spreadsheets are among the most common end-user applications in practice (Carlsson 1988; Panko 1998).

Table 1.5: Empirical Tests*

	Description of Empirical Test					
Chapter	**Step examined**	**Design***	**Level of analysis**	**Methods**	**Subjects**	**Task/IT**
Ch. 2	Structure, Function	Free simulation	Individual level	Usage measured by self-report questionnaire, performance measured by an independent measure	Accounting students in a principles of accounting course in a southern US university	Use Excel to build a spreadsheet model to recommend a method of financing an asset purchase
Ch. 4	Structure, Function	Free simulation	Individual and collective level	Usage and performance measured by self-report questionnaire		
Ch. 5	Method	Free simulation	Individual level	Usage and performance measured by self-report questionnaire and independent measures		

* A free simulation is an experimental design in which the values of the independent variable (e.g., usage) are allowed to vary freely over their natural range (Fromkin and Streufert 1976). This gives an insight into the relationship between the independent and dependent variable and the range over which it occurs.

1.6.2 Data Analysis

Table 1.6 summarizes the data analysis approach. I briefly explain each test below.

In Chapter 2, I draw on theories of performance (Campbell 1990; March 1991) to propose that in the context of studying the relationship between system usage and task performance in cognitively engaging tasks, each element of usage (i.e., user, task, and system) is relevant for explaining the relationship between system usage and task performance. Building on past studies (Agarwal and Karahanna 2000; DeSanctis and Poole 1994; Wand and Weber 1995), I then propose two measures of these elements, cognitive absorption and deep structure usage. I then empirically test whether these measures of system usage explain the relationship between system usage and task performance more effectively than a measure of system usage that would not be recommended by the proposed approach in this context (i.e., minutes of use).

Chapter 4 tests whether the results from Chapter 2 hold in a multilevel context. Drawing on theories of groups (Lindenberg 1997), I propose that in this theoretical context, user interdependencies-in-use are a relevant element of collective system usage in addition to the

Table 1.6: Data Analysis Approach

Chapter	Sample	Statistical Method	Analytical Test*
Ch. 2	171	Partial Least Squares (PLS)	Tests whether the relationship between usage and performance is stronger (in terms of R^2) and more interpretable (in terms of direction) when usage is modeled via a measure that is tailored to the theoretical context rather than a measure that omits one/more elements (i.e., user, system, and/or task) that are proposed to be relevant in this theoretical context.
Ch. 4	173 groups 633 individuals	Regression, Hierarchical Linear Modeling	Tests whether the relationship between usage and performance at the collective level of analysis and across levels of analysis (from the collective level to the individual level) is stronger (in terms of R^2) when collective usage is modeled via a measure that includes interdependencies-in-use rather than a measure that omits this element (i.e., that only measures the user, task, and system elements).
Ch. 5	171 representation bias; 45 distance bias	PLS	Tests multiple models of the usage→performance relationship using data from different methods and examines whether data obtained from the same method exhibit common methods bias (a form of representation bias) and whether the strength of the usage→performance relationship is significantly influenced by the degree of distance bias and representation bias in the data, i.e., β (with distance bias) $\neq \beta$ (without distance bias); β (with representation bias) $\neq \beta$ (without representation bias).

* This table lists the primary analytical tests. Each chapter includes additional secondary tests to provide a complete analysis of the data.

user, task, and system elements. I then draw on past studies (Crowston 1997; Karsten 2003) to propose two relevant measures of interdependencies-in-use: coordination-in-use and collaboration-in-use. I then empirically test whether a measure of collective usage that includes these additional measures yields a stronger explanation of the relationship between system usage and task performance than a measure of collective system usage that does not include these measures.

Chapter 5 tests the impact of the two sources of method variance (distance bias and representation bias) on the relationship between system usage and task performance at the individual level of analysis. Utilizing the same measures of system usage and performance as in Chapter 2, I use two methods to collect data on each measure: self-reports and independent ratings. Self-reports are acquired via participants' responses to validated instruments in a post-test questionnaire. Independent ratings are obtained via independent ratings of participants' use of their spreadsheet program (MS Excel) and their final task performance. To enable an accurate independent coding of system usage, Screen Cam software video records are examined for a subsample of 46 user sessions. As Table 1.6 outlines, I test the impact of method variance by running multiple models of the usage→performance relationship that include different degrees of distance bias and representation bias and I statistically identify the degree of methods bias and distance bias within and across models.

1.7 Conclusion

Although system usage has been studied in IS research for many years, there have been increasing calls to examine it more closely (Chin and Marcolin 2001; DeLone and McLean 2003). This thesis advances a new approach for conceptualizing and measuring system usage in an appropriate manner for a given study and provides empirical tests to demonstrate that the approach is both feasible and useful. Table 1.7 summarizes the intended contributions of the thesis.

Construct development is a key activity in any field. By bringing new perspectives to the system usage construct, my intention in this thesis is to create new opportunities for research on the nature of system usage, its antecedents, and its consequences. Given the heated debates surrounding this topic in journals such as the *Harvard Business Review*, a deeper understanding of system usage should enlighten those who study information systems and those who invest in and use systems in practice.

Table 1.7: Intended Contributions of the Thesis

Component of Thesis	Intended Contribution
Proposed approach	Provide an explicit set of steps and principles that researchers can use to select or evaluate measures of system usage for a given theoretical context. Provide an approach that practitioners can tailor to select metrics of system usage that enable them to explain how systems are used and explain how system usage is associated with downstream outcomes in practice. Instantiate a new approach for conceptualizing and measuring constructs in IS research that is consistent with critical realist assumptions.
Empirical tests	Demonstrate the usefulness of the approach by empirically identifying the degree to which explanations of theoretical models can be improved by: - Selecting elements, properties, and measures of system usage that are appropriate for a theoretical context - Selecting methods for measuring system usage that are appropriate for the nature of a study's inquiry, theory, and practical constraints Provide validated measures of individual and collective usage for a specific theoretical context.

Chapter 2

Reconceptualizing System Usage[9]

Abstract

Although DeLone-McLean and others insist that system usage is a key variable in information systems research, the system usage construct has received little theoretical scrutiny, has no accepted definition, and is measured by a diverse set of measures. In this article, we present a systematic approach for reconceptualizing the system usage construct. Comprised of two stages, definition and selection, the approach enables researchers to develop clear and valid measures of system usage for a given theoretical and substantive context. The definition stage requires that researchers define system usage and explicate its underlying assumptions. In the selection stage, we suggest that system usage be conceptualized in terms of its structure and function. The structure of system usage is tripartite, comprising a user, system, and task, and researchers need to justify which elements of usage are most relevant for their study. In terms of function, researchers should choose measures for each element (i.e., user, system, and/or task) that tie closely to the other constructs in the researcher's nomological network.

To provide evidence of the viability of the approach, we undertook an empirical investigation of the relationship between system usage and short-run task performance in cognitively engaging tasks. The results support the benefit of the proposed approach and suggest new directions for research into the nature of system usage, its antecedents, and its consequences.

Keywords: system usage, theoretical conceptualization, IS success

Acknowledgments: This paper has benefited from presentations at Georgia State University, the University of Georgia, and the University of Houston. We are particularly indebted to Dale Goodhue and Elena Karahanna for incisive comments.

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[9] Burton-Jones, A., and Straub, D. "Reconceptualizing System Usage," Working paper, Department of Computer Information Systems, Georgia State University, 2004.

2.1 Introduction

The system usage construct has played a central role in information systems (IS) research since the 1970's (Barkin and Dickson 1977). Many researchers have studied antecedents to usage and, over time, the field has progressed towards a general model of these antecedents (Venkatesh et al. 2003). Others have studied the impact of usage on individual performance. They report the link to be strongly positive (Doll and Torkzadeh 1998), weakly positive (Goodhue and Thompson 1995; Igbaria and Tan 1997), insignificant (Lucas and Spitler 1999), or negative (Pentland 1989; Szajna 1993). The usage construct itself, however, typically escapes scrutiny in such studies of antecedents and consequences. This has fuelled calls for closer examination of the usage construct (Chin and Marcolin 2001; DeLone and McLean 2003).

Like Melone's conceptual work on the user satisfaction construct in a (1990) issue of *Management Science*, the current study undertakes a theoretical assessment of individual system usage. Despite its centrality in IS research, the system usage construct has received scant theoretical treatment to date. Apart from Trice and Treacy's (1986) brief conceptualization and a short discussion by Seddon (1997), we are unaware of any in-depth, theoretical assessment of the construct. Without theoretical grounding, it is not surprising that past studies have arrived at mixed conclusions about the link between system usage and individual performance.

We begin with a review of how system usage has been <u>implicitly</u> conceptualized in four IS domains: IS acceptance, IS implementation, IS success, and IS-aided decision-making. Next, we shift to the lack of theory and lack of validation in prior usage studies. To remedy these problems, we advance a new, staged approach for reconceptualizing system usage, an approach that enables researchers to construct measures that are more contextualized, complete, and valid. We then empirically investigate the approach by conceptualizing system usage in terms of its link with individual task performance. While other conceptualizations could be developed, a mapping between system usage and performance is key, given mixed results in this area and disagreements about its conceptualization (DeLone and McLean 2003). In addition, the present work: (1) clarifies the richness of system usage, (2) demonstrates how different aspects of usage can be integrated to derive a more complete, contextualized construct, and (3) reports on an empirical test that validates the proposed approach for developing usage measures.

2.2 Implicit Conceptualizations of System Usage in Past Research

Few constructs in IS have had as long a history as system usage (DeLone and McLean, 1992, 2003). Figure 2.1 depicts the implicit conceptualizations of system usage in four research domains: IS success, IS acceptance, IS implementation, and IS for decision-making.

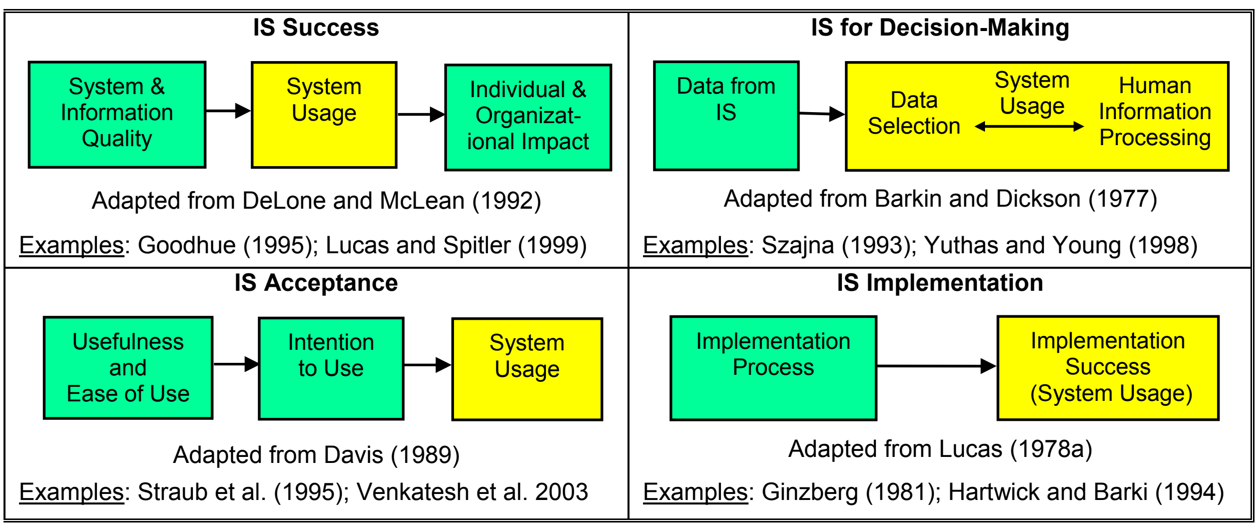


Figure 2.1: Past Conceptualizations of the System Usage Construct

In the *IS success* domain, researchers have measured usage as an independent variable (IV) or mediating variable leading to downstream impacts in order to determine how IT benefits individuals or organizations (DeLone and McLean 1992). In the *IS for decision making* domain, system usage is primarily a dependent variable (DV), as in Barkin and Dickson's (1977) model (see Figure 2.1). Researchers typically study IS characteristics that improve user decision-making. In the *IS acceptance* domain, researchers study system usage as a behavior determined by social and cognitive variables, with the goal of finding variables that explain most variance in usage. Theories employed to specify the range of antecedents include the theory of reasoned action, the theory of planned behavior, and social learning theory. Finally, system usage is a key DV in *IS implementation* research, that is, determining characteristics of IT implementations that lead to greater use of the final system. Although researchers sometimes choose measures specific to their research domains (e.g., use of information from IS in the *decision-making* domain, DeLone and McLean, 1992; or use of IS to support specific tasks in the *IS success* domain, Doll and Torkzadeh, 1998), researchers across these communities generally deploy similar usage measures. Long-standing measures include: features used, tasks supported, extent of use, use or non-use, heavy/light use, frequency of use, and duration (see Table 2.1).

Despite this long-standing investigation of system usage, studies of its relationship with other constructs often report weak effects. With system usage as a DV, researchers have carefully examined a large number of antecedents (Adams et al. 1992), but explained variance is in a middling range, averaging around 30% (Meister and Compeau 2002). With system usage as an IV, studies of its downstream effects (e.g., on performance) report mixed results. Although progress has been made at an organizational level (Devaraj and Kohli 2003), findings at an individual level report that system usage can increase (Doll and Torkzadeh 1998), decrease (Pentland 1989; Szajna 1993), or have no effect on performance (Lucas and Spitler 1999).

2.3 The Need to Reconceptualize System Usage

Why is there a need to reconceptualize system usage? A review of the literature prompts two concerns: no theory and poor-to-no validation. Our evidence draws upon 48 empirical studies of individual-level system usage (see Appendix 5A). The chief limitation in past

Table 2.1: The Diversity of System Usage Measures Employed in Past Research†

Broad Dimension	Individual Measures	Used as IV	Used as DV
System usage measured as the use of information from an IS			
Extent of use	Number of reports or searches requested	✓	✓
Nature of use	Types of reports requested, general versus specific use	✓	
Frequency of use	Frequency of report requests, number of times discuss information		✓
System usage measured as the use of an IS			
Method of use	Direct versus indirect		✓
Extent of use	Number of systems, sessions, displays, functions, or messages; user's report of whether they are a light/medium/heavy user.	✓	✓
Proportion of use	Percentage of times use the IS to perform a task		✓
Duration of use	Connect time, hours per week	✓	✓
Frequency of use	Number of times use system (periods are: daily, weekly, etc.)	✓	✓
Decision to use	Binary variable (use or not use)		✓
Voluntariness of use	Binary variable (voluntary or mandatory)		✓
Variety of use	Number of business tasks supported by the IS	✓	✓
Specificity of use	Specific versus general use		✓
Appropriateness of use	Appropriate versus inappropriate use	✓	✓
Dependence on use	Degree of dependence on use	✓	✓

† Developed from a sampling of 48 articles in major IS journals in the period 1977-2005 (see Appendix 5A).

conceptualizations of system usage has been the *atheoretical* manner in which usage measures have been chosen. While many researchers carefully use theory to choose antecedents to usage (e.g., the theory of reasoned action; Venkatesh et al. 2003), few discuss how theory informs their choice of usage measures. With the exception of early decision-making studies that drew on information processing theory (Barkin and Dickson 1977), we found no studies that expressed a strong *theoretical* basis for system usage, its appropriate empirical indicators, or its relationships with other constructs. The consequences of this dearth of theory can be seen in Table 2.1, which illustrates the diversity of usage measures in past research. In all, 14 broad measures are listed as well as many minor variants. In the presence of strong theory, diversity of measures is desirable (Campbell and Fiske 1959), but in its absence, an abundance of measures is problematical—it lures researchers into believing that there is no problem with measuring usage (Srinivasan 1985). We believe that the problems stemming from lack of theory are now coming to light, evident in the persistence of mixed results and lack of consensus on how to conceptualize system usage in IS success models (DeLone and McLean 2003).

In addition to this theoretical lacuna, our review found that there was almost no validation of the usage construct. Surprisingly, while user satisfaction (the complement of system usage in DeLone and McLean's model), has had extensive instrument development (Doll and Torkzadeh 1988) and validation (Chin and Newsted 1995), work on system usage has not. Most studies select one or two usage measures from the many available (see Appendix 5A). A minority of studies use three or more measures and factor-analyze them to arrive at a composite measure of usage (Igbaria et al. 1997). Even in these instances, however, measures of system usage are chosen for their appearance in past empirical studies rather than for theoretical reasons.

2.4 A Staged Approach for Reconceptualizing System Usage

We suggest that the lack of theory underlying measures of usage in past research and the lack of validation of such measures manifest deeper problems:

- There is no accepted *definition* of the system usage construct in the IS literature.
- There is no accepted approach for *selecting* the relevant content of usage for any given study.

To overcome these problems, this paper presents, for the first time, a systematic approach to enable researchers to reconceptualize system usage (Figure 2.2). By "reconceptualize," we mean that system usage is not a type of construct that can have a single conceptualization or measure. Unlike constructs that are strictly unidimensional, or multidimensional with specific, known dimensions, we believe that relevant measures and dimensions of system usage will vary across contexts. In this light, having diverse conceptualizations of usage (as in past research, per Figure 2.1) is *desirable*. What is needed is a way to make such conceptualizations explicit. Thus, while we do not believe that there can be a single accepted conceptualization of system usage, we believe that there is great value in having an accepted *approach* for systematically developing conceptualizations of usage and selecting usage measures in a theoretically rigorous way. This paper presents such an approach (per Figure 2.2). We outline the stages of the approach in turn.

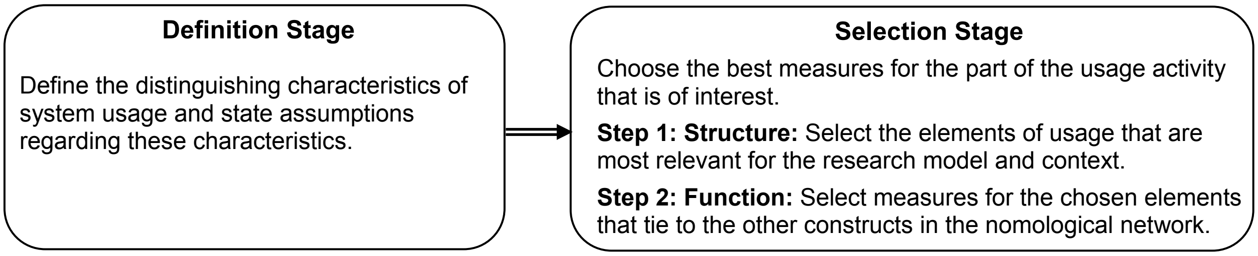


Figure 2.2: Staged Approach for Defining System Usage and Selecting Usage Measures

2.4.1 Defining System Usage

To conceptualize system usage, one must define it. Surprisingly, the IS field has no generally accepted definition of system usage. Granting that other definitions could be constructed, we propose that system usage is an activity that involves three elements: (1) a user, i.e., the subject using the IS, (2) a system, i.e., the object being used, and (3) a task, i.e., the function being performed.[10] Drawing on each element and recognizing that any IS comprises many features (Griffith 1999), we define individual-level system usage as: *an individual user's employment of one or more features of a system to perform a task*. This definition has two implications. First, it distinguishes system usage from related, but distinct constructs. For example, it suggests that *system* usage is distinct from *information* usage. In contrast to DeLone and McLean's (1992) definition of system usage and others (Table 2.1), we suggest that information usage is a useful construct, but it is not identical to system usage. System usage is also distinct from a user's *decision* to use or subsequent *dependence* on an IS and from user *adoption*. Even though many IT acceptance researchers have utilized such constructs as proxies for system usage (Table 2.1), one must not confuse a *proxy* for a construct. Finally, system usage

[10] Any definition of usage must rely on assumptions. Our assumptions about the elements of usage are as follows:
- A *user* is an individual person who employs an IS in a task. This implies that although users are social actors (Lamb and Kling 2003), we assume that it is possible to study user behavior at a purely individual level.
- An *IS* is an artifact that provides representations of one or more task domains. This implies that IS provide features that are designed to support functions in those task domain(s) (Zigurs and Buckland 1998).
- A *task* is a goal directed activity performed by a user. This implies that task outputs can be assessed in terms of pre-defined task requirements (March 1991).

is not an *evaluation*. Evaluations such as quality of use (Auer 1998) and appropriate use (Chin et al. 1997) are useful constructs, but they do not measure system usage; instead, they measure the degree to which one's usage *corresponds* with another construct such as expected use or system "spirit" (Chin et al. 1997). Our definition implies that if one is to measure system usage itself, one must quantify it, not evaluate it.

The second implication of the definition is that it clarifies the content of system usage. Because system usage is a complex *activity* involving a user, IS, and task over time, it has a broad "universe of content" (Cronbach 1971). As Table 2.2 shows, one could use lean or rich measures to measure this content. Lean measures would attempt to capture the *entire* content of the activity in an omnibus measure such as use/non-use, duration of use, or "extent" of use. Although such lean measures can be convenient, they are unfortunately inexact because they do not refer to the aspect of usage that may be most relevant in a specific context and it may not be clear to a respondent what part of the usage activity is actually being measured. Dubin (1978) called omnibus measures of complex constructs "summative units" and warned against their employ. In contrast to lean measures, rich measures incorporate the nature of the usage activity (see Table 2.2). To employ rich measures, one must have a way to select relevant content; this leads to the second stage: *selection*.

2.4.2 Selecting Content Valid, Contextualized Measures: A Two-Step Approach

As Table 2.2 shows, system usage always involves a system, but researchers can use rich measures to capture more or less of its use in a particular context. Some researchers may only be interested in the extent to which the *system* is used, without capturing much of the user or task context (Table 2.2, model 3). Others may wish to include the *user* context by measuring the degree to which a user employs a system (Table 2.2, model 4) or include the *task* context by measuring the degree to which the system is employed in the task (Table 2.2, model 5). None of

Table 2.2: Rich and Lean Measures of System Usage

Richness of measures	1. Very Lean	2. Lean	3. Somewhat Rich *(IS)*	4. Rich *(IS, User)*	5. Rich *(IS, Task)*	6. Very Rich *(IS, User, Task)*
Type	Presence of use	Extent of use (omnibus)	Extent to which the system is used	Extent to which the user employs the system	Extent to which the system is used to carry out the task	Extent to which the user employs the system to carry out the task
Domain of content measured*	**Usage**	**Usage**	**Usage** System User Task	**Usage** System User Task	**Usage** System User Task	**Usage** System User Task
Example	Use/ Non-use	Duration Extent of use	Breadth of use (number of features)	Cognitive absorption	Variety of use (number of subtasks)	None to date (Difficult to capture via a reflective construct)
Reference	Alavi and Henderson (1981)	Venkatesh and Davis (2000)	Saga and Zmud (1994)	Agarwal and Karahanna (2000)	Igbaria et al. (1997)	

* Lean measures reflect usage alone; rich measures reflect its nature, involving the system, user, and/or task.

these approaches is inherently superior. Rather, researchers must choose appropriate measures for their objective, theory, and methods. Methods can, at times, be very restrictive. For example, in some instances, a researcher may be restricted to a very lean measure (per Table 2.2, model 1), simply due to the practical realities of data collection. In other instances, a researcher may wish to use a very rich measure to capture all three elements of usage (per Table 2.2, model 6). Although it is feasible theoretically to construct a single measure that captures each element of usage (i.e., system, user, and task), it is difficult methodologically to do so because the richness of the activity being measured makes it difficult to construct and cognitively difficult to respond to such a measure in practice. Here, a methodological compromise is to combine measures for the system, user, and task aspects of usage and create an aggregate higher-order construct to capture the entire activity (Law et al. 1998). We proffer an example of this strategy later in the paper.

As Figure 2.2 shows, this reasoning leads us to suggest a method for selecting measures of usage in future research. In other words, system usage can be attributed with a precise definition, but the definition refers to a broad range of content, only a *subset* of which will be relevant in a specific study. As different subsets will be relevant in different studies, one cannot create a single *measure* of usage, but one can define an *approach* for creating usage measures in such a way that the measures capture the most relevant content for a specific context (i.e., are content-valid, yet contextualized). To define such an approach, we draw on Cronbach and Meehl's (1955) classic description of construct validity. According to Cronbach and Meehl, a construct's meaning is defined partly by its internal structure or make-up and partly by the other constructs in its nomological network. This implies a two-step method for selecting measures of system usage (per Figure 2.2):

1. **Structure**: select the *elements* of usage (i.e., the user, system, and/or task) that are most relevant for the research model and context,
2. **Function**: for the selected elements of usage, select *measures* of these elements that tie closely to the other construct(s) in the proposed nomological network.

A potential criticism of this two-step method is that it could lead to a proliferation of measures that all refer to "system usage", but refer to *different* content, thus hindering cumulative progress. This criticism would be mistaken, because the different measures would still measure usage, just different *subsets* of the usage activity. The distinction between the two-step method and existing practice is that the method should enable researchers to select and validate their usage measures from a *theoretical* basis. We show this in Figure 2.3 by using subscripts to denote subtypes of usage appropriate for different contexts. By clarifying the subset of usage being measured, and theoretically justifying one's measures, cumulative progress will improve.

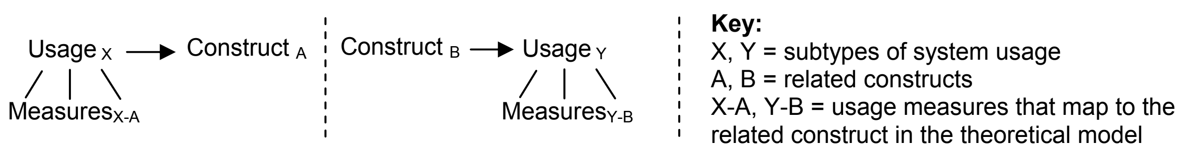


Figure 2.3: Contextualizing the System Usage Construct

2.5 Empirical Investigation of the Staged Approach for Reconceptualizing Usage

If the staged approach for reconceptualizing system usage is beneficial, a researcher should obtain more persuasive and meaningful results in a study of system usage if s/he follows the approach than if s/he does not. We empirically investigate this proposition to provide indicative (albeit, not definitive) support or refutation for the utility of the approach. To limit the scope of the investigation, we adopt the definition of system usage that we proposed in the *Definition* stage above and we focus on the benefit of following the steps in the *Selection* stage.

To conduct an empirical investigation of the approach, we must choose a theoretical and substantive context. The theoretical context that we chose was the relationship between system usage and short-run, individual task performance.[11] This is an important context because DeLone and McLean's IS success model (1992) suggests a link between system usage and individual task performance, but past studies of this link report mixed results (Pentland, 1989, Lucas and Spitler, 1999) and several scholars have called for more research to determine which usage measures are appropriate in this context (Chin and Marcolin 2001). The substantive context that we chose for the empirical investigation was analysts' use of spreadsheets for financial analysis. This is a crucial practical context because spreadsheets are among the most common end-user applications and decision-support tools in practice (Carlsson 1988; Panko 1998).

Following Figure 2.2, the first step when selecting usage measures is to define its structure. Because usage involves an IS, user, and task, the relevance of each element should be judged in light of the theoretical context. As financial analysis is a complex, cognitive activity, not a simple, mechanical task (Goodhue 1995), we expect that each element is relevant and, thus, a very rich usage measure is required (Table 2, model 6). The second step is to choose measures for its elements that relate theoretically to the other constructs in its nomological network. There are two constructs in our case: system usage and performance. Therefore, we select usage

[11] Past research suggests a distinction between the causes of short-run and long-run performance (March 1991). Although both are clearly important, we limit our empirical investigation to short-run performance.

measures by chaining backwards from performance measures to usage measures (per Figure 2.4).

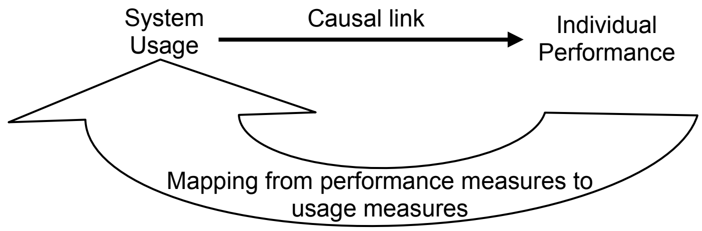


Figure 2.4: Selecting Usage Measures via Nomological Network Analysis

Following the logic in Figure 2.4, the sections below describe the nature of individual task performance and propose measures of system usage that relate to it theoretically. Specifically, we identify a type of system usage (exploitive system usage) that relates theoretically to performance and we demonstrate how a very rich measure of this type of usage can be formed by combining two rich measures of system usage: *cognitive absorption* (that captures a user's employment of an IS, per Table 2, model 4) and *deep structure usage* (that captures the use of the system for the task, per Table 2, model 5). As proposed above, if the staged approach for measuring usage is beneficial, measures selected according to the approach should be superior to other measures. Thus, after defining performance and its related usage measures below, we report on an experiment that we designed to test whether our ability to explain the relationship between individual system usage and short-run task performance improves when richer measures are used. In other words, we test whether explanations are strongest (in terms of the amount of variance explained and the interpretability of relationships) when a very rich measure is employed (i.e., exploitive system usage), less strong when a rich measure is employed (i.e., cognitive absorption or deep structure usage *alone*), and even poorer when a lean measure is employed (i.e., duration).

2.5.1 Defining Individual Task Performance

In the performance measurement literature, job performance comprises two dimensions:

task performance and contextual performance (Sonnentag and Frese 2002). Task performance consists of behaviors carried out to complete a job (Meister 1986), whereas contextual performance consists of behaviors that contribute to the social or psychological climate in which a job is performed (Sonnentag and Frese 2002). Both are measured via *assessments*, but assessments of task performance are job-specific while assessments of contextual performance are not (Sonnentag and Frese 2002). Thus, when studying short-run *task* performance, one's measures must reflect the task under consideration.

Assessments of individual task performance can be made in two ways: assessments of behavior or assessments of outcomes (Campbell 1990; Sonnentag and Frese 2002). These can differ in complex scenarios such as group work where an individual's output is not under his/her complete control (Beal et al. 2003). However, for the purpose of this empirical investigation, we assess performance as an *outcome* because the individual user has complete control of his/her own work on his/her spreadsheet, i.e., his/her output does not depend on other people.

According to Campbell (1990), the outcome of one's task performance can be assessed in terms of effectiveness. Other assessments such as "efficiency" can also be made (Beal et al. 2003), but we focus on effectiveness alone in this study. Thus, consistent with the performance measurement literature, we measure individual task performance as an assessment of individual task output in terms of its effectiveness, i.e., the degree to which it meets the task goals.

2.5.2 Mapping Individual Task Performance to Existing Usage Measures

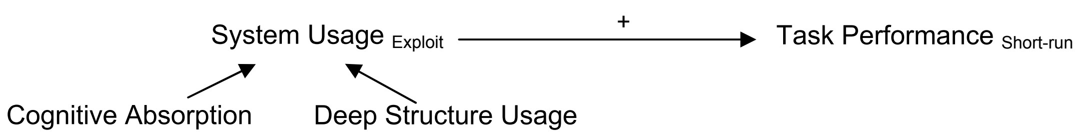
When we examine the relationship between individual task performance and extant usage measures in Table 2.1, only 2 of the 14 measures are, according to the literature, theoretically related to task performance. Specifically, Szajna (1993) explained the benefit of examining the *nature* of information used (e.g., the benefit of requesting particular *types* of reports and by being more *specific*) while Nance (1992) explained the benefits of *appropriate* use. However, neither of these measures complies well with the definition of system usage because Szajna's (1993) measures relate to information usage rather than system usage and Nance's (1992) measure was an evaluation of use rather than a measure of usage itself. Thus, new usage measures are needed. Following the logic in Figure 2.4, we next outline the type of use—exploitive usage—that is most conducive to a clear mapping to short run task performance.

2.5.3 Types of System Usage that Relate to Individual Task Performance

Two types of system usage can drive individual task performance: exploitation and exploration (March 1991). Exploitation refers to routine execution of knowledge, whereas exploration refers to the search for novel or innovative ways of doing things. A balance between these is necessary for long run performance, but exploitation is preferred in the short run because it has more predictable, immediate benefits (March 1991). In this illustrative case, the task is short, so our measures of usage must refer to exploitive use. Exploitive usage refers to usage that implements and executes one's knowledge of one's system and task. Subramani (2004) recently examined the benefit of measures of exploitive use at an organizational level of analysis but no such measures have ever been investigated at an individual level of analysis. Moreover, Subramani's (2004) measures of exploitive use referred only to the extent to which a system was used in specific tasks (per Table 2.2, model 5). Because a very rich measure of usage is needed in our theoretical context (per Table 2.2, model 6), the next sections demonstrate how such a measure was constructed.

2.5.4 A Model of System Usage and Individual Task Performance

Figure 2.5 presents our proposed theoretical model for this empirical investigation. Individual task performance is envisioned as a reflective construct measured in terms of effectiveness. System usage is modeled as an aggregate higher-order construct with two subconstructs that together capture user employment of the system (cognitive absorption) and the use of the system in the task (deep structure usage). The two subconstructs of usage may be, but need not be highly correlated. As such, the subconstructs *form* the very rich, higher-order construct of system usage (per Table 2.2, model 6) (Edwards 2001; Law et al. 1998).[12] Given our assumption of exploitive usage, we next explain the proposed subconstructs of system usage.



Legend: the subscripts 'Exploit' and 'Short-run' indicate that this theoretical model specifies the relationship between exploitive usage and short-run task performance (per Figure 2.3 above).

Figure 2.5: A Contextualized Model of System Usage and Individual Task Performance

2.5.4.1 Measuring User Employment of an IS During Exploitive Use: Cognitive Absorption

Theories such as TAM that explain antecedents to usage conceptualize a user's decision to use an IS as *cognition* but usage as a *behavior* (Compeau et al. 1999). However, from the perspective of the user employing the IS, system usage in cognitively engaging tasks is not merely behavior, but instead *cognitive behavior*. Thus, a user's performance in exploitive tasks should stem not only from the particular features she employs (Table 2.2, model 3) or the tasks for which she uses the system (Table 2.2, model 5), but also from her cognition-in-use (Table 2.2, model 4). This cognitive perspective guided the very first conceptualizations of usage (see Barkin and Dickson's model in Figure 2.1), but much recent research (e.g., TAM research) assumes that usage is non-cognitive.

The relevant cognitive variables for system usage are those that measure a user's cognitive state during usage. As Figure 2.6 shows, scholars view some individual differences as *traits* because they are relatively stable and invariant to external stimuli. They recognize others

[12] The logic for constructing aggregate higher order constructs is similar to the logic for constructing formative lower-order constructs, but in aggregate constructs each sub-construct can itself be reflective (Edwards, 2001).

as *states* because they are variable and can be manipulated (Webster and Martocchio 1992). As Figure 2.6 shows, the proposed model of usage includes measures of users' cognitive *state* during use; user *traits* are exogenous variables and are excluded from the usage construct. For example, users' knowledge of a system or a task, their ability or competence at performing certain tasks, their self-efficacy, and their experience could all influence use and performance (Marcolin et al. 2001), but they are not part of the system usage construct.

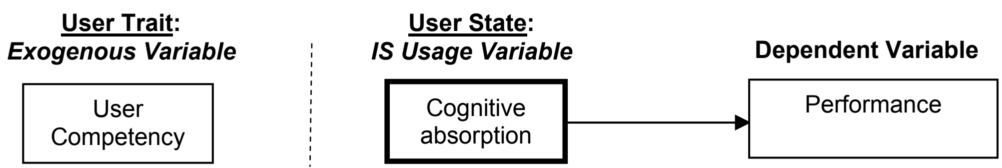


Figure 2.6: User State Versus Trait Variables for Measuring System Usage

Theories of cognition (Ashcraft 2002) delineate cognitive states associated with greater task performance. Several cognitive theories could be used to explain the aspects of cognition that are required for exploitive use of a system. For instance, Locke and Latham's (1990) goal setting theory suggests that users' level of commitment to the task goals while using an IS would be positively related to task performance. However, when choosing rich measures, one must balance completeness with parsimony. To build a parsimonious model, we focus on just one part of a user's relationship with his/her IS: cognitive absorption (Agarwal and Karahanna 2000).

Cognitive absorption is the amount of cognitive resources applied when using a system in a task. Two bodies of theory suggest that cognitive absorption will increase individual task performance. First, cognitive psychology asserts that individuals must engage in conscious (not automatic) processing to perform all but the simplest cognitive tasks (Ashcraft 2002). Cognitive absorption refers to a state in which an individual is applying a significant proportion of his/her conscious processing to the task. This improves cognition by controlling attention, speeding up information processing, filtering out distractions, and reducing errors (Ashcraft 2002; Eysenck

1982). Thus, given a set of tasks requiring cognitive resources, an increase in cognitive absorption, up to a reasonable level (Proctor and Dutta 1995), will increase the amount of cognitive resources applied, and thereby increase the number of tasks completed.

Researchers have also studied performance benefits from absorption under the rubric of *flow*. In a state of flow, individuals lose perception of their surroundings, reduce thinking about extraneous matters, increase attention to the task-at-hand, and feel more control over their work (Csikszentmihalyi 1990; Webster and Martocchio 1992). Flow theory also suggests that users in flow produce better quality work by being more creative. *Flow* supports creativity because individuals absorbed in their work have adaptable thought patterns because their attention is focused in what is unfolding in the moment (Mainemelis 2001).

Drawing upon these two prior research streams, we therefore hypothesize that an increase in cognitive absorption during system usage will lead to an increase in short-run task performance.

As Table 2.2 outlines, a user's cognitive state during exploitive use addresses an important part of use (the user's mental state), but it fails to capture the degree to which the IS is being used in the task. Of course, two users in the same cognitive state could apply an IS differently in their tasks. Thus, when testing the link between use and task performance, a richer measure is needed to capture the way the IS is being used in the task, beyond mere cognition (per Table 2.2, model 6).

2.5.4.2 Measuring Use of the System in the Task During Exploitive Use: Deep Structure Usage

To measure the degree to which a system is employed in a task, researchers often measure *breadth* of use (e.g., Saga and Zmud, 1994, Igbaria et al., 1997). From the perspective of a system, breadth refers to the number of features used (Table 2.2, model 3). From the perspective of a system's use in a task, breadth refers to the number of subtasks that an IS is used to support (Table 2.2, model 5). Unfortunately, the theoretical link between breadth of use and task performance is weak (Jasperson et al. 2005). Thus, following Figure 2.4, we searched for a measure of exploitive use that would capture the degree to which a system is employed in a task and that would theoretically relate to task performance.

Recently, Subramani (2004) developed a measure in this vein called "use for exploitation." In Subramani's study, this was a task-centered measure, with items specifically created to match the study task context. We adopt a similar approach. However, in our theoretical context, exploitive use refers not just to the use of the IS in the task, but also to user's cognitions. Thus, to clearly distinguish these two aspects of exploitive use, we refer to usage of the system to support the task as "deep structure usage" rather than Subramani's "use for exploitation."

We adopt the term "deep structure usage" because it usefully describes one's use of the core features of an IS to complete a task. Originally coined in linguistics (Chomsky 1965), the term "deep structure" has been used by many researchers to describe the underlying rules and structure of *tasks* and compare it to "surface structure" that represents transient patterns of work that are more adaptable and less significant (Gersick 1991; Heracleous and Barrett 2001; Lassila and Brancheau 1999; Malone et al. 1999). Others employ deep structure to describe the *IS* itself. Deep structure distinguishes functional rules and requirements embedded in an IS from its surface structure (i.e., its interface or appearance) and its physical structure (i.e., the physical machine) (DeSanctis and Poole 1994; Long and Denning 1995; Weber 1997).

In Adaptive Structuration Theory, DeSanctis and Poole (1994) integrated these views by proposing that deep structures are designed into IS to support the deep structure of the tasks for which they are used. DeSanctis and Poole characterize deep structures in IS in two ways: (1) as structural features like the rules, resources, and capabilities in the IS, and (2) as the spirit or values and goals underlying the set of structural features. Thus, two interpretations of deep structure usage can be made: (1) using features in the IS that are designed to support the deep structure of the task, and (2) using deep structure features in a way that is faithful with the system spirit (i.e., the intent or vision of the system designers) (DeSanctis and Poole 1994). We adopt the former interpretation. Chin et al. (1997) scaled "faithfulness of use" in the latter vein, but faithfulness is an *evaluation* of usage, not a measure of system usage itself.

In sum, a significant body of theory has been developed regarding deep structure, both in the analysis of users' tasks, analysis of the IS, and DeSanctis and Poole's integrated perspective. In line with developing new constructs to support theory development and testing, we propose a new measure of exploitive use based on this research stream, termed "deep structure usage." It is defined as the degree to which a user employs deep structure features of the system in the task.

Consistent with DeSanctis and Poole (1994), we propose that systems contain deep structures consisting of rules, resources, and capabilities that were designed into the IS to support a conception of user task requirements and that are made available for users to employ in the form of features (Griffith 1999). For systems that support a single task, the IS deep structure will map closely to the task deep structure. However, for systems that support many diverse tasks (such as the system in our illustrative case, i.e., spreadsheets), only a subset of the IS deep structure will support the deep structure of any specific task. Thus, following recent recommendations (Jasperson et al. 2005), when studying the relationship between exploitive use and performance, one must create a *task-centered* measure of deep structure use by examining the *subset* of the IS deep structure that is relevant for the task and then measuring the degree to which users employ those features. This task-centered approach is similar to the approach Subramani (2004) used to create measures of "use for exploitation." It also means that deep structure usage is related to Goodhue's (1995) task-technology-fit (TTF). TTF and deep structure usage are distinct, however, because TTF is an evaluative construct that measures the degree to which a system *fits* the task, whereas deep structure usage measures the degree to which a user is *using* the set of features in the IS that were designed to support the task.[13]

Because deep structure features support the core aspects of a user's task, we construct a measure of the IS and task context during exploitive use (per Table 2.2, model 5) based on the belief that deeper usage (defined as the extent to which deep structure features are used in the task), will lead to greater short-run performance because using these features leverages user cognition in the core executions of the task. By increasing the proportion of cognition applied to the task, deep structure usage should increase task performance in two ways. First, following TTF theory (Goodhue and Thompson 1995), focusing one's use on the core of the task should increase the proportion of required work completed by decreasing time spent on irrelevant tasks. Second, by engaging users' cognition in the task, deep structure features should increase work quality by users gaining insight into the task and how the IS is supportive. Please note that this insight need not only stem from *correct* use of the deep structure. If a user employs deep structure inappropriately, s/he can observe the results of his/her action and learn, but users who fail to use or choose not to use the deep structure, will not gain this understanding.

2.6 Empirical Test of the Staged Approach for Reconceptualizing System Usage

If the staged approach is beneficial, measures created according to the approach should perform more effectively than measures not selected according to the approach. Thus, in the present case, our ability to explain the relationship between system usage and short-run task performance should be best if we employ a very rich measure of system usage (i.e., exploitive system usage), less strong if we employ a rich measure of system usage alone (i.e., cognitive

[13] Both TTF and deep structure usage also differ from "appropriate use," which evaluates the degree to which users employ a system's features in an optimal way in the task, or in a manner expected by its designers (Chin et al. 1997).

absorption *or* deep structure usage), and even poorer if we employ a lean measure of system usage such as "duration of use." We used an experiment to test this proposition. A field study would have increased external validity, but lab experimentation provided a tighter test of the proposed measures of usage (Calder et al. 1981).

2.6.1 Task and Design

The task required a user to build a spreadsheet model in MS Excel to determine the best approach for financing an asset purchase. The task enabled a strong test of the theoretical model as the analysis was cognitively engaging, which allows variation in cognitive absorption, and the system (Excel) contains features that directly support the task, which allows variation in deep structure usage. Because our interest is in the importance of rich measures, but not specific values on these measures, we adopted a free simulation design rather than a factorial design (Fromkin and Streufert 1976). Free simulations allow values of the IVs (e.g., cognitive absorption and deep structure usage) to vary freely over their natural range. This gives an insight into nature of the IV→DV relationship as well as the range over which it occurs.

2.6.2 Subjects

Subjects were 229 students in an intermediate accounting course in a southern U.S. university. The accounting course integrated a series of spreadsheet-based business analysis assignments into the intermediate and prerequisite introductory accounting course. Students were graded on four assignments in the introductory class and four in the intermediate class. Data was collected during an end-of-semester case-exam worth 10% of the student's grade. The case used the same general format as previous assignments and involved accounting concepts learned during the course (present value, asset financing, and risk versus return). To the greatest extent possible, therefore, the system and task were believed to enable exploitive use by our subjects. Completion of the post-test instrument was voluntary; 177 were returned (response rate of 77%). Six cases with non-sensical responses were removed, leaving a full data set of n = 171.

2.6.3 Instrumentation - IV

Table 2.3 shows scales used to capture self-reported usage. To measure cognitive absorption, we adopted Agarwal and Karahanna's (2000) pre-validated scale of "focused immersion." The scale for deep structure usage was created afresh. We defined deep structure usage as: *use of features in the IS that support the underlying structure of the task*. Items were first constructed by one of the researchers. Because the deep structure scale needed to be task-centered, an independent domain expert (in this case, a course instructor) selected activities that described the task's underlying structure: *analyzing data, testing assumptions, and deriving conclusions,* and consistent with the approach in recent research (Subramani, 2004), the items were adapted for this task domain (see Table 2.3). The items were set at a mid-range of task-specificity to ask about the *class* of features used (i.e., deep structure features), but not *specific* features (e.g., present value functions). This balanced the need for broad applicability with the need for focused questions (Jasperson et al. 2005, Griffith and Northcraft 1994).[14] To test the need to capture each component of usage (per Table 2.2), the instrument also included a lean, omnibus measure of "duration of use." To measure duration of use, subjects were asked: *About how many minutes did you spend doing the case?* As the case was entirely computer-based, this captured both usage duration and task duration. As with the self-report measure for deep structure usage, we also obtained an objective measure of usage duration.[15]

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[14] A potential risk with the deep structure usage scale is that it assumes that subjects have knowledge of the system's deep structure. As each subject had previously completed eight similar cases, we believe this assumption is reasonable. However, to control for this risk, we obtained protocol data from a sub-sample of 46 users during the case by having screen-cam software video-record their sessions. Two independent coders then coded the protocols, rating the degree to which each user employed the system's deep structure. The results for this data are stronger, but lead to the same conclusions as the self-report data regarding the value of rich versus lean usage measures (see Appendix 2A).

[15] We objectively measured usage duration by viewing the protocols of system usage referred to in footnote 14. The objective data led to the same conclusions as the self-reported data (see Appendix 2A).

Table 2.3: Measurement scales†

Construct	Items
Cognitive absorption (adapted from Agarwal and Karahanna 2000)	8. When I was using MS Excel, I was able to block out all other distractions 11. When I was using MS Excel, I felt totally immersed in what I was doing 14. When I was using MS Excel, I got distracted very easily* 21. When I was using MS Excel, I felt completely absorbed in what I was doing 24. When I was using MS Excel, my attention did not get diverted very easily
Deep structure usage (new scale)	15. When I was using MS Excel, I did not use features that would help me analyze my data* 17. When I was using MS Excel, I used features that helped me compare and contrast aspects of the data 20. When I was using MS Excel, I used features that helped me test different assumptions in the data 27. When I was using MS Excel, I used features that helped me derive insightful conclusions from the data 30. When I was using MS Excel, I used features that helped me perform calculations on my data
Objective measure of performance (reflective)	This relatively objective scale allocated a single percentage score based on marks for the following components: 1. identifying the problem; 2. building a flexible model, 3. correctly analyzing the data; 4. identifying solutions; 5. highlighting impacts; 6. creating a focused report; and 7. giving clear recommendation. The scale was created independently from the research by task experts and was assessed by independent coders.

† All self-report items used a 9-point strongly agree - strongly disagree Likert scale.[16] * Negatively worded items.[17]

Once the self-reported items for system usage were drafted, a Q-sort exercise was used to improve construct validity (Moore and Benbasat 1991). The ten usage measures and measures from other published scales were randomized and given to eight doctoral students. They were asked to separate items into bundles for each construct and to name each construct. Their feedback supported the validity of the scales; minor changes were made based on their feedback.

2.6.4 Instrumentation - DV

To reduce common method bias between IV and DV, an objective scale for overall task performance was developed independently from the research (Table 2.3). This scale assessed the degree to which an individual's output met the task requirements. Two independent coders rated participant performance using the scale and the interrater reliability was high (ICC (2,2) = 0.87).

2.6.5 Procedure, Pretest, and Pilot Test

In the experiment, subjects read the instructions (5 minutes), performed the analysis task in

[16] To ensure that our use of Likert scales did not bias the results, we collected an additional, smaller data set (n=50) in which we included an additional item per construct using an "extent" scale. The results indicated no method bias; the "extent" and Likert items always loaded together and displayed high reliability (ICC (2,2) = 0.74 CA, 0.79 DS).

[17] Negatively worded items were used to check for response bias. However, recent studies suggest that negatively worded items can change a construct's meaning (Motl and DiStefano 2002). Therefore, we excluded the two negatively worded items (CA3, #14, and DS1, #15) from our tests. This had no substantive effect on the results.

MS Excel (90 minutes), and completed the questionnaire (15 minutes). To validate the procedure and instruments, we conducted a pretest with four students and a pilot test with 38 students.

2.7 Results of the Empirical Investigation

Data analysis proceeded in two steps. We first examined the descriptive statistics and the proposed measurement model, then the posited structural model. Both steps were performed using partial least squares (PLS). PLS was used in preference to LISREL because LISREL is not suited to testing higher-order molar constructs in the presence of only one DV (Edwards 2001).

2.7.1 Descriptive Statistics

Table 2.4 details study descriptive statistics. The data for *minutes* included some values greater than the allowable time (90 minutes). As the results did not vary when these cases were deleted, all were kept in the final analysis. The variance inflation factors from a regression of minutes, cognitive absorption, and deep structure usage against performance ranged from 1.01-1.27, indicating no significant multicollinearity (Mathieson et al. 2001). Skewness and kurtosis and the normal probability plot from a regression of the three variables on performance supported normality. We also checked for outliers, which were not an issue.

Table 2.4: Descriptive Statistics*

Construct/Item	N	Mean	Std. Deviation
Performance	166	81.01	15.87
Minutes	166	81.07	19.99
CA 1	171	5.96	1.89
CA 2	171	5.78	1.80
CA 4	171	5.94	1.65
CA 5	171	5.73	1.68
DS 2	171	6.11	1.73
DS 3	171	6.08	1.68
DS 4	171	6.09	1.59
DS 5	171	6.98	1.56

* Performance measured on a 0-100 scale.
CA (cognitive absorption) and DS (deep structure) used a 1-9 scale.

2.7.2 Measurement Model

Tables 2.5-2.6 report tests of instrument validity and reliability. Table 2.5 supports scale validity as each item loaded cleanly and significantly ($p < .01$) on its construct and loaded on its construct more highly than 0.70 (Hair et al. 1998). Table 2.6 provides further support for construct validity as the square root of the average variance shared between each construct and its indicators is higher than 0.50 and in all cases is higher than the variance it shares with the other constructs (Fornell and Larcker 1981).

Table 2.5: Loadings, Cross-Loadings, and Reliability*

Item	CA	DS	Minutes	Reliability
CA4	**0.84**	0.45	0.11	CA:
CA2	**0.81**	0.36	0.03	Cron. α = 0.81
CA5	**0.81**	0.36	-0.01	CR = 0.69
CA1	**0.73**	0.45	0.06	
DS4	0.53	**0.86**	0.03	DS:
DS3	0.31	**0.82**	-0.04	Cron. α = 0.82
DS2	0.39	**0.81**	-0.02	CR = 0.70
DS5	0.38	**0.73**	-0.01	
Minutes	0.06	-0.01	**1.00**	NA

* All item-to-construct loadings are significant ($p < .05$). Loadings, cross-loadings, and composite reliability (CR) obtained from PLS; Cronbach's alpha obtained from SPSS.

Table 2.6: Inter-construct Correlations and Average Variance Extracted*

	Cognitive Absor.	Deep Structure	Minutes	Performance
Cognitive Absor.	**0.80**			
Deep Structure	0.51	**0.81**		
Minutes	-0.08	-0.03	**1.00**	
Performance	0.37	0.46	-0.29	**1.00**

* The bolded values on the diagonal are the square root of each construct's AVE and should be higher than 0.50.

In terms of reliability, Table 2.5 indicates that both scales' reliabilities are higher than Nunnally's (1967) minimum guideline of 0.60. Therefore, the results suggest that the validity and reliability of the data are adequate for testing the structural model.

2.7.3 Structural Model and Nomological Validity

Table 2.7 reports results for nomological validity. As PLS does not provide overall goodness-of-fit tests, one examines R^2 values (Mathieson et al. 2001). To test the effect of modeling usage as a higher-order construct, we tested two models (Edwards 2001): one included both subconstructs as independent components, and a second formed a higher-order construct using the factor scores of cognitive absorption and deep structure usage as formative indicators.

Table 2.7: PLS Structural Models†

Measurement Approach	Model	Results
Extent of use (omnibus): Table 2.2, Model 2	Minutes → Performance	$B_M = -.29$, $t = -4.00$**, $R^2 = .087$
Extent to which the user employs the system: Table 2.2, Model 4	Cognitive Absorption → Performance	$B_{CA} = .42$, $t = 7.11$**, $R^2 = .178$
Extent to which the system is used to carry out the task: Table 2.2, Model 5	Deep Structure Usage → Performance	$B_{DS} = .47$, $t = 8.97$**, $R^2 = .218$
Extent to which the user employs the system to carry out the task: Table 2.2, Model 6	*Component Model*	
	Cognitive Absorption → Performance Deep Structure Usage → Performance	$B_{CA} = .25$, $t = 3.39$** $B_{DS} = .34$, $t = 4.77$** $R^2 = .264$
	*Higher-Order Model**	
	Usage → Performance Cognitive Absorption → Usage Deep Structure Usage → Usage	$B_U = .51$, $t = 9.57$** $Weight_{CA} = .83$, $Weight_{DS} = .90$ $R^2 = .262$

† B: the coefficient between an antecedent and performance. Weight: the weight of the subconstruct on the higher-order usage construct in PLS.
* The higher-order model was constructed using factor scores for each sub-construct. We also ran this test using averages rather than factor scores; the results were not substantively different.
** All t-values significant at $p < .01$.

In a separate analysis (not shown to conserve space), we tested another higher-order model using the method of "repeated indicators" (Chin et al. 2003; Lohmoller 1989). The results were consistent with those using the formative model. Finally, we ran each model in PLS and stepwise regression controlling for important predictors of task performance and the results did not change (see Appendix 2A). Overall, three findings in Table 2.7 are noteworthy:

1. The lean usage measure (duration) has a significant *negative* relationship with performance;
2. The rich usage measures (cognitive absorption and deep structure usage) both *positively* impact performance and each explain more than twice the variance explained by duration;
3. A very rich measure of usage that captures the user, system, and task aspects of use explains almost three times the variance explained by the lean measure, and the results are similar whether usage is modeled as a higher-order construct or as a combination of components.

As several models in Table 2.7 are nested, one can statistically compare the degree to which each usage measure explains performance. Table 2.8 shows the results of this test. Consistent with our predictions, the results suggest that excluding either the user or task aspects of use leads to a significant (small-to-medium) reduction in R^2. Excluding the rich usage measures altogether and relying solely on a lean measure (duration) leads to a large reduction in R^2 and a change in the direction of the relationship between usage and performance. Although speculations can be drawn *post-hoc* regarding possible reasons for a negative relationship between duration and performance, we do not believe that this relationship is highly interpretable, i.e., speculations could also have been made if we had found the relationship to be positive.

Overall, the results strongly support the proposed two-step method for selecting usage measures. The implications of these results are considered next.

Table 2.8: Impact of Excluding Usage Measures

Test	**Models compared**		**Change in R^2**	**Effect Size**†
	Full Model	**Partial (Nested) Model**		
Impact of not measuring the IS, user, & task (only measuring duration)	Perf = CA, DS, Mins (Table 2.2, Models 6 & 2)	Perf = Mins (Table 2.2, Model 2)	0.25**	f^2 = .37 Large
	Perf = Usage†, Mins (Table 2.2, Models 6 & 2)	Perf = Mins (Table 2.2, Model 2)	0.24**	f^2 = .36 Large
Impact of measuring the user/IS but not the IS/task	Perf = CA, DS (Table 2.2, Model 6)	Perf = CA (Table 2.2, Model 4)	0.09**	f^2 = .12 Small-Medium
Impact of measuring the IS/task but not the user/IS	Perf = CA, DS (Table 2.2, Model 6)	Perf = DS (Table 2.2, Model 5)	0.05**	f^2 = .06 Small-Medium

** Sig. at $p < .01$, † Usage is formed with the factor scores of CA and DS as formative indicators, per Table 2.6.

† Each construct's effect size (f2) can be calculated by the formula $(R^2_{full} - R^2_{partial}) / (1 - R^2_{full})$ (Mathieson, et al., 2001; Chin et al., 2003). Multiplying f2 by (n-k-1), where n is the sample size (171) and k is the number of independent variables, provides a pseudo F test for the change in R2 with 1 and n-k degrees of freedom (Mathieson et al., 2001). An effect size of .02 is small, .15 is medium, and .35 is large (Cohen 1988).

2.8 Discussion

This paper presents a systematic attempt to define, conceptualize, and measure the system usage construct. It contributes in four ways, summarized in Table 2.9, which we discuss in turn.

Table 2.9: Research Contributions and Implications

Element of Research	Contribution
Staged Approach	Provides a way to explicitly conceptualize and reconceptualize system usage in IS research
- Definition Stage	Enables researchers to distinguish between system usage and other constructs, and to specify the content of the system usage construct.
- Selection Stage	Enables researchers to select measures of system usage that minimize errors of inclusion and omission.
Empirical Investigation of the Staged Approach	Provides indicative evidence that the staged approach is beneficial and provides a validated measurement model of system usage for an important, practical domain.

The core contribution of the research is that it provides a way for IS researchers to explicitly reconceptualize system usage. Although system usage has long been a central construct in IS research, past conceptualizations of it have remained implicit, there is no accepted definition of the construct, and there is no standard approach for selecting or validating its measures. The staged approach that we advance in this paper acknowledges the complexity of system usage and the diverse contexts in which it can be studied (per Figure 2.1), and presents the first systematic approach for conceptualizing and contextualizing system usage. This assists IS research in two ways. First, it provides a way for researchers to select precise measures of usage and thereby obtain more meaningful findings about the relationship between system usage and its antecedents and consequences in specific contexts. Second, by encouraging researchers to explicate the theory and assumptions behind their choice of usage measures, the approach should support cumulative research in IS by enabling researchers to achieve a more integrated understanding of system usage across different contexts (e.g., by supporting meta-analyses of usage research).

Each stage of the proposed approach offers additional concrete contributions. For example, the *definition* stage enables researchers to distinguish system usage from other constructs. In past research, many studies have employed *other* constructs (e.g., dependence on use, or information usage, per Table 2.1) as proxies (i.e., measures) of system usage (Goodhue and Thompson 1995, Szajna 1993), or conversely, have employed system usage as a proxy for other constructs (e.g., IT acceptance) (Trice and Treacy 1986). There is nothing inherently wrong with this practice except that in past research it has been implicit rather than explicit, and there remains a lack of evidence for which proxies are accurate and which are not. Consider TAM. TAM explains IT "acceptance," but the DVs in TAM are usage intentions and usage behavior (Davis, 1989). It is not clear that either of these constructs completely captures the notion of "acceptance" (Trice and Treacy 1986). Our research highlights the need for researchers to provide systematic evidence for which usage measures, if any, are valid proxies for related constructs and to determine which other constructs, if any, are good proxies for system usage.

The *selection* stage contributes by providing a way to reduce errors of inclusion and omission when measuring usage. Errors of inclusion occur when a researcher includes irrelevant aspects of usage or another construct in his/her usage measures, while errors of omission occur when a researcher omits key elements of usage from his/her usage measures (e.g., omitting cognitive absorption from measures of exploitive use). The two-step selection method reduces the possibility of both errors. First, it cautions against lean or omnibus usage measures. Lean measures such as use/non-use, duration of use, and "extent" can increase errors of inclusion and omission because they obscure (1) what *constitutes* usage, and (2) what *part* of usage the researcher intends to measure. Thus, subjects who respond to a lean usage measure may have a broader or narrower view of usage than intended by the researcher, leading to systematic errors in their responses. Lean measures can also risk errors of inclusion by simply reflecting *different* constructs. For example, "task duration" can often equate to "usage duration," as it did in our empirical test. The error of inclusion in our investigation was so strong that it changed the direction of the estimated relationship between usage and performance. Because omnibus measures have such significant limitations, we suggest that in future research they be used very cautiously if at all.

The proposed two-step method provides a way to move beyond lean measures by advancing a systematic way to create contextualized usage measures. For too long, we submit, IS researchers have studied "system usage" without specifying and theoretically justifying the type of usage being studied. Nevertheless, it is clear that different types of usage could be more relevant in different contexts. The two-step method provides a way for researchers to develop contextualized usage measures by specifying which elements (i.e., system, user, task) and which measures of usage are most relevant for a theoretical context. For example, user cognitive absorption may be a highly relevant metric for line managers who depend on performance outcomes from employees' usage, but it may be of little relevance to system administrators who must make decisions based on how *systems* are actually being utilized (i.e., in terms of system load) irrespective of employee cognitions. Moreover, even if the *elements* of usage are the same in two contexts, different *measures* may be needed. For example, when studying long run rather than short run performance, one would need additional measures to capture exploratory and exploitive use (March 1991; Subramani 2004). Thus, much research is needed to systematically identify *managerial relevant* subtypes of usage, define appropriate measurement models for these contexts, and theorize the antecedents and consequences of these subtypes of use, rather than, or at least in addition to, studying the antecedents and consequences of system usage in general.

A final contribution of our research is that the empirical investigation provides a validated usage measure for an important, practical context: the relationship between system usage and short-run performance in cognitively engaging tasks. Our test also suggests ways to improve empirical studies of usage. For example, it suggests that the two-step approach could be used to select *methods* for collecting usage data because objective measures may be more able to measure the *system* and *task* aspects of usage (per Appendix 2A), but self-report questionnaires may be more able to measure *user* states (e.g., cognitions/emotions) during usage (Hilbert and Redmiles 2000). These issues are addressed further in Chapter 5. Our test also highlights the need to determine when higher-order models or component models of usage are appropriate. A higher-order model can never increase statistical explanations of a DV over an optimally weighted combination of components (Edwards 2001). Therefore, as the higher-order model in our tests explained performance to the same degree as the component model (per Table 2.7), the parsimony of the higher-order model makes it more attractive (Segars and Grover 1998). But a higher-order model may not always be best. If we examined long run rather than short run performance, we would have needed to include subconstructs for both exploratory and exploitive use and create a third-order model to capture both subconstructs. Whether such a third-order model of usage could be developed is not clear. Thus, more research is needed to determine when an overarching construct of "usage" can be constructed and when it is best modeled as a combination of components (per Edwards, 2001).

When assessing the contributions of our empirical investigation, it is important to recognize its limitations. In terms of construct validity, our work could be extended to see if additional aspects of the user, IS, or task contexts could be modeled. For example, we measured user cognitions, but some *non-cognitive* elements such as a user's affective state may have been relevant. In terms of internal validity, we did not consider antecedents to usage or potential mediators between usage and performance such as learning. Such research would be valuable because tying each usage measure to relevant causes and consequences could allow researchers to develop more complete models of IS success (DeLone and McLean 2003). Finally, in terms of external validity, one may argue that our deep-structure usage measure lacks external validity because operationalizing it requires researchers to create items that reflect the deep structure of the IS and task under investigation. This problem only occurs at the level of measures, however, not constructs. For example, Goodhue (1995) measured task-technology-fit (TTF) in the context of data management, and although his measures are not easily generalizable to other domains, TTF is a generalizable construct (Lee and Baskerville 2003). Overall, the development of feature-specific measures is in line with recent recommendations (Jasperson et al. 2005).

2.9 Conclusion

To overcome the lack of explicit conceptualizations of system usage in past research, the present study advances a staged approach for reconceptualizing system usage. The first stage, definition, recommends that researchers explicitly define system usage and its assumptions. The second stage, selection, recommends that researchers select usage measures by a two-step method that involves identifying the relevant elements of usage for a research context (i.e., IS, user, and/or task) and identifying measures for these elements based on the other constructs in the nomological network.

In the present study, we demonstrated how such an approach would work through an empirical investigation in which we examined the degree to which lean usage measures and rich usage measures would explain the relationship between system usage and task performance in cognitively engaging tasks. The results strongly support the staged approach and indicate that inappropriate choices of usage measures can significantly reduce explanations of performance, even causing the estimated relationship between usage and performance to change direction.

Despite acknowledged limitations, we believe the staged approach advanced in this paper helps to clarify the meaning of system usage and the range and dimensionality of past usage measures. Given contradictory results in past studies of system usage and performance, and the centrality of the usage construct in past research, our focused reconceptualization of the construct should enable more informed research into the pathways by which IT impacts individuals at work.

Appendix 2A: Additional Empirical Results

This appendix details results from two additional tests of the data from the empirical investigation.

2A.1 Testing for the Robustness of our Results across Data Collection Method

We collected additional, independent data to confirm the results from our self-reported measures of deep structure usage and usage duration. As we indicated in footnote 14, a potential risk with the self-reported measure of deep structure usage is that it may rely too strongly on users' knowledge of the system's deep structure. To control for this, we obtained screen-cam video records from a subsample of respondents' usage (n=46) and independent raters coded the video protocol data, rating the extent to which users employed each feature of MS Excel in their tasks.[18] The inter-rater reliability of their coding was acceptable (ICC (2,2) = .73).

To identify the subset of Excel features that related to the task's deep structure, we gave two independent domain experts (instructors of the course from which we obtained our sample) a comprehensive list of features available via menus or shortcuts in Excel. Using a 7-point likert scale, each rater then rated the degree to which each feature supported the task's deep structure (i.e., analyzing data, testing assumptions, and deriving conclusions). Their ratings indicated that two features were the primary features of deep structure: functions (i.e., formulae) and filling (i.e., completing rows or columns of data using absolute or relative cell references). Again, inter-rater reliability was acceptable (ICC (2,2) = .76).

We tested the deep structure use → performance relationship in PLS by creating a model of deep structure usage reflected by indicators of subjects' extent of use of the two deep structure features. As Table 2A.1 shows, the independent measure of deep structure usage had a stronger

[18] To ensure that the coders measured system usage rather than an evaluation of system usage, the coders rated the extent to which each subject used each feature, not how appropriately s/he used each feature (e.g., not whether a user chose an ideal formula or whether s/he entered correct values into a formula).

relationship with performance than the self-reported measures of deep structure usage, but the overall pattern of results from Table 2.7 was confirmed, i.e., the rich measure of usage (deep structure usage) had a stronger and more meaningful relationship with performance than the lean measure (duration).

As Table 2A.1 shows, we used the same screen-cam video files to obtain an independent measure of each participant's duration of Excel usage and re-tested the relationship between usage duration and task performance on this subsample (per footnote 15). As Table 2A.1 shows, the independently coded data for usage duration showed a negative relationship with performance that was slightly stronger, but similar to the self-reported measures in Table 2.7. Overall, the results in Table 2A.1 confirm the pattern of results in Table 2.7 in that richer measures are preferable to leaner measures.

Table 2A.1: Testing the Robustness of the Results across Method†

Measurement Approach	Model	Results
Extent of use (omnibus): Table 2.2, Model 2	N = 171 $\text{Minutes}_{\text{Self}} \longrightarrow$ Performance	B_M = -.29, t = -4.00**, R^2 = .087
	N = 46 $\text{Minutes}_{\text{Ind}} \longrightarrow$ Performance	B_M = -.34, t = -3.18**, R^2 = .117
Extent to which the system is used to carry out the task: Table 2.2, Model 5	N = 171 Deep Structure $\text{Usage}_{\text{Self}} \longrightarrow$ Performance	B_{DS} = .47, t = 8.97**, R^2 = .218
	N = 46 Deep Structure $\text{Usage}_{\text{Ind}} \longrightarrow$ Performance	B_{DS} = .70, t = 8.94**, R^2 = .485

† N = 46; B: the coefficient between an antecedent and performance. Self: self-reported measures; Ind: Independently recorded measure (by examining screen-cam video protocols of users' sessions).
* All t-values significant at p < .05.

2A.2 Testing for the Influence of other Predictors of Performance

Prior research on performance suggests that the primary predictors of individual task performance are an individual's declarative and procedural knowledge (Hesketh and Neal 1999; Sonnentag and Frese 2002). Table 2A.2 replicates the results from Table 2.7, controlling for these two factors. Declarative knowledge was measured via students' score in their final accounting exam (ACC). Procedural knowledge was measured via students' performance in the four cases preceding the experimental case (CASE). The results in Table 2A.2 and Table 2.7 are consistent, confirming the importance of the system usage construct and the benefit of the proposed approach to measuring it.

Table 2A.2: PLS Structural Models with Control Variables†

Measurement Approach	Model	Results
Extent of use (Table 2.2, Model 2) plus control variables[19]	MINS, ACC, CASE → Performance	B_{MINS} = -.30 t = -4.40** B_{ACC} = .07 t = 0.97 B_{CASE} = .19 t = 2.26* R^2 = .143
Extent to which the user employs the system (Table 2.2, Model 4) plus control variables	CA, ACC, CASE → Performance	B_{CA} = .42 t = 7.55** B_{ACC} = .08 t = 1.16 B_{CASE} = .16 t = 2.42* R^2 = .233
Extent to which the system is used to carry out the task (Table 2.2, Model 5) plus control variables	DS, ACC, CASE → Performance	B_{DS} = .41 t = 7.42** B_{ACC} = .07 t = 0.92 B_{CASE} = .14 t = 1.64 R^2 = .219
Extent to which the user employs the system to carry out the task (Table 2.2, Model 6) plus control variables	*Component Model* CA, DS, ACC, CASE → Performance	B_{CA} = .29 t = 4.14** B_{DS} = .26 t = 3.33** B_{ACC} = .07 t = 1.06 B_{CASE} = .14 t = 1.95 R^2 = .284

† B: the coefficient between an antecedent and performance. ** significant at p < .01; * significant at p < .05.

[19] We also tested whether ACC or CASE moderated the effect of duration (MINS) on performance. However, neither interaction effect was significant: MIN*ACC (B = .04, t = .66, p > .05), MIN*CASE (B = .02, t = .26, p > .05).

Chapter 3

Toward a Deeper Understanding of System Usage in Organizations: A Multilevel Perspective[20]

Abstract

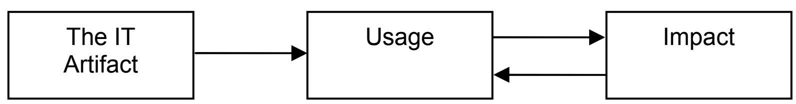
Economic studies show that information systems can positively impact organizations, but little is known of the process by which this occurs. Although the system usage construct is key to understanding this process, there has been little conceptual scrutiny of this construct in past research. The objective of this paper is to contribute towards a deeper understanding of system usage in organizations by examining its multilevel nature. To do so, we provide a framework for building multilevel theories of system usage, introduce principles to help researchers use the framework, and provide a concrete illustration of how a multilevel theory of system usage can be developed. The multilevel perspective advanced in this article offers rich opportunities for theoretical and empirical insights and suggests a new foundation for in-depth research on the nature of system usage, its emergence and change, and its antecedents and consequences.

Keywords: System usage, IT impact, level, multilevel, pattern, configuration, time, longitudinal.

[20] Burton-Jones, A., and Gallivan, M.J. "Towards a Deeper Understanding of System Usage in Organizations: A Multilevel Perspective," Working paper, Department of Computer Information Systems, Georgia State University, 2004.

3.1 Introduction

Economic studies have shown that information systems can have positive organizational impacts (Barua and Mukhopadhyay 2000; Dedrick et al. 2003), but we know little about *how* such impacts occur (Heine et al. 2003; Soh and Markus 1995). Recent reviews have tried to identify a core set of predictors of IT impacts, such as the quality of an organization's IT and human resources, its business processes, and competitive environment (Melville et al. 2004). Although these factors are critical, most researchers agree that IT impacts can only occur if the systems are used (Soh and Markus 1995, DeSanctis and Poole 1994, Orlikowski 2000) (Figure 3.1).



Extract from Benbasat and Zmud (2003)

Figure 3.1: IT Impacts Stem from Use

Given that system usage is a necessary link between IT and its consequences, we might expect that conceptualizations of the systems usage construct occupy a central role in the IS literature. Some evidence supports this view. For example, differing conceptions of systems usage have been offered at the individual (Barkin and Dickson 1977), group (DeSanctis and Poole 1994), and organizational levels (Cooper and Zmud 1990). A substantial body of empirical work has also examined system usage from nomothetic (Straub et al. 1995; Venkatesh et al. 2003) and idiographic perspectives (Markus 1994; Orlikowski 1996; Orlikowski 2000).[21]

Recently, however, researchers have called for a renewed focus on the usage construct (Chin and Marcolin 2001; DeLone and McLean 2003). This appears to be the case for four reasons. First, while a substantial amount of research has studied antecedents to usage (i.e., antecedents → usage) (Agarwal 2000), relatively few studies have investigated system usage in

[21] A complete review of existing conceptualizations of the system usage construct is outside the scope of this paper. Appendix 3A provides a brief review of prior conceptualizations of system usage.

its role as an independent variable leading to downstream consequences (i.e., usage → consequences). Many studies of IT impacts at the individual (Todd and Benbasat 1999), group (Zigurs and Buckland 1998), and organizational levels (Santhanam and Hartono 2003) exclude usage (i.e., they study antecedents → consequences); implicitly assuming that system usage is unproblematic (e.g., that users always use systems in an appropriate or predictable manner). As a result, the consequences of system usage remain under-theorized. As Venkatesh et al. (2003) write:

> "little to no research has addressed the link between user acceptance and individual or organizational usage outcomes. Thus, while it is often assumed that usage will result in positive outcomes, this remains to be tested" (p. 470).

Second, when the consequences of system usage have been studied, results have often been mixed. Although perhaps unsurprising in hindsight, many researchers at an individual, group, and organizational level have hypothesized (or assumed) that system usage will increase performance, but have found that usage can increase (Chidambaram and Jones 1993; Torkzadeh and Doll 1999), decrease (Pentland 1989; Strauss and McGrath 1994; Szajna 1993), or have no effect on performance (Gelderman 1998; Lucas and Spitler 1999; Trauth and Jessup 2000).

Third, there has been little concern in the IS literature regarding the appropriate way to measure system usage. While many self-report and system-generated measures are easy to capture, researchers have given surprisingly little thought to whether such measures are truly meaningful (Straub et al. 1995). Due to the abundance of measures available, many researchers believe that (p. 244) "there really has never been a problem with obtaining usage measures" (Srinivasan 1985). In a field in which construct development is highly valued (Boudreau et al. 2001), it is surprising that system usage has received very limited construct or instrument development (Chin and Marcolin 2001; Straub et al. 1995; Trice and Treacy 1986). Despite occasional calls to more closely study system usage (e.g., Zigurs 1993), DeLone and McLean (2003) demur, "*the problem to date has been a too simplistic definition of this complex variable.*"

Fourth, while systems usage has been a common focus among researchers at different levels of analysis, there has been little integration of conceptions across levels. For example, there is little evidence that conceptions of individual usage have influenced those of group or organizational usage, or vice-versa (see Appendix 3A). While idiographic research has provided rich multilevel descriptions of usage (Orlikowski 1996; 2000), this has had little impact on nomothetic conceptions (Chan, 2000). Although IS researchers may have learned a great deal from studying usage at a single level, we believe that studying organizations one level at a time ultimately leads to an unnatural, incomplete, and very disjointed view of how organizations really function in practice.

In summary, understanding systems usage is central to studies of IS impacts. However, while construct conceptualization lies at the heart of theory building, few theoretical conceptions of system usage have been offered and there has been little cumulative progress across levels of analysis (Chan 2000). In recent years, scholars have worked to reconceptualize basic constructs such as users (Lamb and Kling 2003) and the IT artifact (Orlikowski and Iacono 2001). There have also been calls to reconceptualize system usage (Agarwal 2000; Chin and Marcolin 2001; DeLone and McLean 2003). This is the charge that we undertake in this paper: to propose a new multilevel conceptualization of system usage and advance a detailed framework for researching it.

It is important at the outset that we state the epistemological and ontological assumptions underlying our analysis. In this paper, we adopt critical realist assumptions (Cook and Campbell 1979). In other words, we believe that it is meaningful to talk about and measure a latent construct called "system usage" and to examine its existence, causes, and consequences. We do not deny the value of other perspectives. In fact, we believe that non-positivist research is far ahead of positivist research in understanding the nuances of system usage in practice (e.g., Orlikowski 1996, 2000) and we draw liberally upon findings from such research. However, as our focus is on usage as a "construct," we limit our theoretical perspective to a critical realist one in this paper.

The paper is structured as follows. We first briefly review the principles of multilevel theory. We then draw on these principles to advance a new framework for building multilevel theories of system usage. The framework defines three stages (definition, structure, and function) and nine steps that we propose are necessary for building a multilevel theory of usage. Using the framework as a structure for the remainder or the paper, we then offer principles for carrying out each step in studies of system usage and provide concrete illustrations to demonstrate that the framework is actionable and operationalizable. We conclude with a discussion of the value of a deeper understanding of system usage and the rich, unexplored areas for research that it suggests.

3.2 Overview of Multilevel Theory

Because there are only a few explicitly multilevel studies in IS research (Ang et al. 2002; Angst and Agarwal 2004; Gallivan et al. 2005), it is necessary to provide a brief overview of multilevel theory before applying it to system usage.

3.2.1 Foundations of Multilevel Theory: General Systems Theory

Multilevel theory is a "meta-theory," or set of theoretical principles, for theorizing about organizations. As with any theory, multilevel theory is built upon certain assumptions about the phenomena it investigates. The core assumption in multilevel theory is that "organizations are multilevel systems" (Kozlowski and Klein 2000, p. 3). In contrast to single-level research, in which the "system [to be studied] is sliced into organization, group, and individual levels" (Kozlowski and Klein 2000, p. 3), multilevel theories attempt to account for phenomena that span organizational levels. To study organizations as multilevel systems, multilevel theorists have adopted principles from general systems theory (von Bertalanffy 1968) to serve as assumptions about the nature of organizations (see Table 3.1) (Kozlowski and Klein 2000).

Table 3.1: Principles of Multilevel Theory*

Principle	Example	Example References
Composition: Parts interact with different strengths or coupling, creating wholes (collectives) that have different forms.	Firms have tighter internal coupling than external coupling, but the level of internal and external coupling can vary between firms and within firms.	(Simon 1996; Weick 1976)
Top-down influence: Properties of parts are constrained and enabled by properties of the whole.	Employees' performance is constrained and enabled by their workgroup's norms.	(Churchman 1968; Giddens 1984)
Bottom-up emergence: Properties of the whole emerge from interactions among the parts.	A team's performance emerges not merely from individual performance but from interactions among the team's members.	(Churchman 1968; Giddens 1984)
Equilibrium and change: The existence, strength, and nature of part-whole interactions are stable in equilibrium but can change over time.	In equilibrium, group routines tend to drive members' behavior, but in flux, a single member's behavior can change group routines.	(Ancona et al. 2001; Gersick 1991)
Pace: Properties of parts change more rapidly than emergent properties of wholes.	Team members can improve their own performance more quickly than they can collectively improve team performance.	(Gersick 1991; Simon 1996)

* The principles apply across all levels; the examples focus on just one or two levels for simplicity.

By influencing the way that multilevel researchers view organizations, the principles in Table 3.1 influence the way that multilevel researchers conceive of constructs and relationships to account for organizational phenomena. In particular, because multilevel researchers assume that different collectives can have different structures and that these structures can change over time, they assume that (a) constructs observed in one collective may or may not exist in a different collective or in the same collective at a different time, and (b) the relationships between constructs can differ across collectives and over time. In other words, multilevel researchers acknowledge the context-dependency of theories. Although this assumption of context-dependency is common in interpretive research (Robey and Sahay 1996), it is rare in positivist research.[22] To allow for this context-dependency, and yet still enable broad generalizations, multilevel researchers use

[22] For a critique of theories that exclude context, we refer to Arrow et al. (2000, p. 28), who argued that: "attempts to strip context from groups are…limiting and doomed to fail… We need to study groups in context. This may be inconvenient…but pretending that groups can exist without a context, is, we believe, counterproductive."

principles that enable them to incorporate context in their theories by (a) accounting for how constructs emerge at the level at which they are being examined, and (b) accounting for relationships between constructs at multiple levels. These principles are described next.

3.2.2 Building a Multilevel Theory: Structure and Function

To build a multilevel theory, researchers must define the constructs of their theory and explain their structure and function (Morgeson and Hofmann 1999). *Structure* refers to the nature of the constructs in the theory whereas *function* refers to relationships between the constructs.

3.2.2.1 Structure

In terms of structure, multilevel theories start by specifying the origin and level of each construct (Kozlowski and Klein 2000). A construct's *origin* is the level at which a construct originates. For example, "behaviors" originate at the individual level, while "interdependencies" originate at the dyadic level (as interdependencies require at least two people). A construct's *level* is the level at which a researcher chooses to study a construct. For example, a researcher may choose to study a construct at the level of its origin (e.g., an individual's memory), or s/he may choose to study a construct at a higher level (e.g., an organization's memory). From a multilevel perspective, the distinction between origin and level is critical because if one wishes to study a construct at a level higher than its origin, one must justify how the construct *emerges* at this higher level. For example, "organizational memory" does not just "exist," but emerges from processes and practices that occur at lower levels of an organization (Morgeson and Hofmann 1999).

The distinction between a construct's origin and level allows multilevel theorists to distinguish: (a) between individual and collective constructs, and (b) among different types of collective constructs (Klein et al. 1994). According to this perspective, an individual level construct exists when the construct's origin and level are both at the individual level. In contrast, a "collective construct" refers to any construct that has a *level* higher than the individual level but whose origin could lie at the level being studied or at a lower level (e.g., an individual level).

As Table 3.2 shows, there are three types of collective constructs: global, shared, and configural (Chan 1998; Klein et al. 1994; Kozlowski and Klein 2000; Rousseau 1985). Global constructs originate at the collective level and have no meaning at the individual level. For example, in group-level research, group size is a global construct because it has no meaning at an individual level. Shared constructs originate in the attributes of individuals and emerge at the collective level in the form of homogeneity among members. For example, a group's behavior could be considered shared if each individual behaved similarly. Finally, configural constructs originate in the attributes of individual members but emerge at the collective level in the form of a distinct pattern among members of the collective. For example, a group's behavior is considered configural if each individual demonstrates different, yet fairly consistent behavior, leading to a distinct pattern across the members of the group and/or across time (Kozlowski and Klein 2000).

Table 3.2: Structure of Collective Constructs in Multilevel Theory

Description	Explanation	Measurement
Constructs at the collective level can be: • Global (origin = level), • Shared (level > origin), • Configural (level > origin).	**Global** constructs: properties that exist only at the whole (e.g., a collective's official usage policy).	Direct measure, e.g., by asking a key informant.
	Shared constructs: properties ascribed to the whole due to parts having similar values (e.g., the extent to which members have a common understanding of the official usage policy).	Aggregate measure that captures the homogeneity of a collective, e.g., mean average (if low variance).
	Configural constructs: properties ascribed to the whole due to parts having a pattern of values (e.g., a collective's particular pattern of compliance with the official usage policy).	Aggregate measure that captures the heterogeneity of a collective, e.g., min/max, dispersion, profile deviation.

3.2.2.2 Function

The second limb of a multilevel theory is its *function*. Table 3.3 describes the different functional forms that multilevel theories can employ (Klein et al. 1994; Kozlowski and Klein

2000; Rousseau 1985). Essentially, there are two types of multilevel theories: single-level and cross-level. However, due to the complexity of collective constructs and cross-level relationships, Table 3.3 includes at least eight distinct types of functional relationships. At a single level, theories can use individual or collective-level constructs (global, shared, or configural). Cross-level theories can use individual and/or collective constructs such that the X-Y relationships may either: (1) cross levels, (2) be at the same level, but moderated by a variable at another level, or (3) vary depending on the individual's relative standing within the group (known as the "frog-pond" effect). Analytical methods to study such models are well developed in statistics, including methods to test the validity of multilevel constructs (e.g., tests of within group homogeneity) (Bliese 2000), and methods to test relationships among multilevel constructs (e.g., hierarchical linear modeling) (Castro 2002; Raudenbush and Bryk 2002), but they are little known (Walczuch and Watson 2001) and rarely used (Ang et al. 2002) in IS research.

Table 3.3: Functional Relationships in Multilevel Theory (Kozlowski and Klein 2000)*

Single-Level Models	**Example**	**Cross-Level Models**	**Example**
1. Individual (level = origin) $X_i \longrightarrow Y_i$	Individual system usage affects individual performance	5. Direct Down $X_c \searrow Y_i$	Group norms affect individual performance
Collective 2. Global (level = origin) $X_g \longrightarrow Y_g$	Group size affects group performance (global)	6. Direct Up $X_i \nearrow Y_c$	An individual's decision affects group performance
3. Shared (level > origin) $\begin{array}{ccc} X_s & \longrightarrow & Y_s \\ \uparrow & & \uparrow \\ X_i & & Y_i \end{array}$	Group system usage (shared) affects group performance (shared)	7. Moderated $\begin{array}{ccc} & Z_c & \\ & \downarrow & \\ X_i & \longrightarrow & Y_i \end{array}$	Group norms moderate the effect of individual usage on individual performance
4. Configural (level > origin) $\begin{array}{ccc} X_c & \longrightarrow & Y_c \\ \nearrow \uparrow \nwarrow & & \nearrow \uparrow \nwarrow \\ X_{ia}\, X_{ib}\, X_{ic} & & Y_{ia}\, Y_{ib}\, Y_{ic} \end{array}$	Group system usage (configuration) affects group performance (configuration)	8. Frog Pond $(X_i - X_c) \longrightarrow Y_i$	Organizations' *relative* capabilities within their industry drives their competitive advantage

* The forms of theory apply across all levels; the illustrations focus on just one or two levels for simplicity.
Subscripts: i = individual, g = group, s = shared, c = configural (left column), collective (right column).
These theoretical forms are a minimal set: they can be extended to create additional theoretical forms (known as "mixed models" or "homologous models") that combine features of each one.

As Klein et al. (1994) emphasize, it is important to note that the functional forms of multilevel theory in Table 3.3 *encompass* single-level relationships. In other words, researchers cannot "escape" multilevel principles by just studying a single-level. On the contrary, even researchers who study phenomena at a single level of analysis must understand whether the constructs that they seek to model are purely individual, or whether they are global, shared, or configural in nature. Multilevel researchers assume that organizations *are* social systems and, as a result, assume that multilevel principles apply irrespective of whether one examines just one or multiple levels of the social system (Kozlowski and Klein 2000). Appendix 3B provides a more detailed account of the empirical and theoretical motivations for adopting multilevel research.

3.3 A Multilevel Perspective on System Usage

In this section, we apply multilevel principles to specify, for the first time, how one could build a multilevel theory of usage. Our aim is not to propose "the" multilevel theory of usage, but to propose a general yet actionable framework for theory building. Table 3.4 details the steps that we state are necessary to build a multilevel theory of system usage. We describe each one in turn.

Table 3.4: A Framework for Building a Multilevel Theory of System Usage

Step	Explanation
Definition	Define system usage.
Structure	
- Existence	Identify whether system usage exists at the individual level, collective level, or both.
- Structural form	Identify the form in which collective system usage emerges (i.e., shared or configural).
Function	
- Selection	Select the relevant elements and measures of usage for one's theory. Determine whether the function of each measure is tied to a particular structural form.
- Functional form	Specify the relationships between individual and collective usage. Specify the relationships between system usage and the other constructs in the theory.
- Time frame	Specify the time over which individual and collective usage influence each other. Specify the time over which usage and its related constructs influence each other.

3.3.1 Defining System Usage

To build a multilevel theory of system usage, one must first define the construct. Despite many conceptions of systems usage in the literature, there is no generally accepted definition of the construct, whether at the individual, group, or organizational level. Perhaps the best known definition is one from DeLone and McLean (1992): "recipient's consumption of the output of an information system." We submit, however, that this captures use of *information* from an IS, but not system usage itself. Therefore, to support a multilevel perspective of usage, we propose a new definition. We propose that system usage is an activity that involves three elements: (1) a user (i.e., the subject using the system), (2) a system (i.e., the object being used), and (3) a task (i.e., the function being performed). By drawing on these elements and recognizing that systems comprise many features (Griffith 1999), we define system usage as: *a user's employment of one or more features of a system to perform a task*. This definition is useful for two reasons. First, it provides a scope for what elements can be included in the usage construct. This is needed to provide a basis for determining its structure (e.g., why usage emerges in certain forms) and its function (e.g., how to select properties of usage that are relevant for specific theories). Second, it gives researchers flexibility, because each element of usage (i.e., the user, system, and task) can be viewed in multiple ways (Lamb and Kling 2003; Orlikowski and Iacono 2001; Zigurs and Buckland 1998). With this definition as a starting point, we next outline its structure and function.[23]

3.3.2 The Structure of System Usage

Once a researcher has defined system usage, the next stage is to define its structure. This involves determining: (a) whether it exists at an individual level, collective level, or both, and (b) the form in which it emerges at the collective level (per Table 3.4).

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[23] We recognize that other definitions (with broader or narrower meanings) could be constructed. The principles in Table 3.4 are generic enough to apply to all definitions of system usage.

3.3.2.1 Identifying the Level at which System Usage Exists

To identify the level(s) at which system usage exists, researchers must perform two steps. The first is to identify the level at which system usage *originates* (Kozlowski and Klein 2000). Our definition of usage refers to a user, task, and system. We agree with Lamb and Kling (2003) that "users" can be conceptualized at all levels of analysis (e.g., individual, group, organization). However, as collectives can only "use" systems through actions of their individual members, the origin of the usage construct lies at the *individual* level.[24] The second step is to determine whether usage exists at the collective *level*. Because usage originates at the individual level, it cannot be *global* at the collective level (Kozlowski and Klein 2000). Thus, if a researcher wishes to create a measure of collective (e.g., group) usage when observing a set of individuals using a system, s/he has three options: 1. to define collective usage as shared, 2. to define collective usage as configural, or 3. to acknowledge that collective usage does not exist (Kozlowski and Klein 2000).

Because collectives and collective constructs are social constructions, their existence will always remain somewhat fuzzy (Arrow et al. 2000). Nevertheless, four principles can be used to identify the existence of a collective and collective system usage (Table 3.5). Two implications stem from these principles. First, for collective usage to exist, a "collective" and "usage" must both exist. Thus, collective usage excludes collectives that do not have an IS, as access precedes use (Huber 1990), and it excludes collectives or members of a collective that have access to a system but do not use it. Figure 3.2 depicts this principle by showing three overlapping groups, two of which use systems. If every employee in each group with an IS is a "user," then collective

[24] There are two exceptions to this rule. First, one might argue that collective usage does not *always* originate with the individual. For example, some systems are triggered by inputs from other systems and are only indirectly invoked by people, e.g., intelligent agents and automatic fulfillment systems. Second, one might argue that collective usage does not *only* originate with the individual. Thus, one may consider interdependencies that occur between people using an system (e.g., coordination) to be *part* of collective usage. In this latter case, collective usage would originate partly at the individual level and partly at the dyadic level (i.e., the lowest level at which coordination occurs). We believe the principles in this paper can be applied to both exceptions, but we do not comment on them to simplify the analysis.

Table 3.5: Identifying Collectives and Collective System Usage

Principles for Identifying a Collective (adapted from Arrow et al. 2000, p. 34-35)	Principles for Identifying Collective System Usage
(a) Do the individuals consider themselves to be members of a collective (that may, in turn, be part of a larger collective)?	(a) Do the individuals consider themselves to be using a system as a collective (that may, in turn, be part of a larger collective using the system)?
(b) Do the people recognize one another as members and distinguish members from nonmembers?	(b) Do the people recognize one another as users of the system and distinguish users in their collective from other individuals?
(c) Do the collective members' activities show more tightly coupled interdependence within the group than with others in the larger collective?	(c) Do the collective members' usage patterns show more tightly coupled interdependence within the group than with others in the larger collective?
(d) Do members of the collective share a common fate (or consequence) that is not totally shared by the larger collective?	(d) Do users in the collective share a common fate (or consequence) stemming from their collective use that is not totally shared by the larger collective?

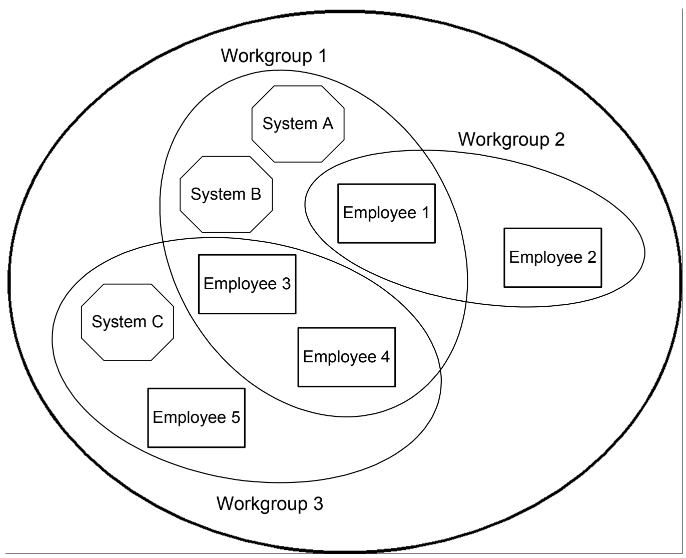


Figure 3.2: Conceptualizing Collective System Usage

usage exists in groups 1 and 3, but not group 2 (since group 2 has no system). Conversely, if *no* employee in group 1 was a "user" of System A, a "collective" would exist, but "collective usage" of System A would not exist. Finally, if Employee 4 in group 3 was not a user, group 3 would be a collective, but collective usage could only involve dyadic use by Employees 3 and 5.[25]

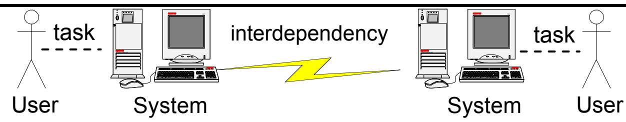
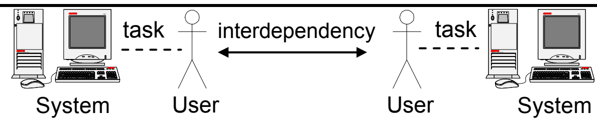
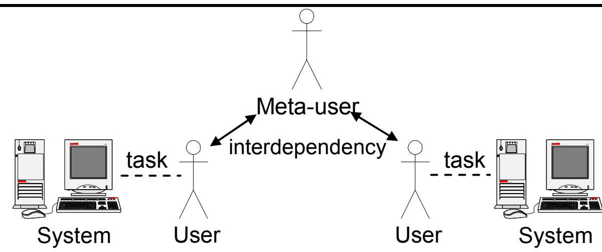
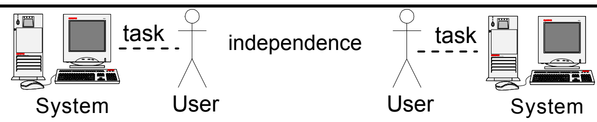
[25] It may appear counterintuitive to exclude non-users from collective use, but it is crucial when assessing causality. For example, a research team may write a paper and one member may use EndNote™ to write all of the references. Did the team use EndNote? No: the team *relied* on EndNote, but only one member used it, so individual-level usage existed but not team-level usage (Table 3.5). This distinction could be crucial. For example, if an embarrassing error occurred (e.g., a mis-citation), the distinction would help researchers determine the error's cause(s) (e.g. individual-level usage and/or team-level monitoring) and resulting changes (e.g., changes in team-level delegation practices).

A second implication is that although collective usage is defined by interdependencies, the principles in Table 3.5 do not limit the medium through which interdependencies are enacted. To illustrate this implication, Table 3.6 depicts four archetypes of collective usage. Following the principles in Table 3.5, users in Table 3.6 can engage in "collective usage" if they share interdependencies during use. Thus, a strict definition of collective usage might limit it to Model 1 in Table 3.6, in which users interact and coordinate their work solely *via their IT* to produce joint output (e.g., as in virtual teams using group support systems, Dennis et al. 2001). Although this definition can be defended logically, we argue that it is too restrictive because it limits collective usage to interdependencies that occur explicitly through the system according to the *physical* structure of the IS (i.e., by the ISs being physically connected) rather than its *deep* structure (i.e., the underlying business process supported by the ISs). We propose that collective usage can exist in Models 1-3, since these represent scenarios in which interdependencies during system usage exist, but not in Model 4 where there are no interdependencies, and thus no "collective."

In Model 2, individuals in a workgroup use systems in their work to produce a joint output, but they manage their interdependencies via face-to-face rather than IT-mediated interaction. An example of collective usage in Model 2 could be members of a consulting team who interact face-to-face or by telephone to collaborate on a project, yet they use common systems to support their joint work (e.g., Microsoft Office applications or Lotus Notes).

In Model 3, collective usage also exists, but another individual, here called a meta-user (Orlikowski et al. 1995), is assigned the role of coordinating the members' work rather than the members interacting directly. Each member produces part of the group output, but the meta-user (e.g., a supervisor) coordinates the workflow. An example of this model would be the use of a customer support system by call center operators. Even if each operator handles calls individually, with no direct interaction among operators, the calls they receive, the procedures

Table 3.6: Nature of System Usage as a Collective Construct

Model	Nature of Systems Usage Among Individuals	Explanation	Type of Construct
1		Interdependency between users via their IT	Both individual and collective system usage exist
2		Interdependency between users who use IT	
3		Indirect interdependency between users who use IT (mediated by a "meta-user," e.g., a manager)	
4		No direct or indirect interdependency exists	Individual system usage exists

they follow and the workflow embedded into the system that they use are all centrally coordinated and managed for the efficiency of the whole collective. In this case, the meta-user would be the call center supervisor who helps to manage and control the customer support process via interaction with operators and via controls and procedures s/he builds into the automatic call distribution system.

Although collective usage exists in Models 1-3, it does not exist in Model 4, because in this last scenario, there is no interdependency among users. If these individuals use the same system(s) but work alone on independent tasks, they cannot be considered a collective—they are just sets of individuals (Barley 1990, p. 70). Thus, Model 4 does not illustrate any collective behavior and cannot be described by a 'collective construct.' This is a key principle. A collective cannot be defined by *researcher-fiat*. If a researcher measures individuals working independently of each other (as in Model 4) and aggregates each individual's system usage to create a summed or averaged measure—labeling it "collective usage,"—this would constitute a cross-level fallacy (Rousseau 1985) (see Appendix 3B). A collective is also not defined by *institutional-fiat*. If an informant claimed that their organization or group "used" IS, but it was found that individuals (or groups) merely operated independently of each other in using these systems, then the informant would also be committing a cross-level fallacy. Unfortunately, these fallacies are all too common in the IS literature, whereby researchers assume that a global measure of system usage exists at the organization or group level, simply because they can aggregate individual data to form group-level or organizational-level scores. As system usage is not a global construct, researchers cannot simply assume that collective usage exists. Rather, they must be able to establish that collective usage is either shared or configural (for which other prerequisites must be demonstrated); otherwise the notion of collective usage is a fallacy (Rousseau 1985, Kozlowski and Klein 2000).

Two responses could be made to our arguments to date. On the one hand, one might suggest that our principles for defining the existence of collective usage are too weak because most members of an organization could be considered somewhat interdependent. On the other hand, some might argue that the principles are too strict because they place a high burden on researchers to determine whether collective "usage" really exists. We recognize both concerns but believe that our principles strike a balance between either extreme. Although employees of most organizations are somewhat interdependent, it would be rare to find a CEO or CIO who could say that their firm used all their IT systems in a truly interdependent and integrated fashion. Using systems as a collective is *difficult*, and CEOs and CIOs could certainly benefit from research that helps them to understand what it takes for an organization to use a system, and to use it effectively, in practice.

3.3.2.2 Identifying the Form in which Collective Usage Exists

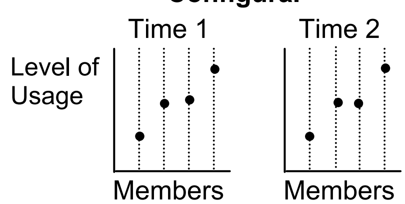
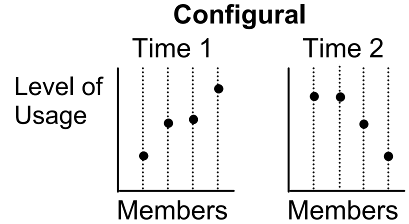
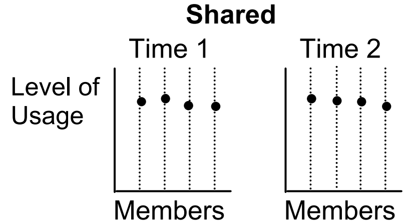
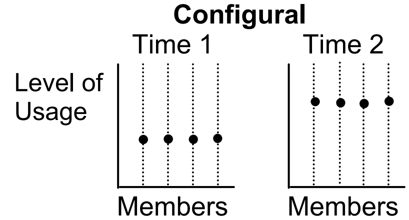
Once a researcher has justified that collective use exists, the next step is to justify whether the construct is shared or configural. *Shared* use emerges at the collective level in the form of homogeneous levels of use among members. For example, a collective (e.g., a virtual team) may develop routines such that each member uses a collaboration system at a similar degree of intensity and frequency. This homogenous use of the system across group members would be considered the group's (shared) level of use.[26] Typical quantitative measures of shared use would be a mean of the levels of the collective's members (assuming that the variance among members is low).

Configural use emerges at the collective level in the form of a distinct pattern of use among members. The levels may vary among members, but the differences are stable or patterned over time, not random. For example, members of a virtual team may use a system in different ways and amounts, and these differences may follow a heterogeneous but stable pattern, e.g., relating to individual differences or job roles. For example, one individual may *send* email messages asking questions to the other team members, who *respond* to these questions, but who do not *initiate* new email messages. This is a simple example of a particular pattern (or configuration), but obviously there are other, more complex configurations, whereby users have distinct patterns of use that fit their workgroup's or organization's task needs. Typical quantitative measures of configural usage would be a measure of dispersion, accompanied by some justification that the variation represents a stable pattern, and not just random "noise." Typical qualitative measures of configural usage would be a categorization of the pattern, e.g., an

[26] Homogeneity is not an all-or-nothing entity. In quantitative research, there are well-defined statistical tests such as intra-class correlation, eta squared, and others to identify whether an attribute is sufficiently similar within members of a group (versus sufficiently diverse for members across groups) to establish homogeneity (Castro 2002; James 1982).

expert-novice division of labor.   As Table 3.7 shows, configurations in collective usage can occur across members at a point in time (Model I), across time within members (Model IV), or across members and time (Model III). In contrast, shared usage occurs when system usage is similar both across members and over time (Model II).

Table 3.7: Forms of Shared and Configural Use*

<table>
<tr><td></td><td></td><td colspan="2">Across Time</td></tr>
<tr><td></td><td></td><td>Same</td><td>Pattern</td></tr>
<tr><td rowspan="2">Across Members</td><td>Pattern</td><td>Configural

</td><td>Configural

</td></tr>
<tr><td>Same</td><td>Shared

</td><td>Configural

</td></tr>
</table>

* As the illustrations rely only on quantitative data and just two periods, they can only *imply* configurations. Accurately identifying and validating the level and form in which a collective construct emerges may ultimately require researchers to collect data over more than two periods and use quantitative and qualitative methods in combination (Berson et al. 2003).

What generates shared usage or configural usage? As Figure 3.3 illustrates, the existence of shared or configural usage depends on three criteria: presence of equilibrium, level of complexity, and length of time. First, multilevel theory assumes that systems tend towards stability or equilibrium over time (per Table 3.1). It may be obvious that shared constructs require stability, but so too do configural constructs because distinct patterns can only emerge if there are periods of equilibrium (Meyer et al. 1993). Past research suggests that an assumption of stability is untenable in two situations. First, disequilibria occur in the transitions that occur between different equilibria (e.g., as in the transition from Time 1→Time 2 in Table 3.7, Model IV) (Gersick 1991). Such a situation can occur, for example, during the "window of opportunity" (Tyre and Orlikowski 1994) following a new IT implementation, before new

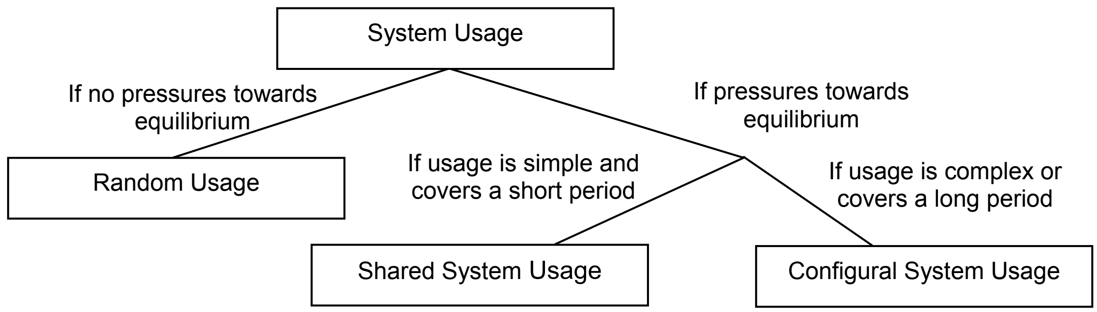


Figure 3.3: Factors Influencing the Form of Collective Usage

routines stabilize. Second, disequilibria can persist if external forces prevent the formation of equilibrium, e.g., in turbulent, "high-velocity" work environments (Brown and Eisenhardt 1997). In either scenario, if equilibrium is not achieved, collective usage can neither be shared or configural; instead it will remain essentially random (per Figure 3.3).

If periods of equilibrium can be identified, Figure 3.3 suggests that the second factor determining whether usage is shared or configural is *complexity* (Arrow et al. 2000). Our definition of usage suggests that collective usage will be more complex if the collective, system, or task, increases in size or complexity. An increase in the complexity of any of these elements increases the opportunity and incentive for members to form configurations by increasing the likelihood that inherent individual differences among members and the inherent modularity of systems and tasks will enable different members to enact different types of usage and achieve efficiencies through specialization of labor, fit, and synergy (Rousseau 2000).

Finally, in terms of *time*, Arrow et al. (2000, p. 39) suggest that the number and variety of patterned regularities in a collective increase over time. This is because longer periods allow for regular patterns to emerge in an individual's behavior or work role due to common temporal phenomena such as learning curves and entrainment (e.g., work cycles) (Ancona et al. 2001).

In summary, our analysis suggests that if periods of equilibrium can be assumed, configural use will be common. As Arrow et al. (2000, p. 38-40) explain, patterned regularities are the norm in real world collectives (even if they are the exception in IS or organizational research). Configurations are enacted by individual members (e.g., to self-regulate behavior), enacted by collectives (e.g., to achieve coordination and synergy), assigned by authority figures in collectives (e.g., to require members to follow assigned roles), and selected by evolutionary forces (e.g., due to variation, selection, and retention of patterns over time; Lassila and Brancheau 1999). For all these reasons, we expect configural usage to be more common in practice than shared usage.[27]

3.3.2.3 Implications for Measuring Collective Usage

Our analysis of the structure of collective usage has important implications for measuring system usage. Specifically, it reveals that past practices for measuring collective usage have risked substantial errors of inclusion and exclusion of measurement. Errors of inclusion occur when researchers include a measure of collective usage when it does not in fact exist. Errors of exclusion occur when researchers exclude a more appropriate representation of collective usage (e.g., representing collective usage as shared when it is more accurately considered configural).

Consider, first, the dominance of cross-sectional research in IS (Pinsonneault and Kraemer 1993). As Table 3.7 showed, researchers cannot determine the form in which collective usage has emerged without measuring usage over at least two time periods. Consequently, cross-sectional research designs provide no way for researchers or reviewers to determine whether errors of inclusion or exclusion have occurred. Unfortunately, even in longitudinal research,

[27] Historically, multilevel researchers initially emphasized that collective constructs emerge from homogeneity. As a result, principles for identifying configural constructs are much less developed than those for shared constructs (Kozlowski and Klein 2000). In recent years, more emphasis has been accorded to configural constructs. As Rousseau (2000, p. 578) argues: "…[in] many situations… the mere study of individuals…would miss the point: variability among individuals may be the critical quality that creates the larger whole." Likewise, Kozlowski and Klein (2000, p. 61) criticize past research which "has tended to limit consideration to shared models of emergence…Theory needs to be able to capture the rich complexity of [different forms of] emergence rather than limiting emergence to universal conceptualizations that often do not exist."

common measurement practices pose risks. For example, one way to gauge an organization's use of IS is to ask key informants (Gelderman 1998). However, typical key informant studies do not verify that lower level units are sufficiently homogenous or patterned to warrant aggregating the data to a shared or configural construct (Rogers 1996). If a researcher captured a measure of organizational-level system usage based on key informant input in such a manner, the meaning of the measure would be unclear and the researcher could be committing an error of inclusion.

To overcome the problems with key informant studies, some have obtained *actual* usage measures from individuals and aggregated them to the group or organizational level using sums or means (Devaraj and Kohli 2003; Easley et al. 2003). Unfortunately, even these cases are problematic. Sums and averages are not configural measures, and unless users have similar levels of use, they cannot represent shared usage (Kozlowski and Klein 2000). Unless researchers report tests for homogeneity or patterned use (James 1982; Klein et al. 1994), the *meaning* of such aggregated measures is unclear (Bliese 2000) and the potential for errors of inclusion is high.

Importantly, the need to defend collective constructs applies equally to positivist, qualitative research. Qualitative methods are ideally suited to multilevel research. Unfortunately, seminal qualitative methods texts give scant attention to multilevel issues (Miles and Huberman 1994; Yin 1994). Consider an example. Assume that a qualitative researcher studied an organization's 'use' of an ERP system and identified 'themes' (i.e., constructs) in their data by aggregating concepts that appeared in and across respondents' statements by (implicitly or explicitly) counting how often such themes appeared in interviews with members. If the researcher generalized these themes to describe "organizational usage," this could create an error of inclusion if the organization was more a collection of independent units rather than an interdependent collective (per Table 3.6, Model 4).

Although we believe that errors of inclusion when studying system usage are very common in IS research, errors of exclusion appear to be particularly prevalent because in our review of past usage studies, we found **no** study that represented collective usage as configural. Instead, IS researchers have continued to use sums and means to paint collectives in flat, single colors, that remove all variations and patterns. By employing this strategy, researchers lose crucial information in their measures. This may be one reason why researchers have obtained mixed results for the relationship between use and other downstream variables such as performance. Acknowledging the need for configurations and patterns in formulating our theories could improve the "precision and power" of empirical tests (Meyer et al. 1993, pp. 1192) and would allow a much more natural conception of organizational life, by recognizing the complex patterns and regularities that occur in real organizations (Arrow et al 2000).

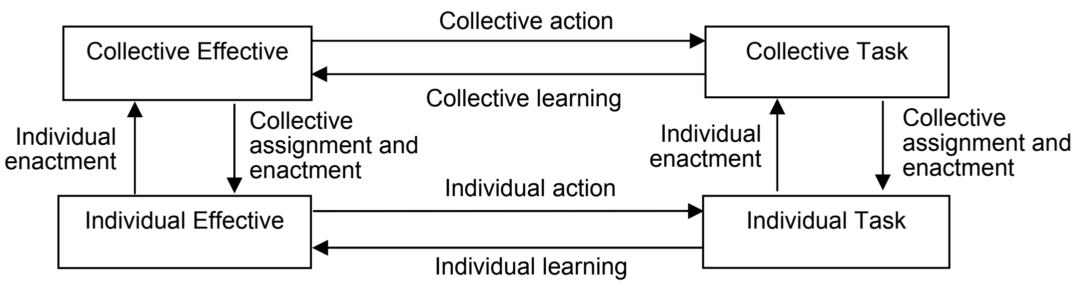
A response to our argument may be that interpretive researchers have long studied how members of a collective enact distinct patterns of use. One can point to observations of patterns among individuals in groups (Mackay 1990) and groups in organizations (Barley 1990; Orlikowski 2000). However, although these studies show how usage patterns can occur, they have been more concerned with the *emergence* of patterns, rather than the *implications* of such patterns for individual and collective outcomes such as performance. For example, Barley (1990) and Mackay (1990) study the emergence of different patterns of technology usage, but not the performance implications arising from these different patterns. Although this is consistent with their interpretive epistemological assumptions, it is incumbent on positivist researchers to not only identify patterns, but to identify their causes and/or consequences (Meyer et al. 1993). To perform this task, researchers need to specify the second leg of a multilevel theory—its function.

3.3.3 The Function of System Usage

The function of system usage refers to its relationships with other constructs in a theory. As Table 3.4 shows, researchers must carry out three steps to specify the function of system usage: (a) select the relevant aspects of usage for one's theory and the functional importance of each one, (b) specify the relationships among system usage and other construct(s) in the theory, and (c) define the time over which the constructs in the theory influence each other. These steps are best discussed in the context of an actual theory. As there are no existing multilevel theories of system usage, we will first briefly introduce a theoretical model that could support such research and then use this to provide a concrete example for how one can specify the function of system usage. As stated earlier, our intention is not to define "the" multilevel theory of system usage, but to provide a concrete example for the steps that should be undertaken to build such a theory.

3.3.3.1 A Representational Theory of System Usage and Task Performance

One way to build a theory about system usage is to refer to a theory of IS artifact (Orlikowski and Iacono 2001). Several theories of the IS artifact could be used, but for illustrative purposes, we will use the "representational" theory of IS developed by Wand and Weber (1995). Wand and Weber theorize that IS are representations of users' perceptions of real world domains and that IS are created, used, and modified over time because "it is the human condition to seek better ways to understand and to represent the world" (Weber 1997, p. 59). Wand and Weber's theory is limited to the IS artifact itself (Weber 1997), but it can be extended to theorize the IS artifact-in-use (Orlikowski 2000). Specifically, one way to study the *function* of system usage is to examine its relationship with downstream outcomes. In this section, we will propose a theory to explain the relationship between usage and one key outcome: task performance. Figure 3.4 illustrates the proposed multilevel model.



Key:

Support in literature:	1. Goodhue and Thompson (1995)	3. Edmondson (2002); March (1991)
	2. Orlikowski (2000); Barley (1990)	4. DeLone and McLean (1992, 2003) (not in the scope of this study)

Figure 3.4: Illustrative Theoretical Model

According to Wand and Weber, the essential purpose of IS is to provide representations of users' conceptions of their world(s). Therefore, *good* IS (i.e., those that lead to favorable impacts) are those that provide faithful representations (Weber 1997, p. 73). By extension, system usage should lead to favorable outcomes (i.e., it can be considered "effective") if it maintains a faithful representation of users' world(s). Thus, Wand and Weber's theory provides a basis for developing an evaluative measure of system usage, i.e., *effective system usage*. An evaluative measure such as this can be a particularly useful way of studying the connection between how individuals or collectives use systems and downstream impacts such as performance (Marcolin et al. 2001, p. 53).

The dependent variable in Figure 3.4, task performance, is an evaluation of the work that one carries out to complete a job (Brown and Duguid 1991). When evaluating work performance, one can evaluate *actions* or *outcomes*. In many jobs, system usage is a required action and, as such, it could be evaluated as a performance. Thus, to ensure a distinction between effective usage and task performance, we conceptualize performance in terms of *outcomes* and effective usage in terms of *actions*. Following Campbell (1990), we evaluate performance in terms of effectiveness, i.e., the degree to which one's task outcomes meets the task goals.

3.3.3.2 Selection

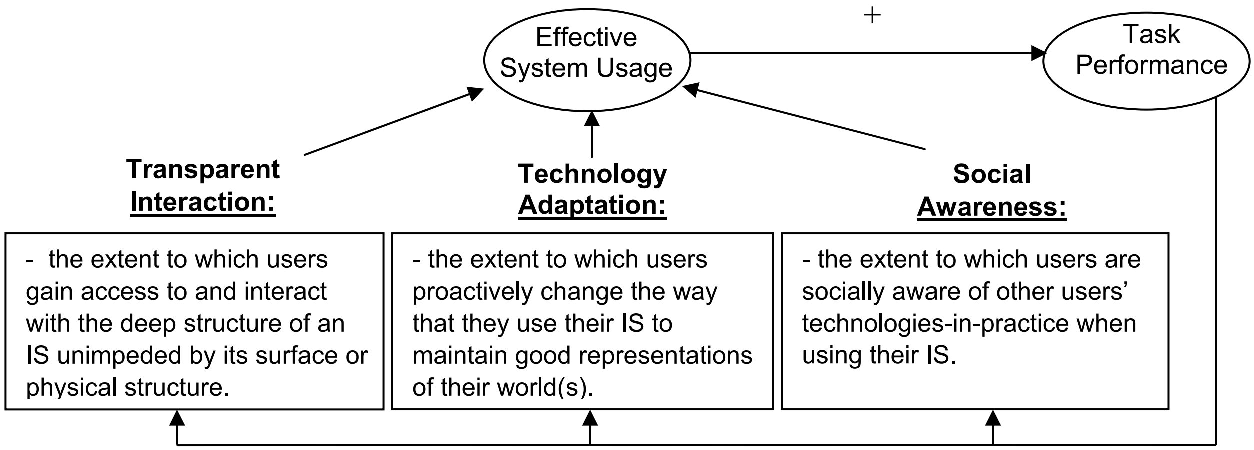
In the prior section on the *structure* of usage, we defined usage as an activity and we examined its existence and form based on the nature of this activity. When examining the *function* of usage, a researcher may be interested in the *entire* usage activity or, instead, one or more specific *aspects* of the activity relevant for theory being studied. In the selection stage, a research must carry out two steps to select and specify the relevant elements and measures of usage for his/her theory.

Step 1: Select Relevant Elements and Measures of System Usage.

Our definition of usage states that usage involves three elements (user, system, and task). When selecting usage measures, a researcher should first select the elements of usage that are most relevant for his/her theory, and then choose measures that are centered around the chosen element(s) that relate theoretically to the other construct(s) in the proposed model (Burton-Jones and Straub 2004, i.e., Chapter 2), e.g.:

1. *Measures of a* user's *state during usage (e.g., the user's cognitive or emotive state);*
2. *Measures of the way that a* system *is used during usage (e.g., the features used);*
3. *Measures of the* tasks *performed during usage (e.g., the number of tasks performed);*
4. *A combination of* system*,* user*, and/or* task*-centered measures.*

In some theoretical contexts, a researcher may need to measure just one precise aspect of usage (e.g., features used), in which case a uni-dimensional construct could be used. However, in the context of our theoretical model, we suggest that a *combination* approach is required to achieve a content-valid measure of effective system usage. As Figure 3.5 shows, effective usage in our theory is a multidimensional construct (Edwards 2001) formed by three measures (subconstructs): *transparent interaction*, *technology adaptation*, and *social awareness*. We lack sufficient space to completely explain the derivation of each subconstruct, but Table 3.8 briefly explains each one.



* We show this model at just one level for convenience, but it applies at multiple levels (per Figure 3.4).

Figure 3.5: Functional Relationship Between System Usage and Downstream Impacts*

Table 3.8: Theoretical Basis for the Three Subconstructs of Effective System Usage

Subconstruct & Key Reference	Elements of Usage Measured	Theoretical Basis for the Importance of each Subconstruct
Transparent Interaction (Weber 1997)	*System:* The measure reflects the way that the system is used.	IS comprise three parts: *surface* structure (the facilities that allow users to interact with the IS), *deep* structure (the data and functions used to represent users' world(s)), and *physical* structure (how surface and deep structures are mapped onto the underlying technology used to operate the IS). The benefit that users obtain from an IS stems from its deep structure. Thus, effective usage requires unimpeded access and interaction with this deep structure (i.e., "transparent interaction").
Technology Adaptation (Majchrzak et al. 2000)	*System and Task:* The measure reflects the way that the *system* is used or adapted for a *task*.	IS deep structure represents users' world(s) partially and problematically. Thus, users need to appropriate the deep structure, and employ it in new ways over time to maintain and create effective representations of their world(s). Such "technology adaptations" are an essential aspect of effective usage.
Social Awareness (Davern 1996)	*User:* The measure captures the *user's* cognitive state during usage.	Users' understanding of their world(s) and the IS deep structure(s) are imperfect. Users need to be aware of the ways that other members in their collective interact with and appropriate IS to learn the nature of effective usage in their collective. Such "social awareness" in use supports effective usage.

Step 2: Specify in what Form Each Measure is Functionally Important.

Once a researcher has selected the relevant measures of usage, s/he must determine whether the functional importance of each measure is tied to a particular structural form. This is due to *equifinality*—the notion that different patterns can have the same functional outcomes

(Meyer et al. 1993). For example, individual members of a collective may have different scores for the three subconstructs of effective usage. At the collective level, this pattern could be *structurally important* if it provides evidence for the existence of usage at the collective level, but the *particular* pattern may not be *functionally important* (i.e., it may not have a different relationship with other constructs in the theory). The possibility that a measure can be structurally but not functionally important means that researchers must not only theorize *whether* certain structural forms are possible; they must identify *which* forms should have stronger ties with other construct(s) in the theory (Table 3.9). We provide illustrative data in Table 3.10 to give a concrete example of this in the case of our model.

Table 3.9: The Structural and Functional Importance of Usage Measures

Dimension	Description	Reason for Importance	Example of Potential Error
Structural Importance	A distribution in the scores of a measure is structurally important if it manifests the existence of the collective construct.	If a researcher does not know the structural importance of a distribution of scores, s/he may conclude that a collective construct does not exist when in fact it does.	If a researcher incorrectly assumes that collective usage must be shared, s/he will fail to identify instances of collective usage that exist in the form of configurations.
Functional Importance	A distribution in the scores of a measure is functionally important if it has a stronger or weaker relationship than other distributions with another construct in the theory.	If a researcher does not know the functional importance of a distribution of scores, s/he may draw inaccurate conclusions about the relationships among constructs in the theory.	If a researcher incorrectly assumes that two patterns of usage have the same relationship with task performance, s/he may obtain misleading results in tests of the relationship between effective usage and performance.

Table 3.10: Illustrative Data for Analyzing Structural and Functional Importance

Group 1	TI	TA	SA	EU	Group 2	TI	TA	SA	EU	Group 3	TI	TA	SA	EU
User 1	3	3	3	9	User 1	4	2	3	9	User 1	4	2	2	8
User 2	3	3	3	9	User 2	4	2	3	9	User 2	4	2	4	10
User 3	3	3	3	9	User 3	4	2	3	9	User 3	2	4	4	10
User 4	3	3	3	9	User 4	4	2	3	9	User 4	2	4	2	8
Average	3	3	3	9	Average	4	2	3	9	Average	3	3	3	9

Key: TI (Transparent Interaction), TA (Technology Adaptation), SA (Social Awareness), EU (Overall Effective Usage).
The values represent scores from 1 (low) to 5 (high). EU is an equally weighted aggregation of TI, TA, and SA.
We only show one period for simplicity; we ask the reader to assume that the data are stable across time.

Our example in Table 3.10 shows three groups with three different distributions of scores:

- Group 1 shows shared scores across users and across the three subconstructs;
- Group 2 shows shared scores across users and a configuration across subconstructs;
- Group 3 shows a configuration of scores across users and across the three subconstructs.

Consider, first, the distribution of scores in Group 1. In this case, collective usage is *shared* as all members have the same scores for effective system usage (i.e., 9 out of a possible score of 15), as well as identical scores for its subconstructs. Next, consider Group 2. This group displays a configuration of scores across the three subconstructs but shared scores across users. Because the pattern is identical across users, the individual members and the group all have the same score for effective system usage, thus effective usage is again *shared*. Groups 1 and 2 give a good example of equifinality. Because effective system usage in our model is a higher-order, aggregate construct (Edwards 2001), the three subconstructs form (rather than reflect) effective system usage. Thus, we assume that two different individuals or groups (such as Groups 1 and 2 in Table 3.10) can have the same effective usage overall (i.e., 9 out of a possible score of 15), yet have different scores on each subconstruct, signifying different ways to achieve effective usage.[28]

Finally, consider Group 3, in which the scores are patterned across users and subconstructs. If a researcher has a theoretical reason to believe that group-level usage should be *shared* in her/his study, s/he must conclude that group-level usage does not exist in Group 3, only individual-level usage exists.[29] If there is a theoretical reason to support a configuration view of usage, then the distribution of scores in Group 3 would be considered structurally important (Table 3.9) and the data could be aggregated to the group level. The researcher must

[28] We do not claim that all models of effective system usage should have this property of equifinality, but management scholars advise other researchers to be aware that such equifinality is possible (Meyer et al. 1993).
[29] This is because if a researcher assumes in a particular context that group-level usage is shared, s/he must run tests of homogeneity within groups to identify those that are sufficiently homogenous to exhibit group-level usage (Castro 2002). As Group 3 has distinct patterns across users, it would fail tests of homogeneity and should not be aggregated.

then determine whether the specific configuration is functionally important (Table 3.9). If the pattern of scores in Group 3 is *not* expected to have a different theoretical relationship with performance (i.e., different from the scores in Groups 1 and 2), then the pattern in Group 3 would be not be considered functionally important and the researcher need not account differently for Group 3's distinct pattern in her/his tests (e.g., s/he could use the mean of each subconstruct to represent the group-level measure).

What theories can be used to determine whether configurations such as those in Group 3 are functionally important (i.e., lead to better or worse performance outcomes)? One of the most general theoretical bases is *congruence*: the notion that some configurations cohere or fit better than others for some or all contexts (Rousseau 2000; Meyer et al. 1993). Revisiting Group 3, we therefore need to determine whether its patterns represent congruence. We summarize findings from prior IS and organizational research in Table 3.11 and explain each in turn below.

Table 3.11: The Structural and Functional Importance of Configurations of Effective Usage

Subconstruct	Configurations expected based on past research	Configurations have structural importance	Configurations have functional importance
Transparent Interaction	Yes	Yes	Yes
Appropriation Moves	Yes	Yes	Yes
Social Awareness	Yes	Yes	No evidence at this stage

In terms of transparent interaction, a collective would ideally have members with uniformly high transparent interaction, but this is unlikely to occur in practice due to differences in experience and competence among members (Marcolin et al. 2001). For collectives with diverse members, the question is whether certain patterns of transparent interaction (as in Group 3, Table 3.10) cohere better than others under certain conditions. According to group research, some patterns of competence among members are more effective for different contexts. Steiner (1972) suggests that in conjunctive tasks (i.e., tasks in which each member depends on a previous member), the least competent member's behavior drives group performance. Conversely, in disjunctive tasks (i.e., tasks in which group output is chosen from one member's contributions), the most competent member's behavior drives group performance. Applying this logic to the groups in Table 3.10 suggests that the specific pattern of transparent interaction in Group 3 is functionally important (per Table 3.11) because depending on the task, a heterogeneous pattern of transparent interaction at the group level could be better (for disjunctive tasks) or worse (for conjunctive tasks) than a shared or homogeneous level of transparent interaction such as in Group 1.

In terms of the second subconstruct, technology adaptation, research suggests that patterns can be functionally important, depending on the type of adaptation that they reflect. Studies of agency distinguish between two types of adaptive behavior: practical evaluative behavior in which agents adapt to immediate, practical concerns, and projective behavior in which agents explore possibilities for the future (Emirbayer and Mische 1998). In terms of configurations of *practical evaluative* behavior, there is insufficient theory to conclude whether patterns of such behavior lead to greater outcomes (i.e., they are not clearly functionally important), but Majchrzak et al. (2000) showed that such patterns can occur in practice (i.e., they are structurally important):

> …adaptations may become discontinuous [i.e., configural] when the costs to change are very high, and when the technology is large or complex; but when the costs to not adapt are higher, and when the technology is more malleable, adaptations may become ongoing, if not continuous [i.e., shared] (p. 594).

In terms of configurations of *projective* behavior, research suggests that patterns of such behavior are both structurally and functionally important. Support for this stems from March's (1991) theory that individuals and collectives benefit from a mix of exploitation and exploration. Exploitation refers to routinization and implementation of existing practices, whereas exploration refers to the search or discovery of novel, innovative practices. Projective adaptations are exploratory in nature. According to March (1991), there is a tension between exploitation and exploration because exploitation tends to drive out exploration due to its higher certainty, speed, and clarity of feedback. Short-term performance can benefit from exploitation alone, but long-term performance will suffer from either (a) high exploitation alone (because it can trap one in suboptimal routines) or (b) high exploration alone (because it yields high costs with few benefits) (Argote and Ophir 2002). March's theory supports a coherence perspective because it suggests that effective system usage over the long run will not stem from *uniform* degrees of projective adaptation (whether high or low) across members of a collective and/or over time, but rather will stem from *patterns* of high and low adaptation. A collective could enact such patterns (a) across members at a point in time (i.e., by some exploring and others exploiting), (b) within members across time (i.e., by individuals exploring and exploiting at different times), or (c) across members and time (per Table 3.7, Models I, III, and IV). Therefore, applying March's (1991) logic to the technology adaptation scores in Group 3 (Table 3.10) suggests that such a heterogeneous pattern of scores would be more effective than one that displayed uniform levels of adaptation across all members (e.g., as in Groups 1-2). Edmondson (2002) provides a good example of this, finding that different groups in a firm engaged in different degrees of exploration, apparently learning more effectively overall by overcoming each other's deficiencies. Likewise, Accenture has long had an "advanced technology practice" in its Chicago headquarters, which experiments with and evaluates emerging technologies, before deciding which ones should be adopting by other offices. This represents a distinct usage configuration, since one unit (the "advanced technology practice") uses new technologies in a deliberately different manner than other units who are expected to use these technologies according to prescribed methodologies. Thus, we suggest that the patterns of scores for adaptation in Group 3 are both structurally and functionally important (per Table 3.11).[30]

Finally, in terms of the third subconstruct, social awareness, past research suggests that different collectives (and different members within collectives) enact varying degrees of social awareness. For example, Mackay (1990) observed a large group of email users and observed the emergence of three informal subgroups: *normal users* who rarely modified the system (low technology adaptation), *gurus* who modified the IS extensively (high technology adaptation), and *translators* who publicized and disseminated the changes made by gurus to other users (high social awareness). Despite this empirical evidence of the existence of varied social awareness practices, we are aware of no general theory that suggests that patterns of social awareness within or across collectives are inevitably more effective than homogeneous levels of social awareness. Therefore, we conclude that such patterns are important from a structural, but not a functional, perspective.

Implications for operationalizing measures of collective usage.

Just as the proposed principles for the structure of usage have implications for measuring the construct, so too do the principles for its function. Specifically, if configural constructs are functionally important, positivist researchers must identify ways to operationalize them. Returning to our example, researchers must seek metrics to operationalize the configurations of transparent interaction and technology adaptation in Table 3.10. Researchers generally capture data on configurations using measures of heterogeneity (Table 3.2). Because of the link between

[30] It is important to make explicit an issue that has remained implicit so far, namely that individual and collective benefits from system usage may not be perfectly aligned. This can have untoward effects for both individuals and collectives. For example, a collective may enact a pattern of use that is in the collective's interest but not the interest of its individual members, and members may enact patterns of use that are in their own interest but not in the collective interest. A detailed examination of this is outside the scope of the paper but we return to it briefly in the Implications section.

configurations and coherence (Rousseau 2000), we can draw upon techniques for operationalizing "fit" (Venkatraman 1989) together with recent typologies of multilevel constructs (Chen et al. 2004) to summarize available approaches for operationalizing configural constructs (see Table 3.12).

Table 3.12: Operationalizing Configural Measures

	Method of Measurement	
Approach	*Qualitative*	*Quantitative*
Criterion-free	Represent the configuration using a categorization or label that describes the aspect of the pattern of interest. *Techniques: Variables for I, S, P, C*	Represent the configuration using a variable that quantifies the aspect of the pattern of interest. *Techniques: Variables for I, S, P, C*
Criterion-specific	Represent the degree to which the configuration deviates from an ideal pattern using a categorization or label that describes the degree of deviation. *Techniques: Distance from ideal P or C*	Represent the degree to which the configuration deviates from an ideal pattern using a variable that quantifies the degree of deviation. *Techniques: Distance from ideal P or C*
Applicable techniques:		
I: Induced categories (Lee et al. 2004)	Use the group's scores to induce categories from qualitative or quantitative cluster analysis and assign collectives to their respective categories. Compare membership in each cluster to variance in the dependent variable (DV).	
S: Selected score (Chen et al. 2004)	Use a single score selected from one member of the group (e.g., minimum, maximum, or mode) to represent the group pattern. Compare variance in the selected score to variance in the DV.	
P: Pattern of scores (Kozlowski and Klein 2000)	Use the group's distribution of scores to deduce a single number (for quantitative research) or code (for qualitative research) that reflects the degree of dispersion (e.g., variance) or symmetry (e.g., proportion) in the pattern. Compare variance in the pattern (or variance in the distance from an ideal pattern) to variance in the DV.	
C: Combined (Straub et al. 2004a)	Use the group's distribution of scores to calculate a single, weighted value (for quantitative research) or a code (for qualitative research) that *combines* the strength of the scores (e.g., mean) and the pattern across scores within the group (e.g., range, variance, or proportion). Compare variance in the combined measure (or variance in the distance from the ideal combined measure) to variance in the DV.	

To apply the techniques in Table 3.12, consider the group "averages" in Table 3.10. Of the three subconstructs, only social awareness can be reflected by an average at the group-level because it is the only subconstruct for which we argue that the specific configuration of scores is not functionally important (Table 3.11). For the other two subconstructs (transparent interaction and technology adaptation) we must choose an approach from Table 3.12. The first step is to choose a criterion-free or a criterion-specific approach. For most multilevel models of system usage, a criterion-free approach will be appropriate, but in the case of our multilevel model, since effective system usage is an *evaluation*, a criterion-specific measure is necessary (Venkatraman 1989). The second step is to choose a suitable technique for operationalizing the group's scores (per Table 3.12). For transparent interaction, we previously argued that the important aspects of the patterns are the minimum/maximum, so the selected score technique is appropriate (Table 3.12). For technology adaptation, we argued that the important aspects of the pattern are the dispersion (e.g., variance) and strength (e.g., mean) in the scores of each group. A *combined* measure that weights the strength and dispersion of scores is appropriate (per Table 3.12) because March's theory does not privilege exploration over exploitation or vice versa, suggesting that a *moderate* amount of exploratory behavior is ideal, with this moderate level being enacted via a pattern of high and low levels across members, over time, or both (per Table 3.7). This implies that in the context of Table 3.10, the ideal pattern of exploratory behavior has a mean value reflecting a moderate level of technology adaptation (e.g., 3 on a 5-point scale) with a high variance. Based on this insight, Group 3 would have the highest score for technology adaptation (of the groups depicted in Table 3.10), followed by Group 1 (which has a lower variance, but identical mean), and then Group 2 (which has both a lower mean and less variance).

Following Table 3.12, as effective system usage is a criterion-specific construct, researchers would create the measure for overall effective system usage by using a distance measure to capture the deviation of each member from the ideal level on each subconstruct, and then aggregating these deviation scores to determine the overall level of effective usage. Quantitative researchers could use techniques such as partial least squares to create the aggregate (i.e., formative) construct, while qualitative researches would create a code or label to characterize the overall effectiveness.[31]

3.3.3.3 Specifying the Functional Form

Once the relevant properties of usage have been specified, one must specify its functional form in the theory (Table 3.4). In this section, we use our model in Figure 3.4 as an example of how to carry out this step. Multilevel researchers can choose from over eight functional forms (per Table 3.3), but past studies of the system usage→ performance link have examined only two forms, testing single-level models at an *individual* level (Model 1, Table 3.3) (Goodhue and Thompson 1995) and a *collective* level (Model 3, Table 3.3) (Dennis et al. 2001, Devaraj and Kohli 2003). The proposed model in Figure 3.4 extends past studies by explaining the usage→performance link over both levels. Researchers must carry out two steps to specify the function of usage in such a model (per Table 3.4):

Step 1: Define the functional relationship between individual usage and collective usage.

The proposed theory of effective system usage and task performance suggests that effective usage comprises the same three subconstructs at an individual and collective level (Figure 3.5) and that there is a reciprocal relationship between effective usage at the individual and collective levels (per the vertical arrows in Figure 3.4). Following distinction between canonical and non-canonical practice, we suggest that the reciprocal links between individual and collective usage can be defined in terms of enactment and assignment. 'Enactment' refers to what members of a collective actually do whereas 'assignment' refers to what an institution

[31] Although qualitative and quantitative researchers can operationalize configural constructs in their own way, the literature provides more guidance for quantitative researchers, such as guidance for calculating combined measures (Straub et al. 2004), inducing configurations from cluster analysis (Lee et al. 2004), and calculating distance measures (Doty and Glick 1994; Doty et al. 1993). A detailed discussion of such work is outside the scope of this study (Chen et al. 2004; Hofmann and Jones 2004), but we return to the need for more research on operationalizing and validating multilevel and configural constructs in the Implications section.

assigns members to do (e.g., via mandatory rules and role assignments) (Gallivan 2001). We propose that the influence of individual usage on collective usage—the upward arrow in Figure 3.4—is one of *enactment*, i.e., collective usage is formed bottom-up from the enactments of the members in the collective. In contrast, we suggest that the influence of collective usage on individual usage—the downward arrow in Figure 3.4—is one of both *enactment* and *assignment*. This is because individual use of systems is influenced by the enactments of other individuals in the collective as well as the formal rules and assignments made by the collective itself (Orlikowski et al. 1995).

Step 2: Define the functional relationship between usage and its related constructs.

As Figure 3.4 shows, we theorize a reciprocal relationship between effective system usage and task performance at each level. This assumes that users learn from their performance outcomes and adjust their usage behaviors over time (Goodhue and Thompson 1995). As shown in Figure 3.5, the functional relationship between effective usage and performance is expected to be positive, i.e., effective usage will increase task performance at both individual and collective levels. However, the feedback relationship between performance and effective usage is unpredictable because learning is a discovery process and user responses to outcomes may not always be advantageous.

When comparing the functional relationships that we specified in Figure 3.4 with the eight possible functional relationships in Table 3.3, it is clear that more complex relationships could, in principle, be theorized. For example, our model in Figure 3.4 specifies no *direct* relationships across levels of analysis (e.g., diagonal lines from individual usage to collective performance or from collective usage to individual performance). We omit such cross-level relationships because we lack strong theoretical grounds to support them at this stage. Specifically, we believe that changes in effective system usage will lead to (a) changes in task performance at the same level of analysis and (b) changes in system usage at a higher (or lower) level of analysis, which will, in turn, impact performance at that same level of analysis (per Figure 3.4). We do not disclaim the possibility of a direct (i.e., diagonal) cross-level relationship but we know of no theory at this stage to support such an effect over and above the effects already included in the model. In addition to the absence of these direct, cross-level effects, Figure 3.4 also omits *moderating* effects. It is certainly possible that moderating variables (e.g., task type) might moderate the links in Figure 3.4; we omit them and any discussion of complex interaction terms and other non-linear relationships only for reasons of parsimony.

3.3.3.4 Specifying the Time Frame

Following Table 3.4, the final stage when defining a multilevel theory is to specify time frames. Two aspects of time frames must be considered:

Step 1. Specify the time it takes for a construct to influence its higher- or lower-level analog.

Applying this step to our proposed model, we must specify the time it takes for a change in effective system usage at the individual level to impact its respective construct at the collective level and vice versa (i.e., the vertical arrows in Figure 3.4). Past research suggests that the influence of changes in individual system usage on collective usage is *gradual*, as it requires coordination among individuals, dyads, groups, and so on, as change diffuses across the collective (Barley 1990) (per Table 3.3). On the other hand, past literature suggests that the reciprocal influence of collective usage on individual usage can be rapid *or* gradual. Rapid changes in individual usage will generally stem from collective *assignment* (e.g., by the collective formally changing usage policies), whereas gradual changes will generally stem from collective enactment by members (e.g., by collective usage providing an environment that constrains and enables changes by individual members; DeSanctis and Poole 1994).

In addition to explaining how much time it takes for individual and collective usage to influence each other *in general*, it is important to recognize that many factors could influence the pace of these changes. While consideration of external variables is outside the scope of the paper, we can identify particular *states* of our model that suggest faster or slower change (Weber 2003c). In this light, we suggest that one subconstruct of our model—social awareness—is a key determinant of the pace of change. Specifically, we propose that the impact of a change in collective usage on individual usage is faster for *individuals* with higher social awareness, and reciprocally, the impact of a change in individual usage on collective usage is faster for *collectives* with higher social awareness (per Figure 3.6). Although this temporal relationship is specific to just one of the sub-constructs of our model (social awareness), researchers should consider both endogenous and exogenous enablers and constraints on the time it takes for changes to transfer across levels when developing multilevel theories of usage. Interpretive research has examined the time over which individual and collective usage influence each other (Orlikowski 1996, Barley 1990), but no positivist research has studied such temporal issues.

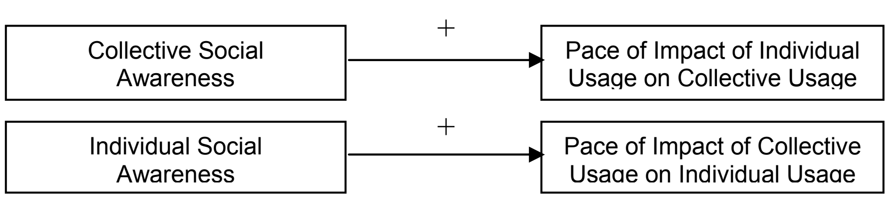


Figure 3.6: Specifying States of the Usage Construct that Influence its Pace of Change

Step 2. Specify the time it takes for a construct to influence its related construct in the theory.

Once a researcher has specified the time it takes for changes in individual and collective usage influence to one another, it is necessary to specify the time it takes for individual and/or collective usage to impact antecedents and/or consequences (i.e., the horizontal arrows in Figure 3.4). This step is applicable to any research model (not merely multilevel models), but it is rarely undertaken (Mitchell and James 2001). Just as in Step 1, various external factors could affect the time it takes for usage to influence performance, but we limit our attention to factors already in the model. Specifically, of the three subconstructs used to measure effective usage in Figure 3.5, we expect that a longer period will be needed to observe the impact of technology adaptation on task performance than to observe the benefits of increased transparent interaction and social awareness. As a result, as researchers increase the period over which they observes the relationship between effective usage and task performance, we expect that the benefit of technology adaptation on performance will become more salient (Figure 3.7). This follows March's (1991) theory that exploitation alone can improve performance in the short run, but that exploration is needed over the long run. Following Mitchell and James (2001), we suggest that the effect of time on the strength of the relationship between technology adaptation and task performance is best viewed in relation to the time over which the change in the independent variable (i.e., the "adaptation") is said to occur. In other words, we suggest that it may often be reasonable for a user (or researcher) to consider small adaptations to be part of a larger "adaptation" that the user undertakes over time. Small technology adaptations may have small impacts, but by helping a user to learn over time, a sequence of small adaptations may allow the individual user or collective to realize broader, more general adaptations that engender more certain benefits (Gallivan et al. 1994). Of course, this has a crucial implication: because the benefits of adaptations are noticeable over longer periods, cross-sectional research and brief longitudinal research will systematically understate the benefit of technology adaptations on performance.[32]

[32] This constitutes the basis for Karsten's (1999) critique of the groupware implementation literature. She argues that the period studied in most research was too short for any changes in behavioral routines or performance to be observed.

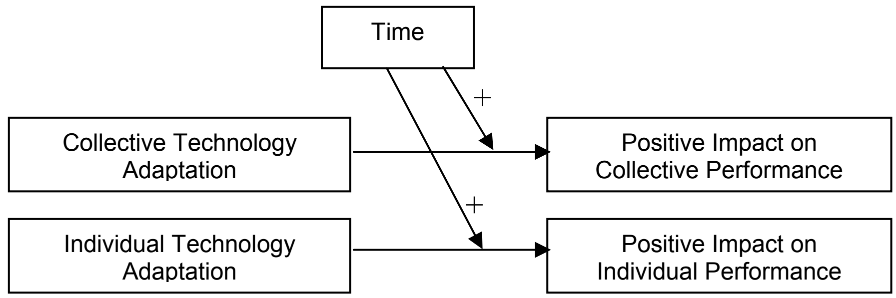


Figure 3.7: The Impact of Time on the Relationship between Usage and other Constructs

3.4 Implications

A decade ago, Zigurs (1993, p. 117) articulated the underlying tenet of this article, arguing that "system usage is an example of a deceptively simple construct that needs to be looked at more carefully." Because of the complexity of organizations, such deceptively simple constructs abound in organizational discourse. Consider the following quote from prominent accounting, strategic management, and marketing scholars, Simons, Mintzberg, and Basu (2002) respectively:

> In its April 14, 1997 issue, *Fortune* magazine wrote of IBM CEO Louis Gerstner, "In four years Gerstner has added more than $40 billion to IBM's market value." Admittedly, Lou Gerstner is an excellent CEO. But did he really do all that by himself?

As Simons et al. (2002) explain, the statement in *Fortune* magazine shows how simplistic notions of leader behavior mask the processes by which leaders and organizations create value. Many years ago, a recognition of this problem within scholarly research on leadership spurred widespread interest in the *multilevel* issues associated with leadership and the degree to which firms' outcomes can be attributed to leaders (Dansereau et al. 1984). As a result, leadership research has been much more proactive than other domains in developing theoretical and empirical approaches for creating and testing multilevel theories (Klein et al. 1994; Klein and Kozlowski 2000; Castro 2002; Bliese et al. 2002).

Although most IS researchers would agree that the above statement from *Fortune* is naïve, we rarely question those who talk about groups or organizations "using" IT. To date, no positivist studies and only a few interpretive studies have examined system usage from a multilevel perspective. However, if Deveraj and Kohli (2003) are correct in stating that system usage is the missing link between IT investments and organizational outcomes, then it is incumbent on researchers to begin identifying what collective system usage means and *how* collectives such as organizations and groups really "use" systems. We can observe a significant amount of single-level research on system usage (at an individual, group, and organizational level; see Appendix 3A), but the linkages *between* these levels remains totally unexplored. The steps, principles, and illustrations in this paper offer a first step towards investigating these linkages and developing a deeper, more integrated understanding of what system usage in groups and organizations means and how system usage impacts downstream consequences across levels and over time.

We are not the first IS researchers to recognize the need for multilevel research. Chan (2000) called for researchers to undertake multilevel research on IT impacts, Walczuch and Watson (2001) called for more precise analysis of multilevel data in GSS research, and Robey and Boudreau (2000) encouraged researchers to conduct multilevel studies to resolve conflicting empirical results. A decade ago, Harris (1994) even suggested that multilevel research was the key to understanding the organizational impacts from IT. Despite such calls, Chan (2000) found that IS researchers have been unwilling to move beyond single-level models (see Ang et al. 2002 for one exception). As Rousseau (1985) observed, it was understandable to conduct single-level research in the past because multilevel principles were immature. However, there is now a significant and mature body of multilevel research with accepted practices for theory building, research design, and data analysis, that is now well accepted in organizational research (Klein et al. 2000; Klein et al. 1999; Klein et al. 1994).

We see four key implications for future research on system usage that stem from adopting a multilevel perspective: 1) improving conceptions of the *structure* of usage by avoiding errors of inclusion and exclusion, 2) improving studies of the *function* of usage by examining cross-level paradoxes, 3) deepening insights into the *IT artifact* by conceptualizing "systems" in new ways, and 4) improving methodological techniques for undertaking and evaluating multilevel research.

First, in relation to the structure of usage, a strong benefit of a multilevel perspective is that it highlights key errors of inclusion and exclusion in past research. Errors of inclusion occur when researchers create a measure of collective usage (e.g., by aggregating individual use, or by asking a key informant about others' use in their group or organization) but do not verify that collective usage actually exists. Our research points to the need for both qualitative and quantitative researchers to present evidence that they are not committing errors of inclusion by inappropriately employing measures of collective usage in their studies. Despite the importance of errors of inclusion, errors of exclusion appear even more prevalent because no prior positivist studies have used measures of configural usage. We believe that configural usage should be the most dominant form of describing and measuring collective use, certainly from a structural perspective and often from a functional perspective. The reason for this is that, as we have argued, system usage cannot be global. Moreover, in order for system usage in a collective to be shared, it must meet two separate prerequisites, which we claim are rarely met: there must be homogeneity both across group members at a given point in time and consistency within group members over time. It is difficult for us to identify any system usage behavior that would be sufficiently regular to meet these two prerequisites and thus, constitute shared usage. In the absence of being global or shared, collective system usage must either be configural or a fallacy. If configural system usage is the most common form of collective usage, as we propose, it is clearly of great structural importance (per Table 3.11), as interpretive research has shown (Mackay 1990, Orlikowski 2000). However, we also believe that it has great functional importance because it will often have a stronger relationship with other constructs than shared usage, which we demonstrated with our proposed model of effective usage and task performance. Studying the emergence of usage configurations and their influence on other constructs offers rich opportunities for studying the nature and meaning of system usage in organizations.

In terms of studying the function of usage, a multilevel perspective provides a way to identify new research streams. For example, many interesting issues in organizations occur when effects at one level lead to no effect or an opposite effect at another level (Goodman 2000). We touched on such cross-level paradoxes in our model of effective system usage. Specifically, because patterns of effective usage can be enacted across members, changes in individual usage practices can improve individual performance (e.g., by increasing individual technology adaptation) but they may reduce or have no effect on collective performance if the individual's enactments move the collective further away from an ideal pattern of *collective* technology adaptation. As we indicated in footnote 30, the fact that benefits that occur at the individual or collective level may have no effect or a reverse effect at other levels is a general issue that arises because individual and organizational interests and practices are not always aligned (Goodman 2000). Further development of our proposed model and other multilevel models of usage offers fruitful avenues for understanding such cross-level paradoxes as well as other unintended outcomes of using IT in organizations (Markus and Robey 2004; Robey and Boudreau 2000).

A third implication of the multilevel perspective is that it reveals new ways to think about IS artifacts. As Orlikowski and Iacono (2001) protested, too many studies in IS insufficiently theorize the IS artifact. Researchers at all levels of analysis could study the IS artifact in more depth, but multilevel research offers a unique way to reach insights about one important aspect of systems: *the degree to which systems can be considered in aggregate terms as one considers the usage construct at a higher level of analysis.* To elucidate this point, we have advanced principles for aggregating system *usage;* other recent research has advanced principles for aggregating innovations or *systems* (Fichman 2000). We believe that these two sets of principles are highly complementary and that IS research would profit from extending these so that they do not merely support studies of collective and configural usage of single systems, but collections and configurations of systems. Such research would not only provide a deeper understanding of system usage, it would also enable researchers to obtain deeper understanding of IS artifacts by identifying how organizations achieve the necessary integration and coordination of IS artifacts *and* usage practices to be able to use their "portfolios" of systems effectively (Zmud 2001).

A final implication is that there are great opportunities for IS researchers to identify new empirical techniques for undertaking and evaluating multilevel research. For example, because organizations contain many embedded levels of analysis and may themselves be contained within higher levels, such as networks and industries (Rousseau 1985), researchers must carefully judge the relevant span of levels over which a theory and data set apply. For example, a researcher who studies GSS usage in virtual teams may be unable to (i) obtain accurate measures of system usage at each level (dyad, group, local team, global team) if there are too many levels, or (ii) obtain accurate measures at any one level if the size of the team is too large. Thus, researchers should avoid two common errors. First, they should avoid studying levels too far apart (e.g., the organization and individual level within a large organization). In such studies, too many unobserved levels (e.g., workgroup or department levels) may dissipate and distort the links between individual and collective constructs and render it difficult to determine if and how collective constructs emerge. Second, researchers should avoid studying a phenomenon at too broad a level (i.e., at a level with too many members). In such studies, relevant subgroups may be overlooked and the validity of collective constructs may be compromised (Chan 1998).

Like Arrow et al. (2000), we are aware that it is onerous to avoid these types of conceptual errors and conduct precise multilevel research, but we consider these difficulties to be opportunities for research and practice, rather than problems to be avoided. Interpretive studies have shown that multilevel theories can offer deep insights into how organizations work (Barley 1990). A multilevel perspective opens up exciting new directions for research on the antecedents and consequences of system usage and it invites researchers and practitioners to begin new discourses about the meaning of system usage, how organizational-level usage can be achieved, and how organizations can improve their use of systems in practice.

Certainly, many multilevel principles need to be refined, such as principles for defining construct validity for collective constructs (Chen et al. 2004; Hofmann and Jones 2004). However, researchers must be aware that this does not mean that multilevel research is any less rigorous than single-level research. In fact, the principles in this paper apply equally to single-level studies, because researchers who study phenomena at a single level (whether at the individual or collective level) must ensure that they control for the common fallacies found in single-level research (per Appendix 3B). Furthermore, no matter how rigorous one's research design and data analysis, *single-level studies cannot make reliable generalizations beyond that level*. Thus, the results of single-level studies are often of little value to practitioners who need to make interventions at multiple levels. For example, a firm may wish to determine employees' perceptions of and usage of a new discussion database. Single-level studies may find that employees use the system frequently and, thus, claim the system a success. Group- or community-level studies may find that few discussions occur, no ongoing communities emerge, and thus judge the system a failure. Only explicitly multilevel studies can resolve such apparently conflicting results (Goodman 2000). Multilevel approaches increase rigor by requiring researchers to specify how constructs emerge at the level at which they are being studied, and they enhance relevance by enabling broader, more integrated generalizations.

3.5 Conclusion

This paper advocates a multilevel perspective on system usage that integrates conceptions of system usage at an individual and collective level and highlights the need for research on the linkages between levels of analysis and the processes by which system usage leads to downstream consequences. In doing so, we provided detailed steps for building multilevel theories of system usage, devised principles for supporting each step, and provided a concrete example of how such research could be undertaken within the context of a specific, actionable theory of effective system usage. Although systems usage has been a key variable in IS research since the 1970's (DeLone and McLean 1992), it has received surprisingly few theoretical assessments. Furthermore, the theoretical conceptualizations of usage that have been offered at different levels of analysis show little to no integration. The principles that we advance in this paper for studying system usage are designed as an initial platform to support research that can explicitly bridge the gaps across levels of analysis that have been noted in past research (Chan 2000). Many prior studies have found inconsistent results for the organizational impact of IT (Heine et al. 2003; Markus and Robey 1988). Most studies, however, have been constrained to a single level of analysis. We contend that such single-level studies are destined to miss the important effects of usage and impacts that emerge across levels of analysis and over time. By providing a set of multilevel principles and a concrete, theoretically-grounded specification for the way in which system usage emerges, stabilizes, and changes over time, empirical researchers should be able to study the relationship between system usage and downstream outcomes from IT more effectively. A multilevel approach appears to be a promising way to resolve conflicting results from past studies, obtain rich insights into the nature and use of IT in organizations, and increase the rigor and relevance of IS research.

Appendix 3A: Conceptualizations of Usage in Past Research

In our review of the system usage literature, we found that most researchers choose measures based on their availability in past studies rather than a theoretical basis. Theoretical descriptions of the system usage construct are extremely rare. Figure 3A.1 illustrates four conceptualizations that have been offered: an individual-level information-processing model (Barkin and Dickson, 1977), a group-level appropriation model (DeSanctis and Poole 1994), an organizational-level diffusion model (Cooper and Zmud 1990), and a multi-level enactment model (Orlikowski 2000).

At an *individual* level, some have used Barkin and Dickson's (1977) information processing model of usage. However, as Trice and Treacy (1986) noted, most researchers just select from among the many available measures of usage. Table 3A.1 illustrates the diversity of individual usage measures that have been employed, including 14 primary usage measures and many minor variants.

At a *group* level, Dennis et al. (2001)explain that researchers have used two approaches to conceptualizing usage: an instrumental approach in which usage is assumed to be unproblematic, and an appropriation approach in which the features chosen by users depend heavily on contextual factors and the process is assumed to be more complex (Griffith 1999). The primary theoretical conceptualization of appropriation is DeSanctis and Poole's (1994) adaptive structuration theory.

At an *organizational* level, Zmud et al. (Cooper and Zmud 1990; Jasperson et al. 2005; Kwon and Zmud 1987; Massetti and Zmud 1996; Saga and Zmud 1994) emphasize "depth" of usage. Depth of usage is normally treated as a dependent variable. Various facets of depth include *acceptance* (staff begin using the IS), *routinization* (staff use the IS as a normal work activity), and *infusion* (staff use the IS extensively to integrate or redefine tasks) (Saga and Zmud

1994). The level of analysis in all of these cases is the organization or work-group, but measures of infusion at an individual level of analysis have recently been developed (Meister and Compeau 2002).

Finally, theorists of practice have conceptualized usage in a richly multilevel fashion (Lassila and Brancheau 1999; Webster 1998). (e.g., Orlikowski 2000). Orlikowski's (2000) model provides the exemplar, which characterizes different forms of usage as inertia, application, or change.

Researchers at each level have called for better conceptualizations of usage, e.g., DeLone and McLean (2003) at an individual level, Gopal and Prasad (2000) and Dennis et al. (2001) at a group level, Lucas (1993) and McKeen and Smith (1993) at an organizational level, and Lassila and Brancheau (1999) and Orlikowski (2000) in theories of practice.

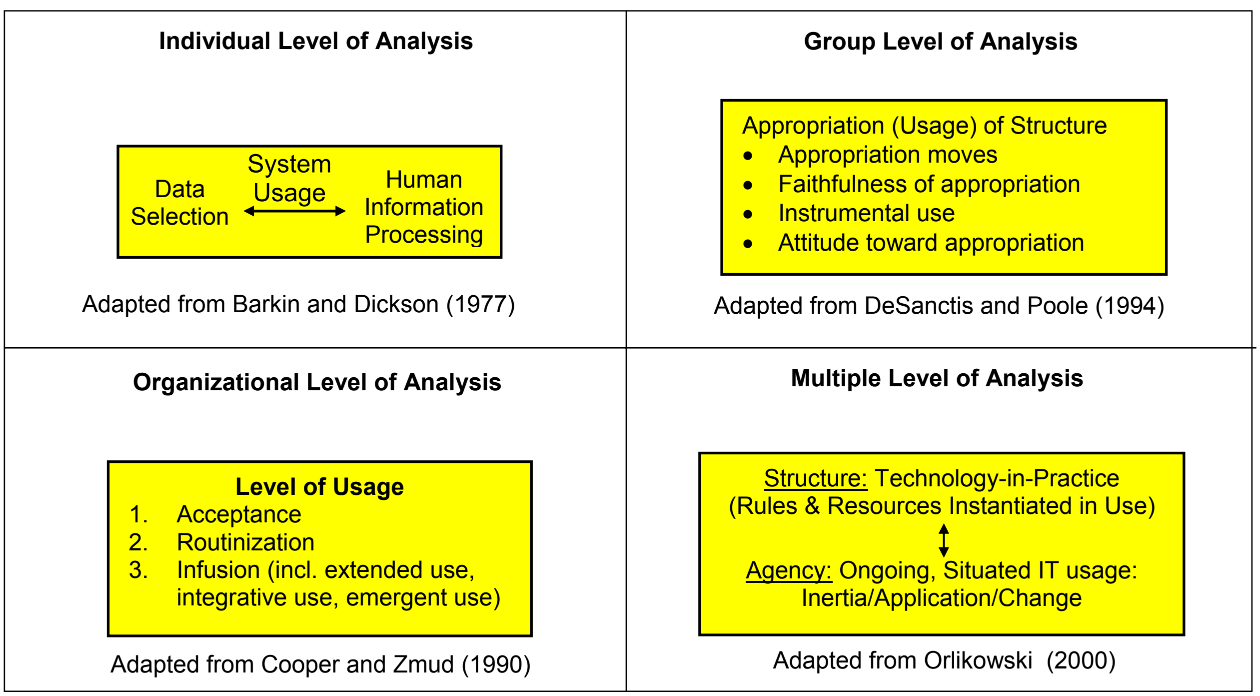


Figure 3A.1: Conceptualizations of System Usage across Levels of Analysis

Table 3A.1: Diverse Measures of System Usage at an Individual Level of Analysis†

Broad Measure	Individual Measures	Used as Independent Variable	Used as Dependent Variable
System usage measured as the use of information from an IS			
Extent of use	Number of reports or searches requested	✓	✓
Nature of use	Types of reports requested, general vs. specific use	✓	
Frequency of use	Frequency of requests for reports, number of times discuss or make decision using information		✓
System usage measured as the use of an IS			
Method of use	Direct versus indirect		✓
Extent of use	Number of systems, sessions, searches, displays, reports, functions, or messages; user's report of whether they are a light/medium/heavy user.	✓	✓
Proportion of use	Percentage of times use the IS to perform a task		✓
Duration of use	Connect time, hours per week	✓	✓
Frequency of use	Number of times use system, daily/weekly	✓	✓
Decision to use	Binary variable (use or not use)		✓
Voluntariness of use	Binary variable (voluntary or mandatory)		✓
Variety of use	Number of business tasks supported by the IS	✓	✓
Specificity of use	Specific versus general use		✓
Appropriateness of use	Appropriate versus inappropriate use	✓	✓
Dependence on use	Degree of dependence on use	✓	✓

†This list was induced from a sample of 48 IS articles in major journals from 1977-2005 (see Appendix 5A).

Appendix 3B: Motivations for Adopting Multilevel Theory

Two decades ago, Rousseau (1985) highlighted the two primary motivations for conducting multilevel research. First, from an empirical perspective, multilevel designs allow researchers to avoid four significant research fallacies that can occur in single-level research (see Table 3B.1).

Table 3B.1: Common Fallacies that Occur in Single-Level Empirical Research

Fallacy	Validity Threat	Description
Contextual fallacy	Internal validity	Occurs when observed relationships at a lower level are spurious because of a failure to account for higher-level factors that impact the relationship. For example, an observed correlation between individual system usage and individual performance could be spurious if the researcher had not tested the effect of correlated variables at a higher level of analysis, such as social norms.
Cross-level fallacy	Construct validity	Occurs when researchers neglect to specify the underlying mechanisms by which individual-level phenomena give rise to higher-level phenomena. For example, if *organizations* cannot use IS (but only *people* can use IS), then constructs such as 'organizational system usage' merely reflect anthropomorphization or metaphor. As Roberts et al. (1978) explain: "organizations don't behave (people do)."
Ecological fallacy	External validity	Occurs when lower-level relationships are incorrectly inferred from relationships at a higher level. Statistical relationships among aggregate data are often higher than corresponding relationships among individual data and can mask subgroup differences. Thus, a high correlation between organizational system usage and organizational performance does not imply that individual system usage is related to individual performance.
Atomistic fallacy	External validity	Occurs when relationships at a higher level are incorrectly inferred from relationships observed at a lower level. For example, a positive correlation between individual system usage and individual performance does not imply that an increase in organizational system usage will lead to an increase in organizational performance.

The fallacies in Table 3B.1 restrict the degree to which single-level research can be valid (Rousseau, 1985). Specifically, they imply that single level research designs can be valid only if:

(a) none of the constructs being examined emerge from attributes that exist at a lower level,
(b) a researcher need only generalize to a single level of an organization (e.g., the same level at which the data are analyzed), and
(c) none of the constructs being examined are affected by correlated variables at a higher level.

Because these assumptions are often violated in organizational research, and this problem cannot be resolved with strictly single-level designs (i.e., designs that do not acknowledge or control for multilevel processes), they provide a strong motivation to adopt alternative, multilevel designs.

The second motivation for adopting multilevel research is theoretical. As Goodman

(2000, p. 6-7) observes: "Our research, how we train each new generation of researchers, and our professional associations display a clear level bias, that is, we tend to focus on one level of analysis and implicitly make assumptions about the relations between the focal unit and other units of analysis." Recognition of this bias suggests two benefits of multilevel theory. First, multilevel theory opens up new research directions. Specifically, it suggests that research is needed to understand the linkages between levels of analysis. For example, Kozlowski and Klein (2000, p. 76) observe: "How individual cognition, affect, behavior, and other characteristics emerge to make contributions to group and organizational outcomes is largely an unchartered frontier." Second, multilevel theory can contribute towards organization-specific theories. As Rousseau (1985) suggests, the reason why most researchers adopt single-level theories is because they were trained in a reference discipline that traditionally operates at a single level of analysis (e.g., psychology, sociology, or economics). Consequently, multilevel theorizing offers an opportunity to create theories that contribute to a deeper understanding of organizations, rather than merely a deeper understanding of phenomena studied by reference disciplines that happen to exist in organizations, as House et al. (1995) explain:

> ... organizational researchers will never be better than psychologists at understanding individuals in general, better than economists at studying large-scale market forces, nor better than sociologists at studying social forces. ***Only an organizational science can address effectively the complexities of the relationships between the units at different levels of analysis that comprise organizations*** (p. 74-75) (emphasis in original).

As Rousseau (1985) noted, the limitations of single-level research have been known for many years, but alternative multilevel designs were not initially feasible, in part, due to a lack of accepted statistical and methodological procedures. These two motivations for adopting multilevel theories—empirical and theoretical—spurred a considerable amount of research on multilevel techniques (Raudenbush and Bryk 2002, Klein et al. 1994, 1999), to the point that now multilevel theories are not only desirable, but also feasible (Klein and Kozlowski 2000).

Chapter 4

A Multilevel Investigation of the Relationship Between System Usage and Task Performance[33]

Abstract

System usage is one of the most widely implemented constructs in information systems research. Yet, to date, the difference between individual and collective system usage, and the links between system usage and its downstream consequences across levels of analysis have not been studied. The present study addresses these gaps by investigating the multilevel character of the relationship between system usage and task performance. The study focuses on three aspects of this multilevel relationship: context, structure, and cross-level relationships.

In relation to *context*, we propose that collectives are inherently different from mere collections of individuals and, as a result, when the usage-performance relationship is aggregated from the individual level to the collective level, the strength of the relationship differs when the individuals studied are in real collectives versus "nominal" collectives (i.e., mere collections of independent individuals). In relation to *structure*, we submit that collective usage is more than the sum of individual usage because it comprises interdependencies that occur among members' usage, and propose that these interdependencies influence collective performance. Finally, in relation to *cross-level relationships*, we propose that aspects of collective usage influence individual usage and individual performance.

We test these propositions through an experiment in which 34 nominal groups (comprising 116 individuals) and 139 groups (comprising 517 individuals) used spreadsheet software to perform financial analysis tasks. The results confirm the importance of context, structure, and cross-level relationships, and reveal important new directions for research on what it means to use systems and how systems lead to consequences across levels of analysis.

Keywords: information system usage, multilevel theory, cross-level relationship, IT impacts, IS success, performance, measures, validation.

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[33] Burton-Jones, A., and Straub, D. "A Multilevel Investigation of the Relationship Between System Usage and Task Performance," Working paper, Department of Computer Information Systems, Georgia State University, 2005.

4.1 Introduction

Information systems are designed to serve individual and collective (e.g., group or organizational) purposes. To achieve these ends, information systems must be utilized. The importance of system usage as a necessary link between systems and their outcomes is widely recognized at an individual (Goodhue and Thompson 1995), group (Dennis et al. 2001), and organizational (Devaraj and Kohli 2003) level of analysis (see Figure 4.1).

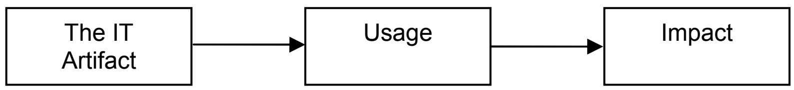

Figure 4.1: IT Impacts Stem from Use (Benbasat and Zmud 2003)

Given the importance of system usage as the link between an IS and its impacts, it is not surprising that many researchers have investigated the system usage construct. For example, researchers have conceptualized system usage at the individual (Barkin and Dickson 1977), group (DeSanctis and Poole 1994), and organizational (Cooper and Zmud 1990) levels. Nevertheless, there have been repeated calls to examine it more carefully (DeLone and McLean 2003; Trice and Treacy 1986). For example, at an individual level of analysis, DeLone and McLean (2003) argued that "the problem to date has been a too simplistic definition of this complex variable" (p. 16). Likewise, at a group level of analysis, Zigurs (1993) has argued that "system usage is an example of a deceptively simple construct that needs to be looked at more carefully" (p. 117).

Although many aspects of system usage could be studied, this study examines one critical aspect: the multilevel character of the relationship between system usage and its consequences. We focus on this aspect for two reasons. First, despite much research on system usage at the individual and collective levels, little is known about how collective usage and individual usage differ. Even though many people talk about groups and organizations using systems, how can collectives really "use" systems? What is collective usage? How is it related to, but distinct from, individual usage? With the exception of one recent conceptual study (Burton-Jones and Gallivan 2004) (i.e., Chapter 3), these questions remain unanswered. To date, most researchers at the group and organizational levels have created measures of collective usage that are separate from and not explicitly connected to measures of usage at lower levels of analysis (Cooper and Zmud 1990; DeSanctis and Poole 1994; Zhu and Kraemer 2005). Others have measured collective usage by aggregating measures of individual usage to the collective level (e.g., via sums or means) (Devaraj and Kohli 2003; Easley et al. 2003). There has been no discussion to date of which of these schemes, if either, is appropriate.

In addition to there being no research on the difference between the usage *construct* at different levels of analysis, there has been no empirical research on the multilevel *relationships* between usage and its consequences such as performance. Several researchers have called for research to understand how IT use leads to performance outcomes both up and down levels of analysis (Chan 2000; Devaraj and Kohli 2000; Harris 1994). Nevertheless, researchers have been unwilling to move from their preferred level of analysis (Chan, 2000). This problem is also evident in the broader organizational literature. For instance, Kozlowski and Klein (2000) (p. 76) observe: "How individual cognition, affect, behavior, and other characteristics emerge to make contributions to group and organizational outcomes is largely an unchartered frontier."

The existence of these gaps in the literature is understandable given that multilevel theory, methods, and data analysis techniques are so new (Rousseau 1985). Nevertheless, the past decade has witnessed great progress in multilevel research and data analysis (Klein et al. 1999; Klein et al. 1994; Raudenbush and Bryk 2002), to the point that rigorous multilevel studies are now feasible (Goodman 2000; Klein and Kozlowski 2000).

We are certainly not the first to investigate multilevel aspects of system usage. Interpretive researchers have long studied how properties of collectives influence individual and collective usage (Barley 1990; Orlikowski 1993; Robey and Sahay 1996). There is also a nascent stream of positivist research examining the process by which collective usage influences individual usage perceptions (Contractor et al. 1996) and behavior (Angst and Agarwal 2004; Gallivan et al. 2005). However, no empirical study has clearly distinguished between individual or collective usage, nor, with the exception of one interpretive study (Orlikowski 2000), has any study specified the multilevel processes by which system usage can lead to performance consequences. The present study addresses both of these gaps. To scope our research, we address these from a purely positivist perspective. We leave an interpretive account of these issues to future research.[34]

To address the gaps in past literature, the present study advances a new theoretical model of the relationship between system usage and task performance that specifically accounts for the difference, yet inherent link, between individual and collective system usage, and the multilevel relationship between system usage and task performance. We also report on an empirical test of the model in which 34 nominal groups (i.e., groups 'in name only') (comprising 116 individuals) and 139 groups (comprising 517 individuals) used spreadsheet software to perform a financial analysis task. The results of the experiment highlight the importance of understanding the *context* of collectives when studying the usage-performance relationship; demonstrate how the *structure* of collective usage is related to, but distinct from individual usage; and reveal important *cross-level relationships* between collective system usage and individual performance. Together, the theoretical model and empirical results contribute by providing a first step towards understanding the multilevel character of the usage-performance relationship and reveal rich directions for research on what it means to use systems and how systems lead to impacts across levels.

[34] Since the passing of logical positivism, all working positivists are, arguably, "post-positivists" (Suppe 1977). Nevertheless, terms are hard to change and we continue using "positivist" in its broad, informal sense, in which it refers to a large class of research concerned with theory testing, measurement, and validity.

4.2 Building a Multilevel Model of System Usage and Task Performance

We build a multilevel model of system usage and task performance in four steps. First, we build a baseline model, theorizing the relationship between individual system usage and individual task performance. Second, we investigate the effect of studying this relationship in different *contexts*, i.e., among independent individuals, collections of individuals, or collectives. Third, we extend this baseline model by examining how the *structure* of collective usage differs from that of individual usage. Finally, we extend the model by positing *cross-level relationships* between collective usage, individual usage, and individual task performance.

4.2.1 System Usage and Task Performance: A Baseline, Individual-Level Model

We adopt our baseline model from an existing study (Burton-Jones and Straub 2004) (i.e., Chapter 2). System usage is conceived as an activity that involves three elements: (1) a user, i.e., the subject using the system, (2) a system, i.e., the object being used, and (3) a task, i.e., the function being performed. Drawing on assumptions regarding each of these elements, we define system usage as: *a user's employment of one or more features of a system to perform a task.*[35] This definition enables us to distinguish between individual and collective usage, i.e., individual usage is where the user is an individual person; collective usage is where the user is a collective.

Burton-Jones and Straub (2004) suggest that researchers explain the link between system usage and other constructs by selecting *elements* of usage that are relevant in the theoretical context (i.e., system, user, and/or task) and by selecting *measures* of these elements that tie to the other constructs in the model. Using this procedure requires that we first define the other

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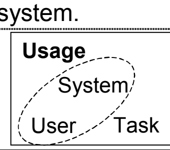
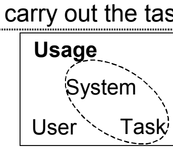
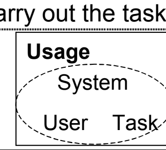
[35] Our assumptions regarding each element of usage are as follows (per Burton-Jones and Straub 2004):

- A *user* is a social actor. This implies that users are individuals or collectives who are using a system to perform one or more aspects of their task(s) (Lamb and Kling 2003).
- A *system* is an artifact that provides representations of one or more task domains. This implies that the system offers features designed to support aspects of those task domains (DeSanctis and Poole 1994; Griffith 1999).
- A *task* is a goal directed activity performed by an individual or collective. This implies that task outputs can be assessed in terms of pre-defined task requirements (Zigurs and Buckland 1998).

construct in our model: task performance. Following Burton-Jones and Straub (2004), we define task performance as: *an assessment of task outputs in terms of effectiveness*. Effectiveness, in turn, has two components: quality and quantity (Meister 1986). Thus, we model task performance as a higher-order construct formed by quality and quantity of work (see Figure 4.2).

We scope this study by focusing on the link between system usage and *short-run* task performance. As Table 4.1 shows, Burton-Jones and Straub (2004) suggest that in complex tasks, the link between system usage and task performance involves all elements of usage (i.e., system, user, and task). They also argue that the type of usage that ties most closely to short-run task performance is the extent to which a user *exploits* a system's features to perform the task (March 1991). Exploitive usage can, in turn, be measured as a higher-order construct formed by two sub-constructs: cognitive absorption and deep structure usage, as shown in Table 4.1. Figure 4.2 summarizes these arguments in a baseline model that we extend in subsequent sections.

Table 4.1: Selecting Measures of System Usage that Explain Short-run Task Performance*

Elements of system usage	*Elements*	Extent to which the user employs the system.	+	Extent to which the system is used to carry out the task.	=	Extent to which the user employs the system to carry out the task.
	Illustrations		+		=	
Measures of elements of system usage that explain short-run task performance	*Measures*	Cognitive absorption-in-use	+	Deep structure usage	=	Exploitive usage
	Definitions	The extent to which the user employs his/her cognitive resources when using the system.	+	The extent to which features in the system that support the task are used.	=	The extent to which the user exploits features in the system to carry out the task.

* Adapted from Burton-Jones and Straub (2004).

Key: the subscripts 'Exploit' and 'Short-run' indicate that the model specifies the relationship between exploitive usage and short-run task performance.

Figure 4.2: Baseline Model: Individual System Usage and Short-run Task Performance

4.2.2 Studying the Baseline Model: The Importance of Context

The proposed baseline model is restricted to the individual level. Thus, it is only a valid model for contexts in which individuals use systems independently of each other (Rousseau 1985). Nonetheless, it can be extended to support studies in other contexts. Figure 4.3 provides a summary of what contexts can be studied, how the model would need to be extended in these contexts, and the degree to which inferences could be made from results in these contexts.

		Context of Population	
		Independent Individuals	*Collectives of Individuals*
Level of Analysis	*Individual level*	*1.* Analysis is appropriate. Inferences are valid at the individual level, but not the collective level.	2. Need to extend the baseline model to account for cross-level effects. Inferences are valid at the individual level, but not the collective level.
	Collective level	*3.* Analysis is inappropriate. Inferences are invalid at the collective and individual levels.	*4.* Need to extend the baseline model to account for the structure of collective constructs. Inferences are valid at the collective level, but not the individual level.

Figure 4.3: Studying the Proposed Baseline Model in Different Contexts

Quadrant 1 (Figure 4.3) illustrates the default case for studying the baseline model. In this context, inferences can be drawn from the model to the individual level of analysis only.

Quadrant 2 (Figure 4.3) reflects a context in which individuals use a system as part of a collective. Here, the baseline model would need to be extended to account for contextual effects, else an observed relationship between usage and performance at the individual level may be spuriously caused by a correlated omitted variable at a higher level (Rousseau, 1985).[36] In this context, researchers must also limit their inferences to the individual level. This is because results at the individual level will not necessarily reflect those at the collective level (Klein et al. 1994).

[36] Another reason to control for contextual effects is due to statistics. Ordinary least-squares estimation assumes that residuals among errors are independent. This assumption is typically violated when individuals are subject to group influences, leading to biased parameter estimates, and increased type-1 error (Raudenbush and Bryk 2002).

Quadrant 3 (Figure 4.3) reflects a context in which the population comprises independent individuals, but the researcher aggregates measures to the collective level. This may seem illogical, but it could easily occur. For example, a researcher studying a consulting firm may be told that consulting teams employ software to collectively track the performance of engagements. However, in practice, consultants may not use the IS in the intended way, e.g., consultants may use the system *independently* (e.g., tracking individual tasks), not as a team (e.g., not pooling each member's work outputs to track the team's *overall* performance). If the researcher failed to verify the way teams *actually* used the IS, s/he may incorrectly aggregate the individual level measures to the team level. By so doing, the researcher would obtain measures of "nominal" teams (i.e., teams in name only). As Figure 4.3 shows, it would be illogical here to make inferences to the collective level. Nor could a researcher infer any findings back to the individual level, because collective-level relationships will not necessarily reflect those at an individual level (Klein et al. 1994).

Finally, quadrant 4 (Figure 4.3) reflects a context in which individuals use systems within collectives and the researcher analyzes data at the collective level. In this context, the model would need to be extended to ensure that it reflects the structure of the collective constructs. If this was done, the researcher could make inferences at the collective level, but again, s/he could not make inferences from the collective-level data to the individual level (Klein et al. 1994).

In summary, the baseline model needs to be extended in two main ways: (1) by specifying the structure of collective constructs, and (2) by accounting for cross-level effects. We discuss these below. Before doing so, it is necessary to highlight the importance of context when aggregating data. One way to measure collective usage in past research has been to aggregate measures of individual usage to the collective level (e.g., via sums or means) (Devaraj and Kohli 2003; Easley et al. 2003). As noted above, individual usage should only be aggregated to the collective level if collective usage actually exists. A failure to verify whether collective usage exists can have an empirical as well as a theoretical implication. This is because the strength of an individual-level X-Y relationship can change when aggregated to the collective level, with the direction of change depending on the nature of the true X-Y relationship and the degree of within-group homogeneity in the population (Robinson 1950; Rousseau 1985). To illustrate, Figure 4.4 shows a hypothetical, positive, X-Y relationship at an individual-level. As Figure 4.4 shows, if groups are relatively homogenous in terms of the measures variables, aggregating the individual-level measures to the group-level results in a *stronger* relationship than found at the individual level (i.e., same slope with lower variance), but if groups are heterogeneous in terms of the measured variables, aggregating the measures results in *no* relationship between the constructs.

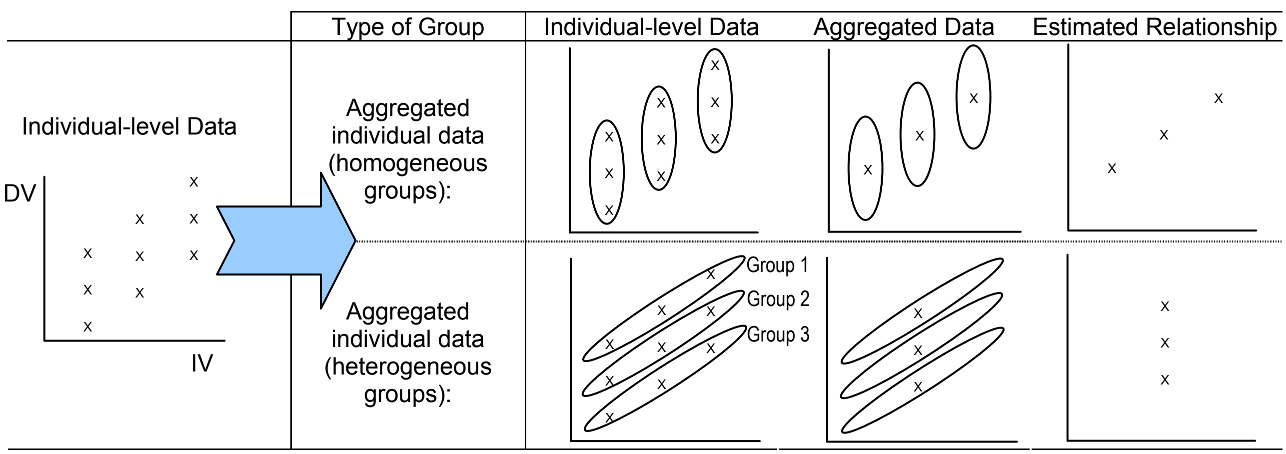


Figure 4.4: Example of Aggregating Data in Homogeneous vs. Heterogeneous Groups

Clearly, the example in Figure 4.4 could be simulated with different groupings and data; the point is that relationships can change when aggregating data from the individual to collective level, with the direction of the change being influenced by the nature of the collectives. This is important in practice because collectives often have higher degrees of homogeneity among members than found among random individuals in a population, resulting in relationships among aggregated data often being higher than corresponding relationships at an individual level (Robinson 1950; Rousseau 1985). Within-group homogeneity tends to occur in collectives for two reasons. First, similarity among individuals is a strong predictor of group formation because individuals tend to be located in space/time near similar individuals and tend to be attracted to such individuals (Bowers et al. 2000; Thompson and Fine 1999). Second, even if collectives are *designed* to be diverse (Bowers et al. 2000; Rodan and Galunic 2004), once formed, interactions within collectives often reduce individual differences. For example, members learn from each other (leading to similar knowledge) (Lave and Wenger 1991) and influence each other (leading to similar behaviors and beliefs) (Cialdini and Goldstein 2004), as studies in the usage literature show (Angst and Agarwal 2004; Gallivan et al. 2005). Of course, members of real collectives will not *always* be more homogeneous than random individuals, e.g., within-group conflict may decrease within-group homogeneity. Rather, our argument is simply that real collectives will have systematically different levels of homogeneity among members than found among random individuals. As a result, we expect that X-Y relationships aggregated to the collective level will differ when data come from real collectives (e.g., quadrant 4, Figure 4.3) than from nominal collectives (e.g., quadrant 3, Figure 4.3). We empirically test this prediction later in the paper.

4.2.3 Extending the Baseline Model: The Structure of Collective Usage and Performance

To study the usage-performance link at the collective level, researchers must not only verify the *context* of the users, but must account for the *structure* of the collective constructs. Past researchers have measured collective system usage by aggregating individual-level usage (e.g., via sums or means) (Devaraj and Kohli 2003; Easley et al. 2003), or setting individual usage aside and creating distinct group- or organizational-level measures (Cooper and Zmud 1990; DeSanctis and Poole 1994). From the perspective of *collective* constructs, both approaches are problematic. Measures of collective system usage that merely aggregate individual use implicitly assume that a collective is the *sum* of its parts. Measures of collective system usage that do not draw upon individual usage implicitly assume that usage can occur without individual users.

How can system usage and task performance differ in structure across levels of analysis? Morgeson and Hofmann (1999) suggest that the difference between individual and collective constructs can be understood in terms of their function and structure. The function of a construct is what it explains. This is often the same at each level of analysis. For example, in our theoretical model, usage at each level of analysis is expected to have a functional relationship with task performance. The structure of a construct is its composition. This generally differs across levels. For example, in our theoretical model, the structure of usage and performance differ across levels. Morgeson and Hofmann (1999) suggest that the difference in structure stems from the collective construct comprising the *interactions* among individuals. In other words, the "whole is more than the sum of its parts." Hofmann (2002) provides an example:

> "Individual ability allows individuals to receive, process, and respond to information in the external environment. It is perhaps self-evident that teams and organizations are also able to receive, interpret, and process information…. The functional outcomes of individual, team, and organizational ability are the same…. Clearly, however, the structure of this ability changes when moving from individuals to some larger collective. The structure of individual ability refers to scripts, schema, and other cognitive and biological factors, whereas the structure of ability for higher-level aggregates not only includes these cognitive and biological factors (since collectives are composed of individuals), but they also involve something more. This something more is the interaction between these individuals" (p. 250).

Our basic assumption, therefore, is that the two constructs in the proposed theoretical model (system usage and task performance) have the same functional relationship at each level (i.e., a positive relationship of system usage on task performance at the individual and collective levels), but the structure of the two constructs changes across levels. To determine the structure of the two constructs at each level, we refer to the original definitions:

- *System usage*: a user's employment of one or more features of a system to perform a task.
- *Task performance*: an assessment of task outputs in terms of effectiveness.

We first deal with task performance (see Figure 4.5). Because task performance is an assessment of *outputs*, we can assess this output via the same criteria (i.e., quantity and quality) at both the individual and collective level (Dennis et al. 2001; Meister 1986; Shadra et al. 1988). However, while the output is *assessed* via the same criteria at each level, the *nature* of the output itself differs. At the individual level, the output being assessed is the output that a single person produces. At the collective level, the output being assessed is the total output produced by a collective, which not only comprises the aggregation of individual output (i.e., 'the sum of the parts'), but also comprises irreducibly collective outputs, such as group decisions and group judgments (i.e., 'more than the sum of the parts'). In summary, as Figure 4.5 shows, we measure individual and collective performance by the same criteria (i.e., quality and quantity of work), but the measures in each case refer explicitly to the output produced at the specified level.

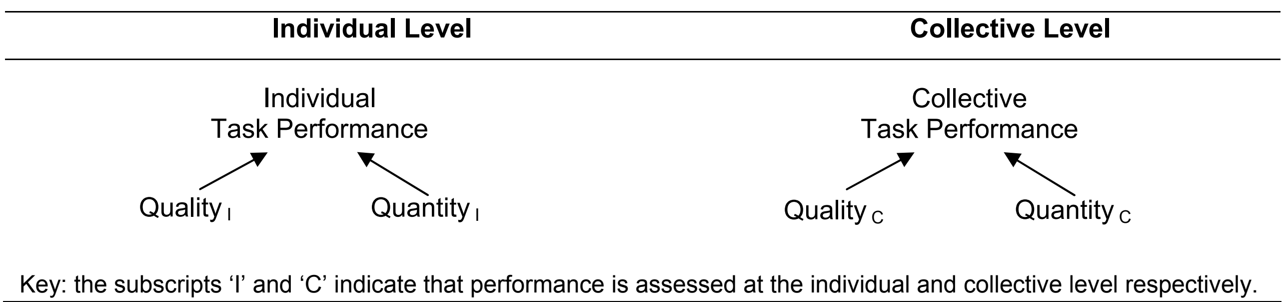


Figure 4.5: The Structure of Task Performance Across Levels

Whereas collective task performance refers to the *output* of a collective process, collective system usage refers to the collective *process* itself, i.e., the employment of a system. How can a collective 'use' a system? Following Morgeson and Hofmann (1999), we argue that collective usage includes aggregate individual usage (which in this case, refers to the sum of each member's cognitive absorption and deep structure usage), but comprises as well the interactions that occur among members during collective usage. Drawing on research on system usage in organizations (Crowston 1997; Karsten 2003), and theories of groups (Lindenberg 1997), we focus on one aspect of these interactions—interdependencies. Interdependencies are (p. 438) "patterns of action and interaction where two or more [entities] are mutually dependent on each other"

(Karsten 2003). As Figure 4.6 shows, we suggest that two elements of interdependence are relevant components of *collective* exploitive usage: *coordination-in-use* and *collaboration-in-use*.

Our choice of these two elements of interdependence stems from theories of groups, which suggest that collectives comprise three basic types of interdependencies: functional, cognitive, and structural (Table 4.2) (Lindenberg 1997). *Functional interdependence* stems from interactions among members to achieve individual and collective goals. For example, if the goal is to complete a task, functional interdependencies are all those relating to task completion. *Cognitive interdependencies* stem from the application of individual and collective cognition to solve task problems. For example, group members tend to have different types and levels of knowledge, which can lead to cognitive dependencies among members. Finally, *structural interdependencies* refer to relations formed by one's place in a network. For example, group members may complete a task in sequence, with each member dependent on prior members' task completion.

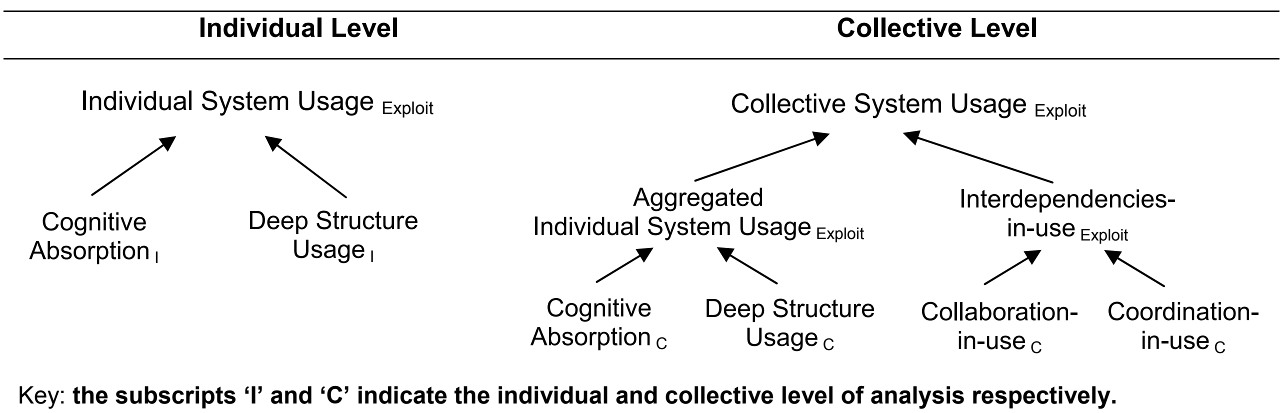


Figure 4.6: The Structure of System Usage Across Levels

Table 4.2: Types of Interdependencies

Interdependency	Type of Relationship	Relevance for Measuring Collective Usage
Functional	…forged between individuals to achieve individual and collective goals	Not easily applicable since it is broader than system usage.
Cognitive	…forged between individuals to reduce individual and collective uncertainty	A relevant measure of collective usage. Measured here via *collaboration-in-use*.
Structural	…forged between individuals based on their location in a network	A relevant measure of collective usage. Measured here by *coordination-in-use*.

As Table 4.2 indicates, it is important when measuring system usage that researchers focus only on those interdependencies that occur *in-use*. As a result, we suggest that functional interdependencies are too broad a concept for measuring interdependencies-*in-use*. Although they are good measures of task interdependence in general, we agree with Lamb and Kling (2003) that system usage is just one of many activities associated with completing a task. Thus, we have not conceptualized a measure of functional interdependencies for collective usage. However, because cognitive and structural interdependencies refer to more specific aspects of collective interdependence, we can use these to measure interdependence-in-use.

We measure cognitive interdependence via *collaboration-in-use* (Figure 4.6). Collaboration refers to cooperation on an intellectual endeavor (Merriam-Webster 2004). Collaboration emphasizes that *collective* cognition is social and distributed, not individual and isolated (Borthick et al. 2003; Halverson 2002; Lamb and Kling 2003; Thompson and Fine 1999). We adapt the general definition of collaboration to define *collaboration-in-use* as the extent to which members share knowledge with fellow members about how to use a system to perform a task. Thus, if one member does not know how to use a specific feature of an IS, s/he can access knowledge from another member (Gallivan et al. 2005; Olivera and Straus 2004; Perry 2003). It is desirable, in the case of our proposed model, to tie collaboration-in-use to "exploitive usage" (Burton-Jones and Straub 2004). Exploitive usage refers to the application of existing knowledge when using a system to perform a task. At an individual level, this knowledge resides in one person's mind, but at the collective level, it is distributed among the members' minds. Thus, in collectives, each member is not limited to applying his/her own knowledge; rather s/he can also apply other members' knowledge. Collaboration-in-use is the means by which members transfer such knowledge. As a result, collaboration-in-use increases knowledge available for exploitation. Thus, we suggest that collaboration-in-use is a necessary element of exploitive usage at the collective level.

We measure structural interdependence via *coordination-in-use* (Figure 4.6). *Coordination* refers to the management of dependencies among activities (Crowston 1997; Malone and Crowston 1994), such as the initial distribution and subsequent transfer of work among members and the assembly of final outputs. Coordination-*in-use* refers more specifically to aspects of coordination that involve system usage, e.g., the transfer of files among members at a time and in a format required by other members. Again, it is desirable, in the case of our proposed model, to tie coordination-in-use to exploitive usage. Whereas collaboration refers to one leg of the definition of exploitive usage (i.e., the "application of knowledge"), coordination refers to the other (i.e., "performing the task"). At an individual level, each person performs tasks singly, but in collectives, the "task" is distributed across members. Although distributing the task in this way can increases the collective's productivity (e.g., by reducing the amount of work required of each member), members must coordinate to produce this collective output (Kraut 2003). Thus, we suggest that coordination-in-use is a necessary element of exploitive usage at the collective level.

To summarize, the *scope* of exploitive system usage is limited to the application of a user's knowledge when using a system to perform a task (Burton-Jones and Straub 2004). We argue that when the user is an individual, this scope is bounded by his/her *individual* cognition and behavior, but when the user is a collective, this scope is bounded by *collective* cognition and behavior. Thus, collaboration-in-use and coordination-in-use are outside the scope (i.e., invalid measures) of individual usage, but clearly within the scope (i.e., valid measures) of collective usage.

4.2.4 Extending the Baseline Model: Cross Level Relationships

As noted earlier, a second reason that the proposed baseline theoretical model may be invalid in some contexts is that it does not account for contextual effects, i.e., cross-level relationships between the collective level and the individual level. Recent studies have examined the influence of collective usage on individual usage (Angst and Agarwal 2004; Gallivan et al. 2005). In this paper, we examine the relationship between collective usage and individual usage and between collective usage and individual performance (see Figure 4.7).

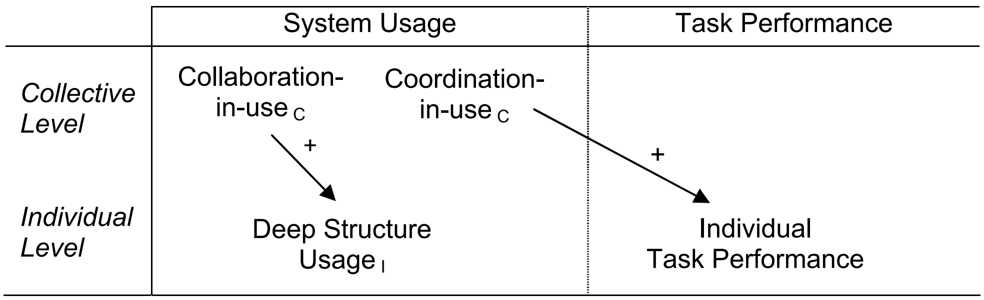


Figure 4.7: Cross-Level Relationships

The cross-level relationships that we posit are between the *interdependencies* that occur in-use at the collective level and individual usage and performance. The first relationship we propose is a positive effect of collective collaboration-in-use on individual deep structure usage. Recall that individual deep structure usage is the extent to which (a user) employs features in the system that support the task (Table 4.1). One reason that an individual may have low levels of deep structure usage is if s/he does not know how to use certain features of the system that support aspects of the task. If this is the case, the individual may have to use alternative features to work around his/her lack of knowledge, or else may simply not be able to perform that aspect of the task. If an individual is working on his/her *own*, one way to resolve this problem is to *explore* the system, to learn what features are available and how they could be used. However, when an individual is working in a *collective*, another way to resolve this situation is to obtain knowledge of how to use features from other members in the collective. Thus, we assume that knowledge of features of the system and knowledge of the task are distributed among members. Consequently, individual limitations in knowledge of the system and task can be overcome by collaboration-in-use. By reducing constraints on individual deep-structure-usage in this way, increased collaboration-in-use should be associated with increases in individual deep structure usage.

As Figure 4.7 shows, the second cross-level relationship proposed is between collective coordination-in-use and individual task performance. The positive link between coordination and *collective* performance is recognized in studies of group system usage. Kraut (2003) explains:

> "When coordination is high, a unit of individual work will translate into more team output. Conversely, when coordination is low, the same quality and quantity of individual work will result in less group output" (p. 339).

In addition to coordination-in-use improving *collective* performance, we propose that it can also have a direct positive effect on individual performance. This is for two reasons. First, coordination is necessary for the alignment of *goals* (Kraut, 2002). That is, coordination helps each individual understand what s/he needs to achieve to complete his/her portion of a collective task. This should have a positive effect on individual performance because it increases individual goal clarity. Goal clarity is a strong predictor of individual task performance (Locke and Latham 1990). Second, coordination is necessary for the alignments of *behaviors* (Kraut, 2002). In other words, coordination helps individuals synchronize their behaviors, ensuring that they prepare work in the time and format required the other members of their collective (Crowston 1997; Malone and Crowston 1994). When a member obtains his/her task inputs from other members, the inputs received from these other members serve as an anchor to his/her individual performance. Coordination-in-use serves to improve this anchor, thereby enabling each individual to have higher task performance.

4.2.5 An Extended, Multilevel Model of Usage and Performance

Drawing upon the baseline model, and the extensions outlined above, we propose a multilevel model of the relationship between system usage and task performance (see Figure 4.8). The model proposes single-level relationships between individual usage and individual performance, and collective usage and collective performance; it distinguishes between the structure of individual and collective usage and performance; and it proposes cross-level relationships between collective usage and individual usage and individual performance.

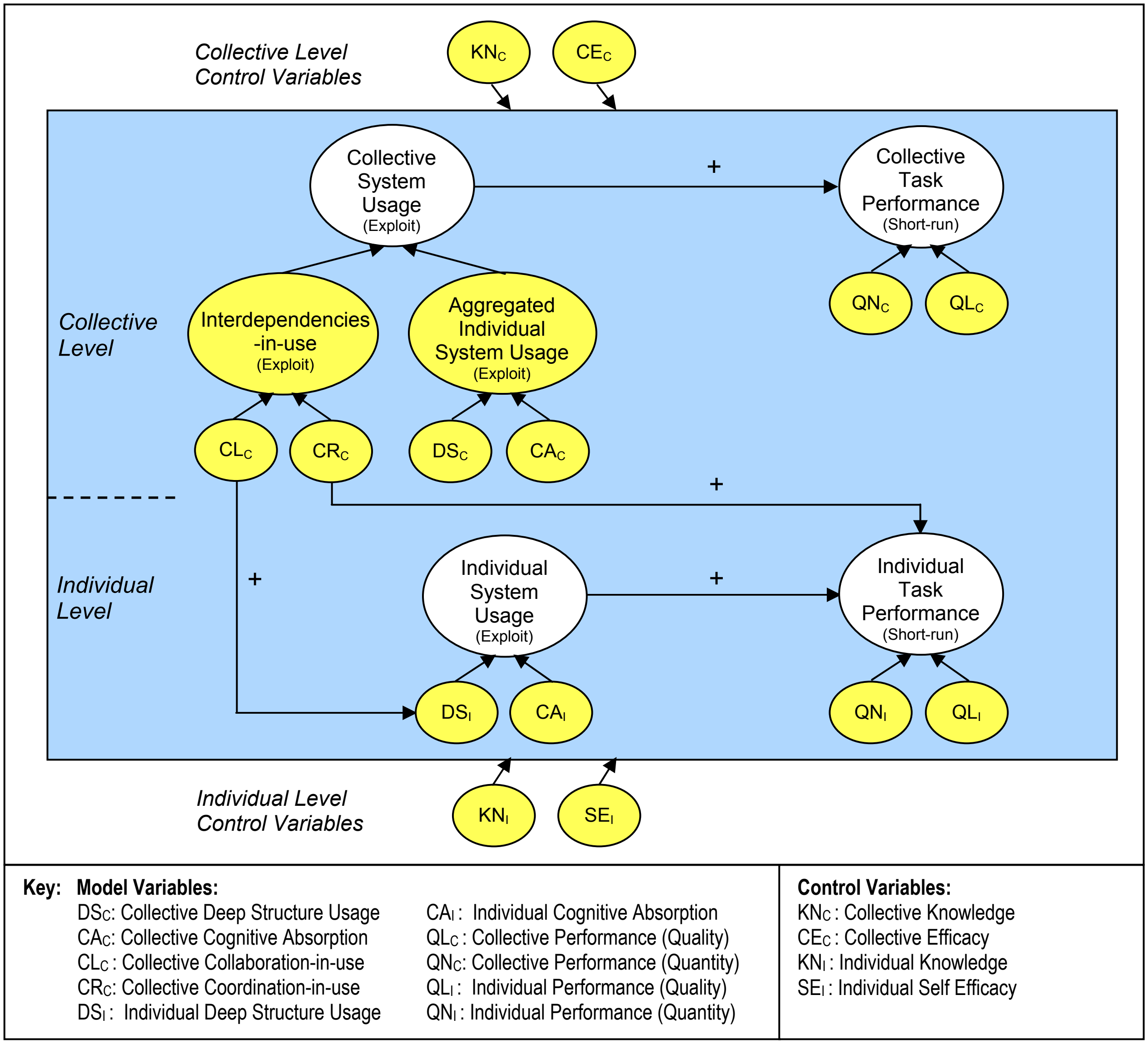


Figure 4.8: Theoretical Model

As Figure 4.8 shows, the theoretical model controls for two constructs: knowledge and efficacy. Knowledge is generally a key predictor of task performance (Sonnentag and Frese 2002). We therefore control for each individual's and collective's task-relevant knowledge. Efficacy refers to beliefs in one's ability to perform a task (Bandura 1997). It can positively affect performance at an individual and collective level (Chen et al. 2002). We control for these two constructs because like Marcolin et al. (2001), we believe that system usage will have a significant direct effect on task performance over-and-above the effects of knowledge and efficacy-beliefs.

Table 4.3 details our propositions and hypotheses that we use to test this multilevel model. The propositions map the issues described above, i.e., the baseline model (P1), the important of context when aggregating (P2), the structure of collective constructs (P3), and cross-level relationships (P4).

Table 4.3: Propositions and Hypotheses

Issue	Proposition	Hypothesis	Contribution
Baseline Model	Individual exploitive system usage has a positive relationship with short run individual task performance.	1. Individual exploitive system usage (formed by cognitive absorption and deep structure usage) has a positive relationship with short-run individual task performance (formed by quantity and quality of work).	Provides a replication of Burton-Jones and Straub (2004)
Context	The relationship between aggregated individual exploitive usage and short run task performance depends on the context in which the individuals exist.	2. When data are aggregated to the collective level, the relationship between exploitive system usage (formed by cognitive absorption and deep structure usage) and short-run task performance (formed by quantity and quality of work) differs when the individuals aggregated work independently (e.g., in "nominal" collectives) than in real collectives.	Provides evidence for whether context "matters" when aggregating data from the individual to the collective level.
Structure	Collective system usage comprises interdependencies-in-use in addition to the aggregation of members' individual system usage.	3. Interdependencies-in-use (formed by coordination-in-use and collaboration-in-use) has a positive significant effect on collective short run task performance (formed by collective quantity and quality of work) over and above the positive significant effect of aggregated individual usage (formed by aggregated cognitive absorption and deep structure usage).	Provides evidence for whether collective system usage is more than the sum of its parts.
Cross-level Effects	Elements of collective system usage influence individual short-run task performance and individual system usage.	4a. Collective coordination-in-use has a significant positive effect on short-run individual task performance (formed by quantity and quality of work). 4b. Collective collaboration-in-use has a significant positive effect on individual deep structure usage.	Provides evidence for whether the usage-performance relationship is multilevel in nature.

4.3 Research Design

We designed an experiment to test the theoretical model. The ability of experiments to rule out confounding explanations was critical to this choice, given that this is the first multilevel investigation of the relationship between usage and task performance (Calder et al. 1981). The task and technology, sample, procedure, and instrumentation are detailed below.

4.3.1 Task and System

The hypotheses are tested in the context of a spreadsheet-based business analysis case in a second-level accounting course in a large southern US university. The case required subjects to determine the lowest cost mortgage to fund an asset purchase (see Appendix 4A). We chose this task and IS for several reasons. First, it is the same context as studied in Burton-Jones and Straub (2004), allowing us to replicate their baseline model and reuse their instruments (e.g., by allowing us to use their scale for deep structure usage, which they tailored for this context). Second, spreadsheet-based business analysis is an important use of IS in practice (Panko 1998). Third, most studies on collective use have examined systems designed specifically for collaboration, e.g., GSS (DeSanctis and Poole 1994; Karsten 2003). However, in practice, individual productivity systems such as word processors and spreadsheets are often used individually and collectively (e.g., when a team of analysts produces a financial report), yet such instances of collective usage have received far less focus in IS research than studies of "pure" collaboration systems.

4.3.2 Sample

The total sample consisted of 873 individuals in 218 groups of 2 to 5 subjects. Of this sample, 633 individuals in 173 groups responded to the questionnaire, for a response rate of 72.5% (see Table 4.4). The average group size was 3.7. The sample comprised students from two consecutive semesters of the same course offered in 2003 and 2004. Due to practical constraints, only the 2004 data included measures of coordination- and collaboration-in-use. Other measures were obtained in both years. Thus, Table 4.5 lists the sample size for testing each hypothesis.

The course from which we obtained the sample instituted a series of 8 MS Excel-based business analysis cases in the accounting sequence, four in the first- and four in the second-level course. Data came from the final case in the second course. The case tested knowledge learned during the course, involved similar requirements to the prior 7 cases, and subjects could review the task for a week prior to the experiment. This allowed us to meet the assumption of exploitive usage, i.e., subjects had knowledge of the task and IS (Burton-Jones and Straub 2004).

The prior 7 cases had been conducted in self-organized groups, with groups created at the start of semester. Subjects performed the experiment in *existing* groups. These groups were randomly assigned to a "*nominal group*" or "*real group*" condition. We used a ratio of 4:1 for this assignment because only data from the real groups are used to test cross-level effects, and statistics for analyzing such effects (i.e., HLM) require large sample sizes. We studied existing groups (rather than new groups) because it allowed us to use past group performance as a control variable and it afforded a conservative test of H2, because in both conditions (real/nominal), each group had been a real collective in the past, but was either a nominal or real collective in *this* exercise.

Table 4.4: Sample of Respondents

	2003		2004		All	
	Total Groups	Average Size	Total Groups	Average Size	Total Groups	Average Size
Real groups	63	3.8	76	3.7	139	3.7
Nominal groups	16	3.9	18	2.9	34	3.4
All groups	79	3.8	94	3.5	173	3.7
*Data obtained from sample**	DS, CA, P		CR, CL, DS, CA, P		DS, CA, P	

* DS = Deep structure usage; CA = Cognitive absorption; P = Performance, CR = Coordination-in-use; CL = Collaboration-in-use.

Table 4.5: Sample for each Hypothesis Test

Hypothesis (per Table 4.3)	Sample size	Description
1.	633 individuals	All groups (2003-4), individual level
2.	139 groups, 34 groups	All groups (2003-4), collective level
3.	76 groups	Real groups (2004), collective level
4a.	76 groups, 278 individuals	Real groups (2004), cross-level
4b.	76 groups, 278 individuals	Real groups (2004), cross-level

4.3.3 Experimental Procedure and Treatments

Table 4.6 describes the experimental procedure for each treatment. The task required each group to prepare a spreadsheet comprising 7 worksheets. The experiment was conducted in PC laboratories supervised by independent proctors. Each group sat together, separated from other groups, with every student in each group having his/her own PC running MS Excel. Nominal groups and real groups performed the exercise in different labs. The experiment took 135 minutes, comprising 20 minutes for administration and 115 minutes for the experimental task.

Table 4.6: Experimental Procedure

	Instructions	Experimental Task (spreadsheet)							Questionnaire
		Sheet 1 Prepare input data	Sheet 2 Calculate bonds	Sheet 3 15-year schedule	Sheet 4 30-year schedule	Sheet 5 Findings & sensitivity	Sheet 6 Summary report	Sheet 7 Group Memo	
Nominal Groups	10 mins	115 minutes Work individually						10 mins Work in groups	10mins
Real Groups	10 mins	115 minutes Work in groups with sequential coordination. Allocation of sheets to members depended on group size (per below)*						10 mins Work in groups	10mins
Group size: 2:		M1	M1 →	M2	M2 →	M1 →	M2		
3:		M1	M1 →	M2	M2 →	M3	M3		
4:		M1 →	M2 →	M3	M3 →	M1 →	M4		
5:		M1 →	M2 →	M3 →	M4 →	M1 →	M5		

* M = member of group; 1..5 = position in group; Arrow = transfer of work.

As Table 4.6 shows, the allocation of tasks depended on the condition (nominal vs. real group) and group size. In nominal groups, each member performed the first six sheets alone and members could not help each other. In real groups, each member was randomly allocated 1 or 2 sheets, passed on his/her work to the next member, and members could help each other. In both conditions, groups worked together to perform the final (7th) sheet. We worked with an independent course instructor to distribute tasks in real groups in such a way that (a) work was approximately evenly split among members, and (b) each member's task had an approximately equivalent deep structure, allowing us to use the same deep-structure-usage scale for each member. We ran a small pre-test with four groups of three students to verify that the procedure worked.

To distinguish individual and group contributions, each group was given a set of floppy disks, one for each student, one for the group. Each student was required to save his/her own work on his/her individual disk, and the group was required to save the final group spreadsheet (with all 7 sheets) on the group disk. In the real group condition, members transferred their assigned sheets to each other, in sequence, via the group disk. This allowed each member's original work to remain intact on his/her individual disk, while enabling later members to modify prior members' work if necessary. This ensured that when asked about individual performance an individual could still refer to his/her own work, which remained intact on his/her individual disk.

In each condition (nominal and real groups), the manipulation was "do your best." Thus, it was up to each individual to determine his/her level of cognitive absorption and deep structure usage, and in the real group condition, it was up to each group to decide how much to coordinate and/or collaborate while using the system. Subjects were awarded a small number of bonus points based on their performance in the exercise. Real groups were told that their grade would comprise two parts: 50% based on individual performance and 50% based on group performance. Nominal groups were told that their individual performance determined their grade with a separate grade for the group's performance on the final sheet.

4.3.3 Instrumentation

Appendix 4B details instrumentation for each construct. Five constructs were measured at the *individual* level: two IVs (cognitive absorption, deep structure usage), the DV (performance), and two control variables (knowledge and efficacy). We adopted the scales for cognitive absorption (Agarwal and Karahanna 2000) and deep structure usage from those in Burton-Jones and Straub (2004). The scale for performance was created afresh. To reduce the size of the questionnaire, we used a three-item, formative measure, with one item for each dimension (quality and quantity) and one item for overall performance. Knowledge was measured via a single-item archival measure (performance in the final exam of the course). We used this as a proxy for individual *declarative* knowledge, i.e., knowledge of the material covered in the course. Finally, efficacy was measured with Compeau and Higgins' (1995) self-efficacy scale, which was adapted to refer to the nature of the task being performed (Johnson and Marakas 2000).

At the *collective* level, the theoretical model contains five constructs: four IVs (cognitive absorption, deep structure usage, coordination-in-use, and collaboration-in-use), the DV (performance), and two controls (knowledge and efficacy). Three ways to measure such constructs are: (1) a single, global measure for the entire group (e.g., an independent assessment of a group's performance), (2) obtaining responses from each respondent on items defined at the individual level (e.g., what was *your* performance?) and aggregating scores to the group level, and (3) obtaining responses from each respondent on items defined at the group level (e.g., what was your *group's* performance?) and aggregating scores to the group level (Chan 1998; Hofmann 2002). There is no firm rule regarding which approach to use (Kozlowski and Klein 2000).

We adopted the first two approaches to measure collective knowledge: using a single, global measure to serve as a proxy for each group's *procedural* knowledge (an archival measure of the group's performance in the previous Excel-based group exercise), and using an aggregation of each member's final exam score to serve as a proxy for the group's *declarative* knowledge.

We adopted the second approach to measure system usage because the four dimensions of collective usage originate 'below' the group level (in each member's cognitions and behavior and in the interactions among members) and we considered that each member would be best qualified to assess his/her own cognitions and behaviors and the interactions s/he was involved in. Moreover, defining items at their lowest level allows us to test whether responses are sufficiently homogenous to aggregate to the collective level (Bliese 2000). Thus, collective deep structure usage and cognitive absorption were measured via the same scales as at the individual level, and in fact, are just an aggregation of these values. New scales were created for coordination- and collaboration-in-use, however. These drew upon Hoegl and Gemuenden's (2001) scale for teamwork quality, which includes a sub-scale for coordination that we adapted to measure coordination-in-use, and sub-scales for communication and mutual support that we adapted to measure collaboration-in-use. The wording was modified in four ways: (1) to be non-evaluative, i.e., to focus on extent, not quality, of interaction (Burton-Jones and Straub, 2004), (2) to focus on interactions-*in-use* rather than in-general (3) to account for the *type* of collaboration and coordination (i.e., sharing knowledge of Excel, transferring files), and (4) to allow for the possibility that groups enact configurations of interaction (Hofmann 2002). In relation to this last point, any interaction involves production and receipt (giving and receiving). Thus, for coordination- and collaboration-in-use, we use two pairs of measures to capture each 'end' of the interaction, and in the data analysis, test whether there is sufficient homogeneity in responses to aggregate to the group level.

Finally, we used the third approach to measure collective efficacy and performance. For collective efficacy, we used the same scale as for the individual level, but specified the collective level in the item wording (Bandura 2000; Johnson and Marakas 2000). Likewise, we measured collective performance via the same scale as used for individual performance, but explicitly denoted the collective level in the items. A potential weakness of self-report performance measures is that individuals may not have the ability to accurately assess their performance. This risk was low in this case because participants had received assessment on seven prior cases.

To improve the reliability and validity and of the measures, we used a Q-sort exercise (Moore and Benbasat 1991). The items were randomized and given to 26 senior students who were asked to separate the items into bundles for each construct and name each one. Their feedback supported the validity of the scales (with items placed in the correct bundle, on average, 77% of the time); minor changes were made to the scales based on their feedback.

4.4 Data Analysis

Data analysis proceeded in two steps: data screening and hypothesis testing.

4.4.1 Data Screening

Tables 4.7A-B show the descriptives, aggregation statistics, reliabilities, and correlations for constructs in the theoretical model. At the *individual* level (Table 4.7A), the correlations indicate some support for H1 as both cognitive absorption (CA) and deep structure usage (DS) are significantly related to performance ($r = .30$ CA, $r = .41$ DS). The control variables are less influential. Knowledge is not significantly related to any variable, while self-efficacy is related to each variable *except* task performance ($r = .00$). As neither control variable is related to individual task performance, we exclude them from further tests at the individual level.

Table 4.7B details results for the *collective* level. In multilevel research, it is important to verify that there is sufficient homogeneity within groups and heterogeneity between groups to

Table 4.7: Descriptives, Aggregation Statistics, and Correlations[+]

A. Individual Level (sample: all groups)

	Descriptives		Correlations				
Variable	Mean	Std Dev.	1	2	3	4	5
1. Cognitive absorption$_I$	5.8	1.7	(.87)				
2. Deep structure usage$_I$	5.9	1.7	.60**	(.88)			
3. Individual performance$_I$	5.8	2.1	.30**	.41**	--		
4. Knowledge$_I$	74.7	10.8	.01	.05	.01	--	
5. Efficacy$_I$	6.1	2.2	.27**	.17**	.00	.17**	(.91)

B: Collective Level (sample: real groups)

	Descriptives		Aggregation		Correlations						
Variable	Mean	Std. Dev.	ICC(1)	ADI	1	2	3	4	5	6	7
1. Cognitive absorption$_C$	5.8	1.2	.77	1.02	(.90)						
2. Deep structure usage$_C$	5.9	1.2	.79	0.95	.65**	(.92)					
3. Collaboration-in-use$_C$	5.1	1.0	.90	0.94	-.02	.03	(.84)				
4. Coordination-in-use$_C$	5.5	1.2	.84	1.05	.26*	.17	.58**	(.95)			
5. Collective performance$_C$	5.4	1.7	.49	0.87	.46**	.56**	.30**	.40**	--		
6. Knowledge$_C$	83.1	6.0	.48	NA	.05	.10	-.13	-.22	.20*	--	
7. Efficacy$_C$	6.9	1.5	.65	1.24	.16	.19	-.04	.03	-.13	.05	(.88)

[+] Key: (): The principal diagonal shows scale reliability statistics (Cronbach's alpha) for the reflective measures.
* : Significant at $p < .05$, ** $p < .01$ (two-tailed);
ICC(1): Intra-class correlation. Indicates between-groups variance, with a scale of 0 to 1.
ADI: Average deviation index. Indicates within-group homogeneity. It should be less than c/6, where c is the number of items for each scale (i.e., 1.8 or 11/6 for efficacy, and 1.5 or 9/6 for the other scales). ADI is not applicable for "Knowledge" because each group only had one score for procedural knowledge.

warrant aggregating data to the collective level (Bliese 2000; Hofmann 2002; Klein et al. 1994). Table 4.7B lists standard aggregation statistics (ICC[1] and ADI) for testing this assumption (Bliese 2000; Burke and Dunlap 2002). ICC(1) reflects between-groups variance. The data suggest that half or more of the variance of each construct lies between-groups, well above minimum levels, e.g., 0.10 (Gavin and Hofmann 2002; Hofmann and Jones 2005). ADI is a measure of within-group homogeneity (Burke and Dunlap 2002). Table 4.7B shows that mean ADI values are within accepted guidelines, suggesting that group members agreed (i.e., held a shared view) of the scores for each construct at the collective level. Together, these results suggest that it is reasonable to aggregate the constructs to the collective level (Bliese 2000).

Table 4.7B shows correlations among the collective constructs. The correlations indicate some support for the theoretical model, as each component of collective usage is significantly related to collective performance (r = .46 CA, r = .56 DS, r = .30 CL, r = .40 CR). The correlations show mixed support for the control variables. We expected that both measures of knowledge (declarative and procedural) would relate to performance. Table 4.7B shows that the average of declarative and procedural knowledge is significantly related to performance (r = .20). Yet, on inspection, we found this was driven by procedural knowledge (r = .23), with declarative knowledge insignificant (r = .09). Likewise, collective efficacy was unrelated to any variable (Table 4.7B). Based on these results, we retain procedural knowledge as a control variable at the collective level, but we exclude efficacy and declarative knowledge from further tests.

After screening the correlations and aggregation statistics, we examined instrument reliability and validity. Tables 4.7A-B include evidence for the reliability of each instrument. Reliabilities were all above minimum guidelines, with Cronbach's alpha ranging from .84 to .95. Tables 4.8-4.9 include evidence for construct validity. The reflective measures of individual usage (deep structure usage and cognitive absorption) converged and discriminated in the expected ways (Table 4.8A). So too did the additional usage measures obtained at the collective level (collaboration-in-use and coordination-in-use) (Table 4.8B).[37] Table 4.9 shows evidence for the validity of the formative measures of individual and collective performance. There is no firm rule to determine the validity of formative measures (Chin 1998). To verify the validity of these items, we used PLS-Graph to test the two single-level models that comprised the theoretical model (i.e., one at the individual level and one at the collective level), and examined the weight of each formative indicator of performance on the latent construct. As Table 4.9 shows, each indicator was strongly related to the latent construct, suggesting that each is an important measure of performance (Chin 1998). Overall, the results for the reflective and formative measures suggest that the data have adequate reliability and validity to test the hypotheses.

Table 4.8: Convergent and Discriminant Validity for Reflective Measures*

A. Individual level usage measures		
	CA	DS
CA13	.85	.22
CA16	.80	.28
CA19	.79	.29
CA12	.76	.28
DS15	.24	.87
DS14	.25	.85
DS18	.30	.83
DS17	.30	.71
Principal components analysis, Varimax with Kaiser normalization, Converged in 3 iterations.		

B. Collective level usage measures		
	CL	CR
CL24	.87	.31
CL20	.86	-.01
CL25	.70	.51
CL21	.60	.42
CR27	.17	.93
CR26	.24	.89
CR22	.22	.89
CR23	.30	.88
Principal components analysis, Varimax with Kaiser normalization, Converged in 3 iterations.		

* CA: Cognitive absorption; DS: Deep structure usage; CL: Collaboration-in-use, CR: Coordination-in-use; Item numbers reflect the order of the item in the questionnaire (see Appendix 4B).

Table 4.9: Weights of Formative Measures

	Weight
IP7	.80
IP8	.96
IP9	.87

	Weight
CP2	.92
CP3	.97
CP4	.84

4.4.2 Hypothesis Tests

Table 4.10 summarizes our hypothesis tests. We use two statistical techniques:

[37] As an additional test, we used PLS-Graph to examine composite reliabilities, item loadings and cross-loadings, and correlations and the average variance extracted (AVE) for each construct. These results (not shown to conserve space) supported those shown above, providing further assurance that the instrumentation was reliable and valid.

hierarchical linear modeling (HLM) and OLS regression. HLM is a widely accepted multilevel modeling technique used in education (Raudenbush and Bryk 2002), health (Luke 2004), management (Hofmann 1997), and IS research (Ang et al. 2002). HLM requires researchers to specify the DV at the lowest level (Castro 2002). As the proposed theoretical model contains a DV at each level, we use HLM to test the baseline model (H1) and cross-level models (H4), and use OLS regression to test the single-level models at the collective level (H2-3) (see Table 4.10). We use OLS regression in preference to structural equation models (e.g., PLS or LISREL) because HLM is itself a regression- rather than a SEM-based technique.

We tested the assumptions of HLM and OLS regression before conducting the hypothesis tests. Other than the assumption regarding independence of errors at the individual level (which HLM accounts for), all assumptions were met (Table 4.11).

Table 4.10: Summary of Hypothesis Tests

Hypothesis (per Table 4.3)	Sample size (per Table 4.5)	Variables*	Level of analysis	Statistical technique
1.	633 individuals	IV: Individual system usage DV: Individual task performance	Individual	HLM
2	139 groups, 34 groups	Same as H1, but aggregated to the collective level.	Collective	OLS Regression
3.	76 groups	IV: Aggregated individual system usage, Collective interactions-in-use DV: Collective task performance	Collective	OLS Regression
4a.	76 groups, 278 individuals	IV: Coordination-in-use DV: Individual task performance	Cross-level	HLM
4b.	76 groups, 278 individuals	IV: Collaboration-in-use DV: Individual deep structure usage	Cross-level	HLM

* For simplicity, we do not list the control variables in this table, although we test for them in the data analysis.

Table 4.11: Testing Statistical Assumptions*

Technique	Assumption	Test Description	Violation?
Regression	Linearity of X -Y relationships	Examined box plots and outliers	No
	Normal distribution of errors	Examined normal probability plots	No
	Constant variance	Examined homogeneity of variance statistic	No
	Independence of errors	NA (violated because individuals in groups)	NA
	Linear independence of predictors	Examined colinearity diagnostics (VIFs)	No
HLM*	Multivariate normality	Examined normal probability plot	No

* Because HLM is a regression-based technique, it shares the assumptions of regression (except the assumption of independence of errors). Thus, this table only lists HLM assumptions that are *in addition* to those of OLS regression.

Table 4.12 details results for H1. After accounting for the positive effects of working in a group (γ_{02}), and being in a group with high procedural knowledge (γ_{01}), individual system usage (formed by deep structure usage and cognitive absorption) explains a large amount of variance in individual performance (R^2 = .31). While variance remains in the intercept (i.e., τ_{00} is significant), indicating that the DV could be further explained, no variance remains in the slope (i.e., τ_{11} is insignificant), indicating that the effect of individual usage on performance is not moderated by other collective level constructs. Overall, the results support H1 and the baseline model.[38]

Table 4.12: Results for Hypothesis 1 (Individual Level of Analysis)

	Parameter Estimates							R^2	
Hypothesis and Model	γ_{00}	γ_{01}	γ_{02}	γ_{10}	σ^2	τ_{00}	τ_{11}	IU	Model
H1: $L1: IP_{ij} = \beta_{0j} + \beta_{1j}(IU_{ij}) + r_{ij}$ $L2: \beta_{0j} = \gamma_{00} + \gamma_{01}(Kn_j) + \gamma_{02}(Tr_j) + U_{0j}$ $L2: \beta_{1j} = \gamma_{10} + U_{1j}$	5.78*	0.04*	1.19*	0.46*	2.36	0.86*	0.06	0.31	0.43

Key: Model

L1 = Level 1; L2 = Level 2; IP = Individual performance.; IU = Individual usage; Kn = Collective procedural knowledge; Tr = Treatment (working in nominal group = 1, working in real group = 2)

Parameters

γ_{00} = Intercept of Level 2 regression predicting β_{0j}

γ_{01} - γ_{02} = Slopes of Level 2 regression predicting β_{0j}

γ_{10} = Intercept of Level 2 regression predicting β_{1j} (pooled Level 1 slopes)

σ^2 = Variance in Level 1 residual (i.e., variance in r_{ij})

τ_{00} = Variance in Level 2 residual for models predicting β_{0j} (i.e., variance in U_0)

τ_{11} = Variance in Level 2 residual for models predicting β_{1j} (i.e., variance in U_1)

R^2

IU = Incremental R^2 due to Usage, i.e., $(\tau_{00[conditional-level 2]} - \tau_{00[fully-conditional]}) / \tau_{00[conditional-level 2]}$

Model = R^2 of model, i.e., $(\tau_{00[unconditional]} - \tau_{00[fully-conditional]}) / \tau_{00[unconditional]}$

Note: Each predictor is entered as grand-mean centered (per Luke, 2004). * p < .05

Table 4.13 details results for H2-3. H2 predicted that the usage-performance relationship would differ when aggregated from real collectives than from nominal collectives. As Table 4.13

[38] We also tested whether two aspects of our research design (group size and year) affected these results. The results were robust; these variables had no significant direct (p = .93, p = .64) or moderating (p. = .23, p = .93) effects.

Table 4.13: Results for Hypotheses 2-3 (Collective Level of Analysis)

Hypothesis and Model	Groups	Parameter Estimates				Model Statistics			
		b_0	b_1	b_2	b_3	F	Adj. R^2	$\triangle R^2$	f^2
H2: $IP_i = \beta_0 + \beta_1(IU_i) + \beta_2(Kn_i)r_i$	Nominal	4.43*	0.06	--‡	--	0.6	-.03	--	--
	Real	-2.06	0.76*	0.04*	--	35.4*	.33	--	--
H3: $CP_i = \beta_0 + \beta_1(IU_i) + \beta_2(Kn_i) + r_i$	Real	1.26	0.72*	--‡	--	23.2*	.23	--	--
$CP_i = \beta_0 + \beta_1(IU_i) + \beta_2(Kn_i) + \beta_3(CI_i) + r_i$	Real	-1.04	0.65*	--‡	0.52*	19.7*	.34	.11*	0.16

Key: Model:
IP = Aggregated individual performance, IU = Aggregated individual usage, Kn = Collective procedural knowledge; CP = Collective performance, CI = Collective interdependencies-in-use
Parameters:
b_0 = sample estimate of β_0, i.e., intercept for the DV (individual performance, collective performance)
b1-b2 = sample estimates of β_1 - β_2, i.e., coefficients for the IVs (ind. usage, collect. interdependencies-in-use)
Model Statistics:
$\triangle R^2$ = The significance of the change in R^2 is tested by multiplying f2 by (n-k-1), where n is the sample size (76) and k is the number of IVs, allowing a pseudo F-test with 1, n-k degrees of freedom (Mathieson et al. 2001).
f^2 = $(R^2_{full} - R^2_{partial})/(1 - R^2_{full})$. An f^2 of .02 is small, .15 is medium, .35 is large (Chin et al. 2003; Cohen 1988)

* p < .05, ‡ 'Kn' was found to be insignificant in these tests and therefore excluded to simplify interpretation of the data.

shows, the aggregated usage-performance relationship is insignificant in the nominal condition, but positive and significant in the real condition (Adj R^2 = .33). To ensure this difference was not due to sample size differences (n = 34 nominal, n = 139 real), we selected 30 random subsamples of n = 34 from the real sample, re-ran the model in each subsample, calculated the average beta and standard error for usage across the 30 subsamples (*b* = .75, *SE* = .20), and compared them to those from the nominal sample (*b* = .06, *SE* = .26). The estimates were significantly different (t = 12.3, p < .01).[39] To determine whether this difference reflected different relationships at the *individual* level, we re-ran the individual-level model (in Table 4.12), adding a parameter to test whether *treatment* (real/nominal) moderated the usage-performance relationship. The moderation was not significant (p = .43). Finally, we re-ran the individual-level model separately for the real and nominal conditions, finding that usage had a significant positive effect on performance in each condition (p ≤ .01) (results not shown to conserve space). In summary, the results suggest:

[39] The formula for the two independent samples t-test is: $(b_1 - b_2)/\sqrt{(s_1^2/n_1) + (s_2^2/n_2)}$.

- at the individual level, the usage-performance relationship is positive and significant
- the individual-level relationship is not significantly affected by group type (real/nominal)
- when aggregated to the collective level, the usage-performance relationship is positive and significant when data are from *real* groups, but not when data are from *nominal* groups.

These results strongly support H2, demonstrating that context matters when aggregating data. For example, if a researcher aggregated the data for *all* the groups in our sample, without verifying the nature of the collectives, the explained variance for the usage-performance relationship would be Adj. $R^2 = .23$, which differs significantly from the true results in both types of groups (Adj. $R^2 = -.03$, Nominal, Adj. $R^2 = .33$, Real, per Table 4.13).

H3 predicted that the effect of collective usage on collective performance would be greater when accounting for interdependencies-in-use. As Table 4.13 shows, including interdependencies-in-use significantly increases the explained variance ($\triangle R^2 = .11$), having a "medium" effect on the DV (i.e., $f^2 = 0.16$) (Cohen 1988). Thus, H3 is supported.

Table 4.14 details results for H4. H4a predicted that collective coordination-in-use would have a significant positive effect on individual performance. As Table 4.14 shows, after controlling for individual usage and collective knowledge, coordination-in-use had a significant positive effect on individual performance (i.e., γ_{01} is significant), increasing R^2 by 0.15. In HLM, the significance of this increase can be tested by comparing each model's deviance statistic (produced using full maximum likelihood estimation), with the difference distributed as a chi-square statistic with degrees of freedom equal to the difference in the number of parameters (Luke 2004). Following this procedure, the difference in deviance between the models was 4.64 (1035.21 – 1030.57), which, with one additional parameter (i.e., γ_{01}), was significant at $p < .05$. As in H1, we tested whether these results were influenced by the size of the groups, but group size had no direct ($p = .92$) or moderating effect ($p = .55$). Thus, H4a is supported.

H4b predicted a significant positive effect of collective collaboration-in-use on individual

Table 4.14: Results for Hypothesis 4 (Cross Levels of Analysis)

Hypothesis and Model	Parameter Estimates							R^2	
	γ_{00}	γ_{01}	γ_{02}	γ_{10}	σ^2	τ_{00}	τ_{11}	CI	Model
H4a: $L1: IP_{ij} = \beta_{0j} + \beta_{1j}(IU_{ij}) + r_{ij}$; $L2: \beta_{0j} = \gamma_{00} + \gamma_{01}(CR_j) + \gamma_{02}(Kn_j) + U_{0j}$; $L2: \beta_{1j} = \gamma_{10} + U_{1j}$	6.24*	0.10*	.02	.07*	2.06	0.31*	0.04	0.15	0.42
H4b: $L1: DS_{ij} = \beta_{0j} + r_{ij}$; $L2: \beta_{0j} = \gamma_{00} + \gamma_{01}(CL_j) + U_{0j}$; $L2: \beta_{1j} = \gamma_{10} + U_{1j}$	6.13*	0.13	--	--	1.91	0.56*	--	0.00	0.00

Key:

Model

L1 = Level 1; L2 = Level 2; CP = Collective performance; IU = Aggregated individual usage; CI = Collective interdependencies-in-use (CR in H4a, CL in H4b); Kn = Collective procedural knowledge.

Parameters

γ_{00} = Intercept of Level 2 regression predicting β_{0j}

γ_{01} - γ_{02} = Slopes of Level 2 regression predicting β_{0j}

γ_{10} = Intercept of Level 2 regression predicting β_{1j} (pooled Level 1 slopes)

σ^2 = Variance in Level 1 residual (i.e., variance in r_{ij})

τ_{00} = Variance in Level 2 residual for models predicting β_{0j} (i.e., variance in U_0)

τ_{11} = Variance in Level 2 residual for models predicting β_{1j} (i.e., variance in U_1)

R^2

CI = Incremental R^2 for CI, i.e., $(\tau_{00[fully-conditional-without-CI]} - \tau_{00[fully-conditional-with-CI]}) / \tau_{00[fully-conditional-without-CI]}$

Model = R^2 of model, i.e., $(\tau_{00[unconditional]} - \tau_{00[fully-conditional]}) / \tau_{00[unconditional]}$

* p < .05

deep structure usage. Table 4.14 shows that even though there is a significant variation in deep structure usage among groups (i.e., τ_{00} is significant), collective collaboration-in-use accounts for none of this variance (R^2 CI = 0.00). Again, we investigated whether the effect of collaboration-in-use on individual deep structure usage might depend on group size, but group size had no significant effect (p = .47). Thus, there is no support for H4b.

Table 4.15 summarizes the results of the hypothesis tests. H1-H4a were supported, while H4b was not supported. The implications of these results are considered next.

Table 4.15: Summary of Results

Hypothesis	Supported?
1	Yes
2	Yes
3	Yes
4a	Yes
4b	No

4.5 Discussion

This study contributes by providing a first step towards understanding the multilevel relationship between system usage and task performance. It advances an understanding of three elements of this multilevel relationship: *context*, *structure*, and *cross-level relationships*.

In relation to *context*, one way to measure collective usage in past studies has been to aggregate individual-level data to the collective level (Devaraj and Kohli 2003; Easley et al. 2003). Although it may seem self-evident that this should be done only if individuals are using the system *as a collective* (e.g., a workgroup, organization, etc.), researchers may be tempted to assume rather than verify that this is the case, especially if they believe it would not make much empirical difference if the sample consisted of a mix of users, some using ISs collectively, some independently. Our results demonstrate why it is important for researchers to verify whether users are using systems collectively before aggregating data, because data aggregated from collectives and mere "collections of individuals" do not have the same relationship with downstream constructs. In other words, context "matters" not only theoretically, but also empirically. Thus, in future studies in which researchers wish to aggregate individual-level data to the collective level, we suggest that they must verify the "state" (Dubin 1978; Weber 2003c) of the context in which single-level phenomena exist before aggregating data to the collective level .

In relation to *structure*, the nature of single-level "constructs" is familiar to researchers, but the nature of "collective constructs" is not (Morgeson and Hofmann 1999). A key argument in multilevel theory is that higher-level constructs can serve the same function as their lower-level counterparts, but with different structures (Morgeson and Hofmann 1999). To our knowledge, the present study is the first in IS to explicitly model this difference in structure in a construct across levels of analysis. Although some recent studies have examined how collective usage relates to individual usage (Angst and Agarwal 2004; Gallivan et al. 2005), these studies have conceived of collective usage as a simple aggregation of individual usage.

Collective usage certainly includes the sum of the parts, and the results of our empirical study demonstrate that this alone has a substantial impact on collective performance (R^2 = .23, Table 4.13). Nevertheless, collective usage is *more* than the sum of individual usage, because it also comprises interdependencies that occur during collective usage, and these have a significant incremental effect on collective performance (f^2 = .16, Table 4.13). The challenge lies in identifying *what* comprises the additional structure in any collective construct, i.e., the whole minus its parts. Morgeson and Hofmann (1999) provide initial guidance, suggesting that the difference lies in the "interactions" among parts. Following Burton-Jones and Straub (2004), we suggest that researchers choose elements of these interactions that are appropriate for the theory and research question. For example, in the present study, we took a different approach for measuring a group *output* (e.g., performance) than a group *process* (e.g., system usage). Likewise, to measure collective exploitive usage, we drew on theories of groups (Lindenberg 1997) to submit that two types of interdependencies were relevant: collaboration-in-use and coordination-in-use. Although we believe that our measures were appropriate for our theory and research question, other measures may have been more appropriate if we were studying a different question (e.g., the relationship between system usage and long-run, rather than short-run, task performance).

In relation to *cross-level relationships*, the present study is the first (to our knowledge) to examine cross-level relationships between collective usage and individual performance. As Goodman (2000) demurs, little is known of the links between levels of analysis in organizational research. Our results show that aspects of collective usage can significantly affect individual performance. Thus, studies of individual usage that ignore potential cross-level effects from the collective level could obtain spurious results simply due to an unmeasured effect from the collective level. In this way, the internal validity of many IS studies could be threatened.

In addition to these three contributions, the empirical study contributes by replicating recent work on individual usage (Burton-Jones and Straub 2004), providing additional evidence for the validity of the measurement model and structural model developed in that earlier study, and extending that study by validating a measurement model of *collective* usage. Thus, the study contributes towards a cumulative tradition in investigating the usage-performance relationship.

Despite this empirical study's contributions, it is not without limitation. From the perspective of internal validity, the theoretical model is very limited in scope, only comprising a small set of constructs. We controlled for efficacy and knowledge, but additional variables (e.g., motivation) may have been important to control. Furthermore, there may be relevant mediating variables (e.g., individual or collective learning) that are not accounted for in the theoretical model. Finally, our conception of cross-level relationships was only partial. We theorized "downward" cross level effects, for which data analysis techniques are reasonably mature. We did not theorize "upward" cross-level effects, as data analysis techniques for these are still in their infancy (Griffin 1997; Kozlowski and Klein 2000). These limitations are all worthy of research.

The lack of support for H4b was notable. We had expected that collaboration-in-use would be positively associated with individual deep structure usage, but our results showed no evidence of any effect. We speculate on two possible reasons. First, it might be a measurement issue. The scale for collaboration-in-use referred to advice given and received regarding "how to use Excel", whereas the scale for deep structure usage referred to the use of Excel features to perform specific tasks, such as "features that helped me perform calculations on my data." The difference in specificity of these two scales may be the cause of the problem. Future research could investigate this issue by: (a) creating more specific measures of collaboration-in-use, which would accord with recent calls for more feature-specific measures (Jasperson et al. 2005), or (b) creating more generalized measures of deep structure usage, which would accord with past calls to test different levels of specificity when measuring feature use (Griffith and Northcraft 1994).

A competing reason for the lack of a result for H4b is that the construct is misspecified. For example, it may be that *non-evaluative* measures of collaboration-in-use do not have a clear relationship with individual usage. In this light, Burton-Jones and Straub (2004) suggested that system usage is a non-evaluative construct (i.e., it differs from an evaluation of usage such as "effective" usage). Although our results suggest that a non-evaluative measure of collaboration-in-use is associated with higher collective performance (see Table 4.7b), it may be that an *evaluative* measure (e.g., collaboration 'quality') is needed to find an effect on individual usage, e.g., good collaboration may improve but poor collaboration may hinder individual deep structure usage. Future research is needed to investigate these issues.

With respect to measurement, research is needed to examine whether results would differ if we used measures of collective usage that explicitly referred to the collective level (e.g., how did your *group* collaborate-in-use?). There is no firm rule at present regarding which approach is optimal (Kozlowski and Klein 2000); more research is needed to address this issue.

In terms of statistical conclusion validity, our sample size was relatively small for HLM (Hofmann et al. 2000), so a larger sample may have offered more powerful tests. Nevertheless, recent studies have used HLM with much smaller samples (e.g., 25 teams of 4 people) (Hofmann et al. 2003), and our study did find significant results despite the small sample size.

Finally, in terms of external validity, our results stem from just one experiment. Although the experiment used a widely used computer application, and a realistic business case, it is still limited by the standard limitations of laboratory experiments with student samples (Greenberg 1987); replications using other samples would be valuable. Furthermore, the study was limited to the individual and small-group levels. Although we believe that our measures of interdependencies-in-use (collaboration-in-use and coordination-in-use) are sufficiently general to be applicable to any two levels (e.g., individual-group, group-organization, organization-network), this remains an open theoretical and empirical issue.

This research could be extended in several ways. Perhaps the most obvious way would be to extend our study by examining different contexts, structures, or cross-level relationships, or to improve our study by directly addressing the limitations noted above. In addition to these, we see four additional important areas.

First, researchers could extend our work by accounting for *antecedents* of individual and collective usage. Researchers have begun to investigate common antecedents to usage across levels (Dennis et al. 2004). Nevertheless, there are likely to be critical differences in antecedents across levels because collective usage is more than the sum of its parts. Investigating these antecedents would be very valuable because DeLone and McLean's (1992) IS success model specified consequences at the individual and collective levels, but only specified system usage and its antecedents at the *individual* level. Thus, by identifying the antecedents to individual *and* collective usage, researchers could begin to construct a full, multilevel IS success model.

Second, our theoretical model did not consider the possibility that collective level constructs could reflect distinct *configurations* in values across members of a collective. We allowed for this possibility in our measures, but the data was still restricted in that it met the necessary assumptions of a *shared* view (as opposed to a *configurational* view) of the constructs (Hofmann 2002). Recent research has suggested that many collectives enact distinct configurations when performing tasks (e.g., for specialization of labor and loose coupling)

(Hofmann 2002; Kozlowski and Klein 2000; Rousseau 2000). To our knowledge, no empirical studies in IS have examined this issue. Doing so could be a particularly valuable way to improve understanding of the relationship between usage and performance.

Third, the present study largely ignored the *temporal* dimension. The issue of time in multilevel research is clearly critical (Rousseau 2000). Future research could study whether certain effects from collectives on individuals occur more quickly than others, or whether certain individual level effects emerge at the collective level at different rates. The same data analysis procedures (e.g., HLM) that are used for multilevel data analysis are also able to analyze temporal effects (e.g., through studies of "growth curves" with HLM) (Raudenbush and Bryk 2002). Consequently, such research on temporal issues is much more feasible now than it was in the past.

A final important avenue of research would be to examine the degree to which multilevel studies of usage and performance (or other relationships in IS research) can contribute vis-à-vis *social network analyses*. These two approaches (multilevel research, social network research) conceptualize the collection of lower-level units differently, as an entity (i.e., a "collective") or as a set of nodes and links (i.e., a "network"). It is likely that these are complementary. For example, multilevel research (but not network research) can isolate the effect of the "collective" on its members, but social network research (but not multilevel research) can isolate the relative importance of specific nodes and links within the collective that lead to effects (Borgatti and Cross 2003; Sparrowe et al. 2001). Investigating the complementarity between multilevel and social network analyses of the usage-performance relationships promises very rich insights into the pathways by which system usage leads to performance for individuals and collectives.

4.6 Conclusion

System usage is a key construct in IS research, a necessary link between IS investments and individual and organizational outcomes from IS (Heine et al. 2003; Soh and Markus 1995). Although some studies have investigated the link between system usage and its consequences at a single level (whether at an individual, group, and organization level), none to our knowledge have explicitly examined it from a multilevel perspective. This study provides a first step towards understanding the multilevel nature of the usage-performance relationship. We examined three elements of this multilevel relationship: the importance of verifying the *context* in which users exist (i.e., collectives of individuals or collections of independent individuals), the *structure* of collective- vis-à-vis individual-level usage and performance, and the presence of *cross-level relationships* from collective usage to individual usage and performance.

To examine the importance of these elements, we adopted a baseline model of the usage-performance relationship developed in recent research (Burton-Jones and Straub 2004), extended this model to account for its multilevel nature, and then tested the extended model in an experiment involving 34 nominal groups (comprising 116 individuals) and 139 groups (comprising 517 individuals) performing a spreadsheet-based financial analysis task.

The results of the experiment support the importance of context, structure, and cross-level relationships when studying the usage-performance relationship. The study is not without weaknesses, but despite its limitations, this first multilevel investigation of the usage-performance relationship revealed important new finding, suggests profitable directions for future research, and demonstrates the feasibility of conducting multilevel studies of the usage-performance relationship (and, by extension, other such relationships in the field). Researchers have long called for a multilevel approach for studying the consequences of system usage in organizations (Harris 1994). We hope that our theoretical model and results offer a potential, if partial, baseline to support such research.

Appendix 4A: Experimental Task

Summary of Business Dilemma (additional accounting data not shown to conserve space)

- Early in 2003, Mimi sold her home
- She immediately bought bonds with the proceeds and now receives bond interest payments semi-annually
- Mimi aims to buy a beach house in ten years. She wants to buy a townhouse now, to be sold in ten years when she buys the beach house
- Mimi wants to know whether she should sell the bonds or some of them or take out a new mortgage for the full town home price
- She wants to have the most cash on hand from selling the property to buy the beach house at the end of 10 yrs.

Requirements

Develop a spreadsheet model to support your advice to Mimi about financing the beach house. Include the following 7 sheets in the spreadsheet:

1. *Input sheet*:

Containing the bond portfolio and assumptions for a loan

2. *Bonds calculation sheet*:

Containing their face value, present value for interest payments, present value of lump sum maturity value, estimated price, annual cashflow from interest, estimated market value of bond portfolio, and average interest rate of cashflows.

Note, if you finance the home all by bonds, then you will save mortgage payments. If you finance the home by mortgage, you can invest the interest payments received from the bonds. Therefore, your bonds worksheet should include the potential value of saving and investing mortgage payments each month over 10 years, and the potential value of saving and investing bond interest payments received over 10 years.

3. *15 year loan sheet*:

Containing a 15 year loan amortization schedule, and calculations for the maximum loan amount and the maximum home price.

4. *30 year loan sheet*:

Containing a 30 year loan amortization schedule, and calculations for the maximum loan amount and the maximum home price

5. *Report sheet*:

This report should detail the potential down payment for the beach-house in 10 years under each option (15-year loan, 30-year loan, or bonds).

To do this report, you will need to know the equity build-up, market value of bond portfolio (if applicable), and potential accumulations from investing the savings under each option. You could prepare two analyses: one would show calculations if Mimi's assumption about real-estate appreciation were met; one would show calculations if Mimi's assumptions were not met (i.e., real-estate values do not rise). The report should be easy for Mimi to understand.

6. *Findings sheet*:

This should list:

(1) key <u>findings</u> that occur to you during the case (e.g., the effect of selling bonds on the type of house she can buy now and in ten years) and
(2) the <u>sensitivity</u> of these findings to key parameters (including mortgage interest rates, real estate appreciation, and spending/saving assumptions).

7. *Memo sheet*:

The memo should recommend whether Mimi should sell the bonds or get a mortgage and how much equity she can expect to have as a down payment on the beach house in ten years. Indicate assumptions and their implications. The memo should be concise; in <u>bullet-point form</u> rather than paragraph form.

Appendix 4B: Instrumentation*

Deep Structure Usage (Burton-Jones and Straub 2004)
14. When I was using MS Excel, I used features that helped me test different assumptions in the data
15. When I was using MS Excel, I used features that helped me compare and contrast aspects of the data
17. When I was using MS Excel, I used features that helped me perform calculations on my data
18. When I was using MS Excel, I used features that helped me derive insightful conclusions from the data

Cognitive Absorption (Agarwal and Karahanna 2000)
12. When I was using MS Excel, I felt completely absorbed in what I was doing
13. When I was using MS Excel, I was able to block out all other distractions
16. When I was using MS Excel, my attention did not get diverted very easily
19. When I was using MS Excel, I felt totally immersed in what I was doing

Collaboration-in-use (new scale)
20. To what extent did you give advice about how to use Excel to your group members during the simulation?
24. To what extent did you share your knowledge of how to use Excel with your group members during the simulation?
21. To what extent did you receive advice about how to use Excel from your group members during the simulation?
25. To what extent did your group members share their knowledge of how to use Excel with you during the simulation?

Coordination-in-use (new scale)
22. To what extent did you discuss with your group members about when to transfer work during the simulation?
26. To what extent did you coordinate with your group members about when to transfer work during the simulation?
23. To what extent did your group members discuss with you about when to transfer work during the simulation?
27. To what extent did your group members coordinate with you about when to transfer work during the simulation?

Collective Performance (new scale)
2. I think the instructor would probably give our group the following percentage score on the case we just completed:**
3. The instructor will probably consider our solution to be of very high quality:
4. I believe that our group completed all required parts of the case:

Individual Performance (new scale)
7. I think the instructor would probably give me the following percentage score on my section of the case:**
8. The instructor will probably consider my individual work to be of very high quality:
9. I believe that I completed all required parts of my tasks:

Knowledge (Burton-Jones and Straub 2004)
Declarative knowledge: Final exam score (individual-level measure)
Procedural knowledge: The group's score on the previous Excel exercise (collective-level measure)

Collective Efficacy*** (adapted from (Compeau and Higgins 1995)
Our <u>group</u> could complete [name of exercise] using MS Excel …
….if there was no one around to tell us what to do as we [I] did it.
….if we had only the solution to the previous case for reference
….if we had seen someone else doing it before trying it ourselves
….if we could call someone for help if we got stuck
….if someone else had helped us get started
….if we had a lot of time to complete the case
….if we had just the built-in Excel help facilities for assistance
….if someone showed us how to do it first.

Self Efficacy*** (adapted from (Compeau and Higgins 1995)
<u>I</u> could complete [name of exercise] using MS Excel…
….if there was no one around to tell me what to do as I did it.
….if I had only the solution to the previous case for reference
….if I had seen someone else doing it before trying it myself
….if I could call someone for help if we got stuck
….if someone else had helped me get started
….if I had a lot of time to complete the case
….if I had just the built-in Excel help facilities for assistance
….if someone showed me how to do it first.

* 9-point likert scale; ** percentage scale; *** 11 point likert scale; Item numbers reflect the order in the questionnaire.

Chapter 5

A Comprehensive Approach for Dealing with Method Variance in IS Research[40]

Abstract

Researchers have long known that choice of research method influences the viability of construct measurements. Campbell and Fiske (1959) proposed that researchers should account for such method variance by measuring each construct in a model via multiple methods and using a special correlation matrix (a multi-trait, multi-method matrix), together with the logic of triangulation, to estimate the true relationships among the constructs. Many statistical tests have evolved to implement Campbell and Fiske's approach in ever more advanced ways, yet the underlying logic stands to this day.

This paper responds to Fiske and Campbell's (1992) own call to improve their approach. Specifically, their approach provides no guidance for selecting methods, and overemphasizes the importance of convergence in triangulation. To address these limitations, we propose the first comprehensive approach for dealing with method variance. This approach consists of two strategies for dealing with method variance, criteria for selecting an appropriate strategy for a given study, and guidance for executing a selected strategy.

We conduct a demonstration exercise to show how the approach would work in an empirical study of the relationship between individual-level system usage and task performance. The results support the approach, demonstrate its feasibility, and illustrate how it can help researchers test theories and identify new research opportunities.

Keywords: research methods, method variance, bias, construct development, theoretical conceptualization, measurement, validation, system usage.

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[40] Burton-Jones, A., and Straub, D. "Toward a Comprehensive Approach for Dealing with Method Variance in IS Research," Working paper, Department of Computer Information Systems, Georgia State University, 2005.

5.1 Introduction

To bridge the intellectual and empirical realms of research (e.g., to test a theory), researchers use "research methods." Because science relies so heavily on its methods, an enduring quest is to improve the way researchers use methods in practice. This paper aims to contribute to this goal by providing researchers with a deeper understanding of one key problem related to "positivist" research methods—method variance—and providing a comprehensive approach that positivist researchers can use to deal with method variance in practice.[41]

Campbell and Fiske (1959) introduced method variance to the behavioral sciences in the following way:

> Each test or task employed for measurement purposes is a trait-method unit, a union of a particular trait content with measurement procedures not specific to that content. The systematic variance among test scores can be due to responses to the measurement features [method variance] as well as responses to the trait content [trait variance] (p. 81).

Campbell and Fiske (1959) argued that method variance was a serious concern and that researchers need a way to unpack the relative effects of trait (i.e., "true") variance and method variance. They suggested that researchers pursue the following strategy: (1) use multiple methods to measure each construct in their model, (2) enter the correlations among all of the measures in a special correlation matrix (the multi-trait, multi-method [MTMM] matrix), and (3) use triangulation to converge on the true (i.e., unbiased) relationships among the constructs.

Although Campbell and Fiske's (1959) paper is one of the most cited in social science, the adoption of their recommendations has been notoriously low. Fiske and Campbell (1992) explained that this was due to two problems. First, the MTMM matrix requires researchers to use judgment when interpreting data, that is, it does not offer elegant statistics to quantify trait and

[41] Since the demise of logical positivism, all working positivists are, arguably, "post-positivists" (Suppe 1977). Nevertheless, terms are hard to change and we continue using "positivist" in its broad, informal sense, in which it refers to a large class of research concerned with theory testing, measurement, and validity. We leave an analysis of the potential for method variance in non-positivist research (e.g., interpretive research) to future studies.

method variance. Second, at the conceptual level, method variance turned out to be more complex than originally envisioned. Although many methodologists have attempted to resolve the first problem, by creating advanced statistical techniques for analyzing MTMM data, the second problem remains. Fiske and Campbell (1992) demurred that methods and traits might be so conceptually intertwined that separating them might be impossible, a problem they found disturbing yet seemingly intractable. Despite calls for research on these conceptual difficulties, the issues remain little understood and unresolved (Cronbach 1995; Fiske 1995; Fiske and Campbell 1992). The unfortunate result is that there is no accepted approach for dealing with method variance in behavioral research (Sechrest et al. 2000). Although recent studies offer strategies for dealing with parts of method variance, such as common method bias (Podsakoff et al. 2003), what is missing is a strategy for dealing with method variance as a whole. This problem, and the lack of awareness of it in IS research (Woszczynski and Whitman 2004), has led to a state in which:

- Most IS researchers do not account for method variance in their studies
- The minority of IS researchers who account for method variance do so only partially
- Consumers of IS research are not aware of the impact of method variance on results.

To redress this situation, the present study clarifies the meaning of method variance and advances, for the first time, a comprehensive approach for dealing with it. The approach makes an important contribution by enabling researchers to deal with method variance in a way that (1) is complete, (2) fits a study's aims, theoretical context, and practical constraints, and (3) supports cumulative theoretical progress. The present study also contributes by showing how the approach would work in an important research context, namely, the relationship between individual-level system usage and task performance. The results demonstrate that the approach is feasible, illustrate its benefits for testing theories, and reveal how it can help generate important insights for future research.

5.2 Context: The Current State of Research on Method Variance

Table 5.1 summarizes extant research on method variance. Three categories can be identified: research on the nature of method variance; research on methodological approaches to reduce method variance; and research on statistical approaches to estimate method variance.

Table 5.1: Summary of Extant Research on Method Variance

Research Focus	Question	Representative Articles	Conclusions/Findings to Date
Nature of method variance	(1) What is method variance?	(Campbell and Fiske 1959; Crano 1981; Cronbach 1995; Fiske 1982; Golding 1977; Sechrest et al. 2000)	Method variance is not well defined. Doubts have surfaced about whether method variance and trait variance can be distinguished.
	(2) Is method variance substantial?	(Cote and Buckley 1987; Crampton and Wagner 1994; Glick et al. 1986; Meyer et al. 2001; Podsakoff et al. 2003; Sharma and Yetton 2004; Wilson and Lipsey 2001)	In a given study, method variance can range from high to low. On average across studies, method variance appears to be as large as, or larger than, trait variance.
	(3) How have past studies accounted for method variance?	(Fiske and Campbell 1992; Podsakoff et al. 2003; Woszczynski and Whitman 2004)	Many empirical studies do not account for method variance. Very few studies in IS discuss method variance.
Methodological approaches to reduce method variance	(4) What research methods tend to increase method variance?	(Glick et al. 1986; Harrison et al. 1996; Hoyt 2000; Hufnagel and Conca 1994; Podsakoff and Organ 1986; Tepper and Tepper 1993).	Method variance is often found to affect data obtained by self-report methods and common methods. The extent of bias caused by a method depends on the context in which the method is used.
	(5) What alternative methods can be used to minimize method variance?	(Donaldson et al. 2000; Fazio and Olson 2003; Hilbert and Redmiles 2000; Payne et al. 2003; van Bruggen et al. 2002; Webb et al. 2000)	Many alternatives methods to self-report have been developed, e.g., archival measures, primary or secondary observation, behavioral traces, and implicit measures.
Statistical approaches to identify or reduce method variance	(6) What statistical approaches can be used to estimate method variance?	(Bagozzi et al. 1991; Campbell and O'Connell 1982; Corten et al. 2002; Eid et al. 2003; Kenny 1995; Lindell and Whitney 2001; Millsap 1995; Reichardt and Coleman 1995; Saris and Aalberts 2003; Westen and Rosenthal 2003).	Many technique have been created to estimate method variance, e.g.: MTMM matrices; ANOVA; correlations; and structural equation models (including single factor models; correlated uniqueness models; direct product models).

5.2.1 The Nature of Method Variance

A surprising feature of research on method variance is that few-to-no studies have examined underlying conceptual issues such as defining method variance, describing its nature, and distinguishing it from trait variance (Fiske, 1995, Fiske and Campbell 1992). According to

Cronbach (1995, p.145), "nearly everyone's attention went to [statistical] techniques for analysis." As Sechrest et al. (2000, p. 84) state: "Method effects are omnipresent, but we do not as yet know very well how to define them, let alone detect them and estimate their magnitude."

If method variance were not a problem, there would be no need to resolve these conceptual issues. Unfortunately, method variance causes two critical problems. First, method variance can artificially increase reliability, leading researchers to (unknowingly) place faith in biased measures. Specifically, many researchers justify the reliability of measures using tests such as Cronbach's alpha (Boudreau et al. 2001). Reliability theory assumes that test scores are a function of true score and random error (Cote and Buckley 1987). As method variance is not random, reliability theory includes method variance in true variance. As a result, method variance tends to artificially inflate reliability scores such as Cronbach's alpha (Tepper and Tepper 1993). A common example of this occurs when researchers list questionnaire items in groups for each construct rather than randomize the items. As Goodhue and Loiacono (2002) note, respondents often give similar scores to grouped questions (e.g., to avoid cognitive dissonance), thus leading to high Cronbach's alpha (high reliability) but biased scores (low validity). Thus, researchers who use this practice could, unknowingly, be relying on biased data (Straub et al. 2004b).

A second problem is that method variance biases the estimates of relationships among constructs. An early meta-analysis of MTMM studies found that on average across studies, measures contain 42% true variance, 26% method variance, and 32% random error variance (Cote and Buckley 1987). Using these percentages, Podsakoff et al. (2003) showed that where two constructs have a true correlation of 1.00, method variance can lead researchers to conclude that the correlation is 0.52, and where two constructs have a true correlation of 0.00, method variance can lead researchers to conclude that the correlation is 0.23. In a recent synthesis of meta-analyses (reflecting 16,525 individual studies), Wilson and Lipsey (2001) found that method variance was an even stronger influence than in Cote and Buckley (1987). They found that method variance accounted for nearly as much variability as true variance. Thus, method variance can be a substantial cause of both type 1 and type 2 errors.

In short, method variance may not be well understood, but it can cause considerable problems and raise serious questions about the validity of research findings. Unfortunately, few researchers attempt to address it (Fiske and Campbell, 1992). In IS, Woszczynski and Whitman (2004) analyzed 116 recent studies in *MIS Quarterly*, *ISR*, and *JMIS*, and found that only 12 studies even mentioned method variance, while 58% of studies used only one method. Dishearteningly, Scandura and Williams (2000) found that management researchers had reduced their emphasis on using multiple methods over the course of the last two decades.

5.2.2 Methodological Approaches to Reduce Method Variance

Despite the lack of research on the nature of method variance, it is true that researchers have attempted to identify what methods tend to produce method variance (per Table 5.1). Many studies have shown that the variance caused by a method depends on the context of the measurement, e.g., the instrument used, the construct being measured, and the data source (Crampton and Wagner 1994; Glick et al. 1986; Harrison et al. 1996; Kline et al. 2000; Williams and Brown 1994). Nevertheless, some methods are considered to be especially problematic. For example, many highlight the problems of self-report measures. According to Spector (1992; 1994), self-reported measures often reflect only 10-20% of the true variance of a construct. To overcome known flaws of self-report measures, researchers have proposed: (a) improved methods for constructing self-report measures (Hufnagel and Conca 1994; Schaeffer and Presser 2003; Tourangeau et al. 2000); (b) alternatives to self-report measures such as “implicit” measures and behavioral traces (Fazio and Olson 2003; Hilbert and Redmiles 2000; Webb et al. 2000). Another typical cause of method variance is the use of common methods. The use of common methods often leads to common method bias, i.e., covariance between items that stems from the items being measured by the same method. Recent studies have highlighted the problems of common method bias and have summarized strategies that can be used for preventing, detecting, or correcting it (Podsakoff et al. 2003; Woszczynski and Whitman 2004).

5.2.3 Statistical Approaches to Estimate Method Variance

The largest body of literature on method variance addresses statistical issues (Cronbach, 1995). Many statistical techniques have been tested (per Table 5.1). These fall into two categories: (1) techniques that estimate common method variance; (2) techniques that estimate method variance in general. The vast majority estimate common method variance (e.g., MTMM matrices, ANOVA, and structural equation models). Although these differ in particulars (e.g., the degree to which the tests require researchers to name the aspect of the method that is common across items or constructs), they all estimate common method bias rather than other aspects of method variance (Saris and Aalberts 2003, Podsakoff et al. 2003). Recently, more general techniques have emerged. For example, Westen and Rosenthal (2003) propose a technique for quantifying the construct validity of a set of scores by testing the degree to which the actual correlations match the theoretically expected correlations. This test assumes, however, that researchers know the method variance in their data *prior* to data analysis. Without this knowledge, the results of the test are inconclusive because method variance can increase or decrease correlations leading to a positive or negative result. While this is not a criticism of the test per se, it highlights the point that statistics alone cannot solve method variance (Cronbach, 1995).

5.2.4 The Need for a Comprehensive Approach for Dealing with Method Variance

Overall, our analysis of past research on method variance highlights three critical, unresolved problems:

- The nature of method variance is not well understood
- Most empirical researchers do not account for method variance
- The minority of researchers who do attempt to account for method variance (whether statistically or methodologically) generally only deal with part of the problem (e.g., common method bias or self-report bias).

To overcome these problems, this paper aims to clarify the meaning of method variance and then to present a comprehensive approach for dealing with it in IS research.

5.3 Clarifying the Meaning of Method Variance

Method variance is complex for at least two reasons. First, its meaning depends on the meta-theoretical assumptions underlying positivist research, which are complex and often poorly understood (Messick 1989). Second, it can manifest itself in many ways, so it can be a difficult concept to pin down (Fiske and Campbell, 1992). To address these issues, we first briefly clarify the meta-theoretical assumptions of positivist research. We focus, especially, on critical realism, the view arguably held (at least implicitly) by most positivist researchers (Cook and Campbell 1979; Messick 1989; Moldoveanu and Baum 2002; Weber 2003b).[42] Second, we decompose method variance into its sources. We then examine the meaning and significance of each source.

5.3.1 The Meta-Theoretical Assumptions Underlying Positivist Research

Table 5.2 illustrates the evolution of meta-theoretical assumptions in positivist research. The initial meta-theoretical position was *operationalism*. Operationalists (e.g., logical

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[42] Like all ontological viewpoints, critical realism continues to evolve (Archer et al. 1998; Mingers 2004b; Mir and Watson 2001). Our focus is on the core assumptions of critical realism that concern issues of measurement. These have remained largely invariant since their classic treatment in Cook and Campbell (1979) [see Messick 1989].

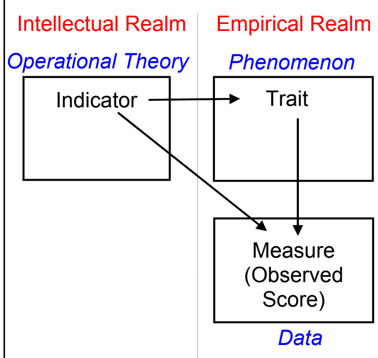
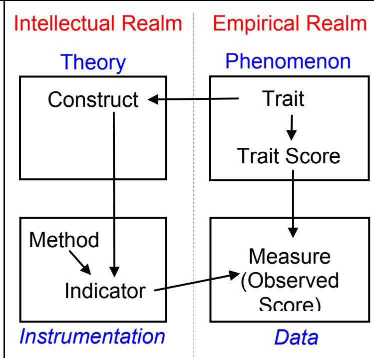
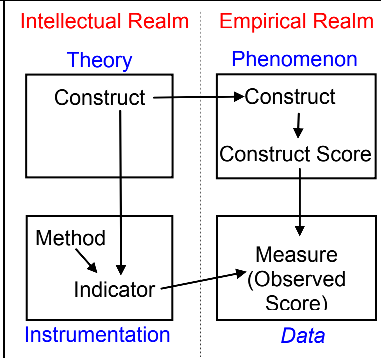
positivists) rejected unobservable intellectual notions such as research constructs. To operate without constructs, operationalists simply assumed that operations defined real world traits and measures defined trait scores (e.g., your IQ *is* your intelligence) (Trout 1999). Although initially influential, operationalism, along with logical positivism, has long been rejected by most scientists (Grace 2001).

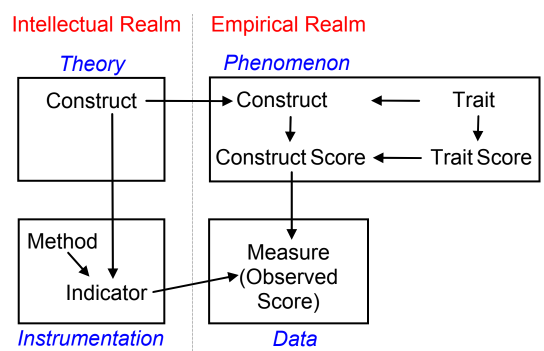
Realism was an early alternative to operationalism. Realists embraced unobservable notions (e.g., constructs) and assumed that the empirical realm (e.g., traits and trait scores) could be mirrored in the intellectual realm. Thus, realists assumed that real world phenomena (e.g., humans) have inherent traits (e.g., anxiety) with an inherent scale (e.g., nominal, ordinal, interval, etc.) and that members of a population vary in their "true" trait scores on this scale (Essex and Smythe 1999). The aim of research, according to the realist, was to build constructs that *correspond* to real world traits (hence the arrow leading from traits to constructs in Table 5.2) and to use methods to obtain the true scores of these traits (Borsboom et al. 2004; Trout 1999).

The *constructivists* rejected the realists' assumption that the "true" nature of the world could be observed (Kuhn 1996). To operate without this assumption, constructivists assumed that: (a) constructs do not correspond to reality; they are merely useful fictions that researchers *attribute to* phenomena; (b) scales do not reflect the *true* nature of a trait; they merely enable researchers to obtain interesting/useful insights about phenomena; and (c) scores are not "true," but simply fictions that allow researchers to discuss the relative standing of phenomena on a socially constructed construct and scale (Messick, 1989). Unfortunately, while constructivism overcame some limitations of realism, it left the position of measurement and progress rather vacuous in that nothing really exists to measure or to progress towards (Borsboom et al. 2004).

Critical realism melded constructivism and realism in an attempt to overcome each one's limitations (Cook and Campbell, 1979). Critical realists assume that real world phenomena have

Table 5.2: Progression of Meta-Theoretical Assumptions in Positivist Research*

<table>
<tr><th></th><th colspan="3">Meta-Theoretical Assumptions In Past Positivist Research</th></tr>
<tr><th></th><th>Operationalism</th><th>Realism</th><th>Constructivism</th></tr>
<tr><td>Worldview:</td><td>
</td><td>
</td><td>
</td></tr>
<tr><td>Goal:</td><td>Reductionism (i.e., explanation without metaphysical notions)</td><td>Truth (i.e., explaining/predicting reality)</td><td>Theoretical progress (i.e., useful, accepted theories)</td></tr>
<tr><td>Trait score:</td><td>…is the observed score.</td><td>… exists, but is unknown.</td><td>…does not exist.</td></tr>
<tr><td>Methods:</td><td>…are part of constructs.</td><td>…can be used to obtain the trait score.</td><td>…are socially accepted ways of collecting data.</td></tr>
<tr><td>Valid measures:</td><td>…are reliable and useful.</td><td>…reflect the trait score.</td><td>…fit a pattern expected by the theory.</td></tr>
</table>

<table>
<tr><th></th><th>Meta-Theoretical Assumptions in Extant Positivist Research
Critical Realism (Constructive-Realism)</th><th>Legend
(Illustrative Example)</th></tr>
<tr><td>Worldview:</td><td>
</td><td rowspan="5">Trait**: the nature of human intelligence in reality.

Trait score***: a person’s true level of intelligence in reality.

Indicator: an operation designed to measure intelligence (e.g., IQ).

Construct: researchers’ notion of human intelligence.

Construct score: a person’s level of what researchers consider to be human intelligence.

Method: approach for collecting data about human intelligence.

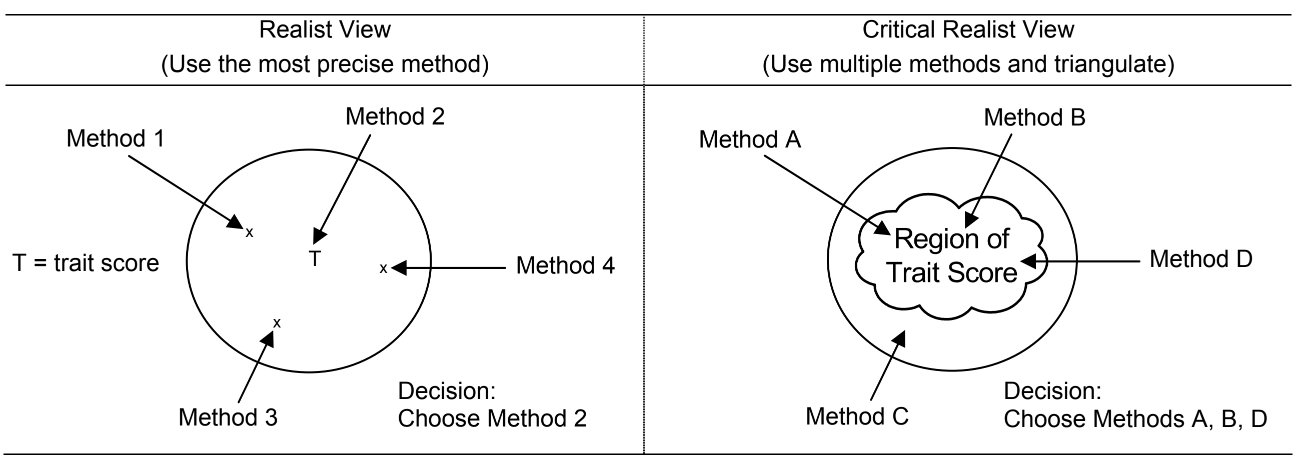
Measure: an observed score of intelligence on an indicator.</td></tr>
<tr><td>Goal:</td><td>Theoretical progress, attempting to attain, yet never attaining, truth.</td></tr>
<tr><td>Trait score:</td><td>…exists, but is unknowable.</td></tr>
<tr><td>Methods:</td><td>…are socially accepted ways of approximating the trait score.</td></tr>
<tr><td>Valid measures:</td><td>…approximate the trait score and fit a pattern expected by the theory.</td></tr>
</table>

* Adapted from (Borsboom et al. 2004; Messick 1989; Trout 1999).
** For simplicity, we use “trait” to refer to both traits and states (per Messick, 1989).
*** The trait score is a “true” score, but it differs from the statistical notion of “true score.” The latter refers to tests and samples rather than a phenomenon’s true score on a particular property (Borsboom et al. 2004).

traits with scores, but they assume that these are *unknowable*; they can only be approximated by constructs and measures (Loevinger 1957; Messick 1989). This influences the way critical realists view data analysis. Specifically, critical realists assume that a measure is valid if it measures the trait score (Borsboom et al. 2004), but because the trait score is unknowable, they assume that data analysis cannot *prove* the validity of a measure. Rather, data analysis can only show whether the data fit an expected theoretical pattern (Westen and Rosenthall, 2003). Thus, critical realists emphasize obtaining precise measures prior to data analysis (Tourangeau et al. 2000, Schaeffer and Presser, 2003).

Even so, critical realists do not place as much faith in methods as realists did. Because realists had assumed that trait scores could be obtained, they tended to believe that there was a single "best" method for measuring any trait (Webb et al. 2000, p. xvi). In contrast, critical realists assume that the trait score is unknowable and that multiple methods may be needed in an attempt to approximate it (Webb et al. 2000). This led to the importance of "triangulation" in critical realist research (Campbell and Fiske 1959; Crano 1981; Crano 2000) (see Figure 5.1). Researchers triangulate by using multiple independent methods to converge on the true trait score (Sechrest et al. 2000, p. 65). In the best case, all methods selected for studying a trait should have merit, yet have different (and ideally, independent) strengths and limitations (Webb et al. 2000). The underlying assumption is that "truth lies at the intersection of independent lies" (Levins 1966, p. 423).

Figure 5.1: Choice of Methods in Realist versus Critical Realist Research



Campbell and Fiske (1959) proposed that triangulation was the best way for critical realists to deal with the problem of method variance. They suggested that researchers could have some faith that their measures approximate true trait scores if (a) they measured each construct in a study using multiple "appropriate" methods and (b) if their measures of each construct converged and their measures of different constructs discriminated. Campbell and Fiske (1959) acknowledged that this approach was fallible, e.g., it contains little guidance for determining "appropriate" methods. Nevertheless, while many statistical techniques have evolved to implement the approach in ever more sophisticated ways, its underlying conceptual logic remains dominant to this day (Crano, 2000).

Following Campbell and Fiske (1959), this paper examines method variance on the basis of critical realist assumptions. However, like Fiske (1995), we suggest that Campbell and Fiske's logic of triangulation is useful but insufficient for dealing with method variance. The following sections set out to define the necessary principles to enable us to overcome its limitations.

5.3.2 Decomposing "Methods" and "Method Variance"

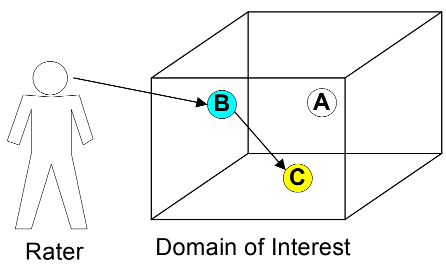
Method and method variance are not well defined in the literature. In its broadest sense, method refers to "everything that is done to collect the data" (Viswanathan 2005, p. 148). Three elements are generally agreed to exist (Cronbach 1995, Sechrest et al. 2000, Viswnathan 2005):

- **Rater**: the provider of the score (e.g., subject, researcher, independent rater, computer log).
- **Instrument**: the device used for scoring (e.g., questions and response options in a questionnaire or interview, coding scripts for recording observations, recording programs for collecting electronic/computer records).
- **Procedure**: the process by which the rater's score is obtained, whether for a single item of one construct (e.g., instructions for raters), or for multiple items (e.g., the sequence and timing of scoring multiple items of one construct or multiple items of different constructs).

The "score" referred to above is an observed score obtained via a method in an attempt to approximate the trait score. Thus, we draw on these elements to define method variance as:

the systematic difference between an observed score and the trait score that stems from the rater, instrument, and/or procedure used to obtain the score.

Because critical realists assume that the trait score is unknowable, method variance is also. Researchers' only recourse is to identify *sources* of method variance and minimize their impact. This cannot be done conclusively at present because there is no comprehensive typology of sources of method variance (Cronbach, 1995, Sechrest et al. 2000). Recent studies have made progress by collating examples of biases due to raters, instruments, and procedures (Podsakoff et al. 2003; Viswanathan 2005). We extend these studies here by distinguishing between two distinct sources of method variance: *distance bias* and *representation bias* (see Figure 5.2, Table 5.3).[43]



A = Trait score
B = Rater's best estimate of the trait score
C = Rater's recorded rating of the trait score

Method variance: The distance C→A
Distance bias: The distance B→A
Representation bias: The distance B→C

Figure 5.2: Two Sources of Method Variance: Distance Bias and Representation Bias

Table 5.3: Decomposing Method Variance by Components of Methods and Sources of Bias

	Source of Bias	
Component of Method	**Distance Bias**	**Representation Bias**
Rater	Bias due to a rater's lack of access to the trait score (e.g., self-rating versus observers' ratings of self-efficacy).	Bias due to a rater's unwillingness to provide his/her best estimate of the trait score (e.g., social desirability, privacy concerns, mood).
Instrument used by rater	Does not result in distance bias.	Bias due to an instrument influencing a rater to give a different score (e.g., biased or ambiguous wording of items or response scales, too few/many response categories).
Procedure followed by rater	Does not result in distance bias.	Bias due to a procedure influencing a rater to give a different score (e.g., the use of a common method to rate several items can induce hypothesis guessing and order effects).

[43] We adapt the term "distance" from measurement theory where it has been used to refer to a difference in *meaning*: the distance between the meaning of an indicator and a construct (Little et al. 1999). We use it to refer to a difference in *scores*: the distance between the rater's best estimate of the trait score and the true trait score. Likewise, we adapt the term "representation" from measurement theory where it has been used to refer to a difference in *scales*: whether a researcher's scale faithfully represents a trait's "true" scale (Schwager 1991). We use it to refer to a difference in *scores*: whether a rater's score faithfully represents the rater's best estimate of the trait score.

Distance bias refers to a difference between the rater's (true) estimate of the trait score and the true trait score due to the rater's lack of access (or "distance") to the trait. Distance bias stems from the *rater* rather than the rater's use of an instrument or procedure (per Table 5.3). As the trait score is unknowable, so too is distance bias. Nevertheless, it can be theorized. A common example of distance bias occurs when a person rates an internal trait of another person. In such instances, the rater must use proxies (e.g., observable behaviors) to estimate the score. Distance bias can also occur when a person rates him/herself. For example, people can find it difficult to recall past behavior (Blair and Burton 1987) and can lack awareness of mental states (Bem 1972) and, thus, need to rely on proxies to estimate the scores. In all of these instances, lack of knowledge results in systematic differences between estimated scores and trait scores.

Representation bias refers to a difference between a rater's true estimate of the trait score and the reported trait score that stems from the rater not providing (i.e., "misrepresenting") his/her best estimate of the trait score. As representation bias does not depend on the actual trait score, it is knowable. As Table 5.3 shows, it can be caused intentionally (i.e., stem directly from the *rater*) or unintentionally (i.e., stem from the rater being influenced by the *instrument* or *procedure*). Intentional biases are common, and tend to occur when the rater has a general tendency or a specific motivation to give a biased response (e.g., to maintain privacy). Unintentional biases are also common and have been the focus of much research. For example, the tendency of some instruments to induce raters to provide biased scores has been the basis of past criticisms of self-report questionnaires (Spector, 1994, Webb et al. 2000). Likewise, the tendency of some procedures to induce raters to provide biased scores has been the focus in the common methods literature (Podsakoff et al. 2003). In all of these cases, the result of representation bias is that the rater gives a score that differs systematically from their best estimate of the trait score.

Our distinction between distance and representation bias helps to overcome a limitation of Campbell and Fiske's (1959) approach by providing researchers with a basis for choosing sets of methods. Past studies tend to associate bias with *methods*. For example, an oft-cited view is that "no research method is without bias" (Webb et al. 2000, p. 2). However, methods alone have no inherent biases, they are merely associated with different *sources* of bias. To address method variance, therefore, we argue that researchers need to focus on these sources, rather than methods per se, and they need to do this systematically, ruling out both distance bias and representation bias.

For example, in studies of systems usage, Straub et al. (1995) found that usage had different relationships with antecedent constructs depending on the method used to measure it. Since then, several studies have used different methods to measure antecedents to use (questionnaires) and use (computer logs) (Venkatesh et al. 2003). While the intent here is laudable, it only tackles one aspect of representation bias (common methods bias). If not used carefully, it could exacerbate distance bias (e.g., if computer logs are less able than self-reported measures to rate certain aspects of individual's use of systems). Logically, it is clear that a systematic approach is needed to address both sources of bias.

5.3.3 The Meaning and Significance of Distance Bias and Representation Bias

Another reason for distinguishing between distance bias and representation bias is that they have different relationships with the reality of the domain under investigation. Specifically, a rating that reflects distance bias is not a true rating of the intended trait, but it is, nevertheless, a true rating. Moreover, this rating may be a valid rating of a relevant, albeit unintended trait, in the domain. For example, assume that a researcher asks a manager to complete a questionnaire regarding her subordinate's performance. If the manager has a mistaken perception of her subordinate's performance, her rating would be a biased rating of actual performance, but it would be a valid rating of her perception of the employee's performance, and this perception could have a real effect on how s/he interacts with the employee at work, i.e., it is a relevant trait in the domain, but just a different trait to the employee's true performance.

In contrast, a rating that reflects representation bias is not a true rating of any trait in the domain; it is a transitory bias that exists only due to the act of measurement. For example, a manager who responds to a questionnaire regarding employee performance may give a biased score because she attempts to provide a socially desirable answer. Unlike distance bias, the biased score resulting from such a misrepresentation is not a valid rating of any trait in the domain.[44]

The distinction between distance bias and representation bias enables us to clarify the meaning of a "trait-method unit" (Fiske and Campbell, 1992; Cronbach, 1995). Campbell and Fiske (1959) coined this phrase to suggest that any observed score partly reflects the trait score and partly the method used to obtain it. While the intuition behind the phrase is useful, the phrase itself is unclear because a score cannot reflect a method (as methods have no scores). We argue that scores are not "trait-method units." Scores are simply scores. Whether a score reflects the trait score depends on its level of bias. A score that contains significant distance bias will not measure the intended trait, but it could be a valid measure of a different trait. A score that contains significant representation bias will not reflect the true score of any trait.[45]

If distance bias can result in valid measures of different traits, what can such traits be? Like Chin (1996), we turn to ontology to answer this, specifically Searle's (1995) ontological

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[44] Note that the human tendencies that cause representation bias (e.g., acquiescence) do exist in the domain and can be measured to control for representation bias, but they too would have to be measured without representation bias.

[45] There is one exception to this argument. In the case of secondary data, the act of measurement occurs in the domain, so representation bias could exist naturally in such data. The majority of misrepresentations in secondary data will not result in valid measures of traits, but in rare instances they could do so. For example, performance appraisals may sometimes be biased ratings of actual performance but valid ratings of what managers wish others to think are their ratings, e.g., for impression management (Arvey and Murphy 1998). Although we admit that such meaningful instances of representation bias are sometimes possible, we believe that they would be nearly impossible to identify in practice, because researchers typically have no knowledge of how secondary data is produced.

analysis. Searle's (1995) analysis is useful because it is consistent with critical realist assumptions and it provides a way to classify the types of traits that method variance can influence. Searle (1995) submits that all real world traits are either *ontologically objective* (that is, traits that exist outside the human mind, such as height) or *ontologically subjective* (that is, traits that exist in the human mind, such as joy). Furthermore, Searle (1995) argues that there are two types of ontologically objective traits: *observer-independent* traits that exist irrespective of human thought (e.g., height), and *observer-dependent* traits that have been 'objectified' because a class of observers sees them (subjectively) in a similar way (e.g., IT spending is objective as it occurs outside the mind, but it is observer-dependent as different people may define "IT" differently, per (Orlikowski and Iacono 2001). Of these types of traits, Searle (1995) suggests that social science is generally concerned with *ontologically subjective* traits (e.g., psychological traits), and *ontologically objective, observer dependent* traits (e.g., sociological traits).[46]

Searle's (1995) analysis reveals several ways that distance bias can lead to a valid rating of an unintended trait (see Figure 5.3). In the case of *ontologically objective* traits, a rater may lack access to the intended frame of reference for rating the trait. For example, a rater may rate IT spending differently because s/he views IT differently (see Figure 5.3, path D_1). In this case, the rating is true, but it reflects a different trait. Another reason why ratings of objective traits may have distance bias is that a rater may lack access to the true score of the intended trait (e.g., past observable behavior) and instead rate their perception of it (a subjective trait). As perceptions are fallible, this perception may be 'true,' but it differs from the objective trait (Figure 5.3, path D_2).

[46] Both ontologically objective traits (e.g., IT spending) and ontologically subjective traits (e.g., joy) can be studied from an epistemologically objective perspective (e.g., whether IT spending occurs or whether a person is joyful) and an epistemologically subjective perspective (e.g., whether IT spending or joy are good) (Searle 1995). Our analysis applies equally to both epistemological dimensions, but we omit a discussion of this issue due to space constraints.

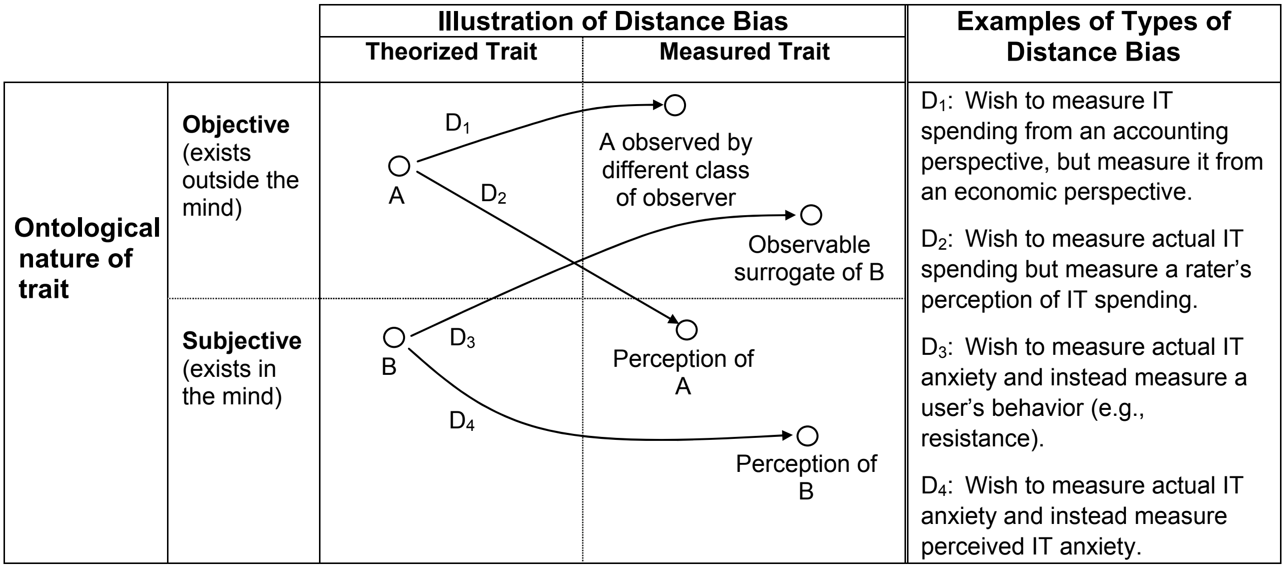


Figure 5.3: Valid, but Unintended Traits Measured by Distance Bias

In the case of *ontologically subjective* traits, the rater may lack access to the subjective trait (e.g., a person's intelligence), and instead may rate an objective trait in the hope that it is a good surrogate (e.g., a person's work performance) (per Figure 5.3, path D_3). Psychological research suggests another reason why ratings of subjective traits suffer from distance bias: many mental states are unconscious and inaccessible to conscious scrutiny (Wilson and Dunn 2004). Thus, people literally do not have access to their own mental traits, e.g., those involved in "perception, attention, learning, evaluation, emotion, and motivation" (Wilson and Dunn, 2004, p. 499). Thus, perceived subjective traits can be true *perceptions* of the trait of interest, but they can differ from the actual subjective traits of interest (see Figure 5.3, path D_4).

It is important to note that the "distance" of the paths in Figure 5.3 is not necessarily fixed. For example, it is possible to use instruments or procedures to reduce many of these biases (i.e., shorten the paths), e.g., by training raters so that their perceptions more accurately match reality. Although some approaches for dealing with method variance recommend such techniques (Hoyt and Kerns 1999; Martell and Leavitt 2002), such approaches must be used with caution. The ultimate arbiter of validity is whether a measure captures the trait score. Thus, if the trait of interest is *actual* anxiety, rater training may help in obtaining valid measures. However, if the trait of interest is *perceived* anxiety, rater training will be counter-productive because it will yield ratings that differ systematically from a person's natural (i.e., naturally 'biased') perception.

Because each type of distance bias listed in Figure 5.3 reflects a violation of construct validity (i.e., a failure to measure the intended trait), each one is a serious threat to research. However, because distance bias is meaningful, it is only a threat if misinterpreted. If interpreted correctly, it could lead to an opportunity, e.g., the identification of a trait. For example, Straub et al. (1995) found that self-reported and computer-recorded measures of IS usage did not highly correlate. They argued that self-reported measures might be biased measures of actual usage, but valid measures of "perceived usage," a new construct (per Figure 5.3, path D_2). Other such findings may exist in the literature. However, until now, no classification of types of distance bias has existed to assist researchers in systematically identifying such findings. Our proposed classification in Figure 5.3 is a first step in this direction.

5.3.4 Rethinking the Logic of Triangulation

Based on the preceding analysis, we propose that any strategy for dealing with method variance must account for the possibility that method variance can lead to valid measures of different traits. In this light, recall that Campbell and Fiske (1959) recommended that researchers use triangulation to deal with method variance. Triangulation is convergent, i.e., the aim is to converge on the true score (Crano, 1981, Sechrest et al. 2000). Our analysis suggests that ratings of a trait can *diverge* for two reasons: representation bias and/or distance bias. Convergence is an appropriate, albeit partial, logic for addressing *representation* bias, consonant with Levins' (1966) view that "truth lies at the intersection of multiple lies" (p. 423). However, convergence is an inappropriate logic for dealing with *distance* bias because distance bias can create multiple truths and a converged score will not reflect any of them. Thus, we suggest that if representation bias can be ruled out, a divergence among scores does not indicate complete invalidity. Rather, it signals the valid measurement of one or more *unexpected* traits, and the need to explore the significance of each one for the research. In short, triangulation is insufficient for dealing with method variance because it overemphasizes the importance of convergence. A more holistic approach is needed that acknowledges the importance of both convergence and divergence.

Our preceding arguments are based on ontology (Searle, 1995) and psychology (Wilson and Dunn, 2004). However, our arguments also have a basis in epistemology. For example, Churchman (1971) defined five different systems of inquiry (i.e., epistemological strategies): Leibnizian, Lockean, Kantian, Hegelian, and Singerian. Although each one is different, two of these (Leibnizian and Lockean) are generally considered to follow a logic of convergence; they both strive for one "true" model of reality (Messick, 1989). Two other strategies (Kantian and Hegelian) are generally considered to follow a logic of divergence (Messick, 1989). The Kantian view is pluralistic, allowing multiple truths, whereas the Hegelian view is dialectic, encouraging rivalry among alternative truths. The fifth strategy (Singerian) subsumes the preceding strategies and, thus, utilizes both convergence and divergence (Messick, 1989). In summary, convergence is just one of several epistemological strategies and a researcher should use it only to the degree to which it is consistent with the objectives and assumptions underlying his/her study.

5.4 A Comprehensive Approach for Dealing with Method Variance

To overcome the limits of past approaches, we propose a comprehensive approach for dealing with method variance (see Figure 5.4). The approach consists of two phases: selecting a strategy, and executing a strategy. We discuss each phase below.

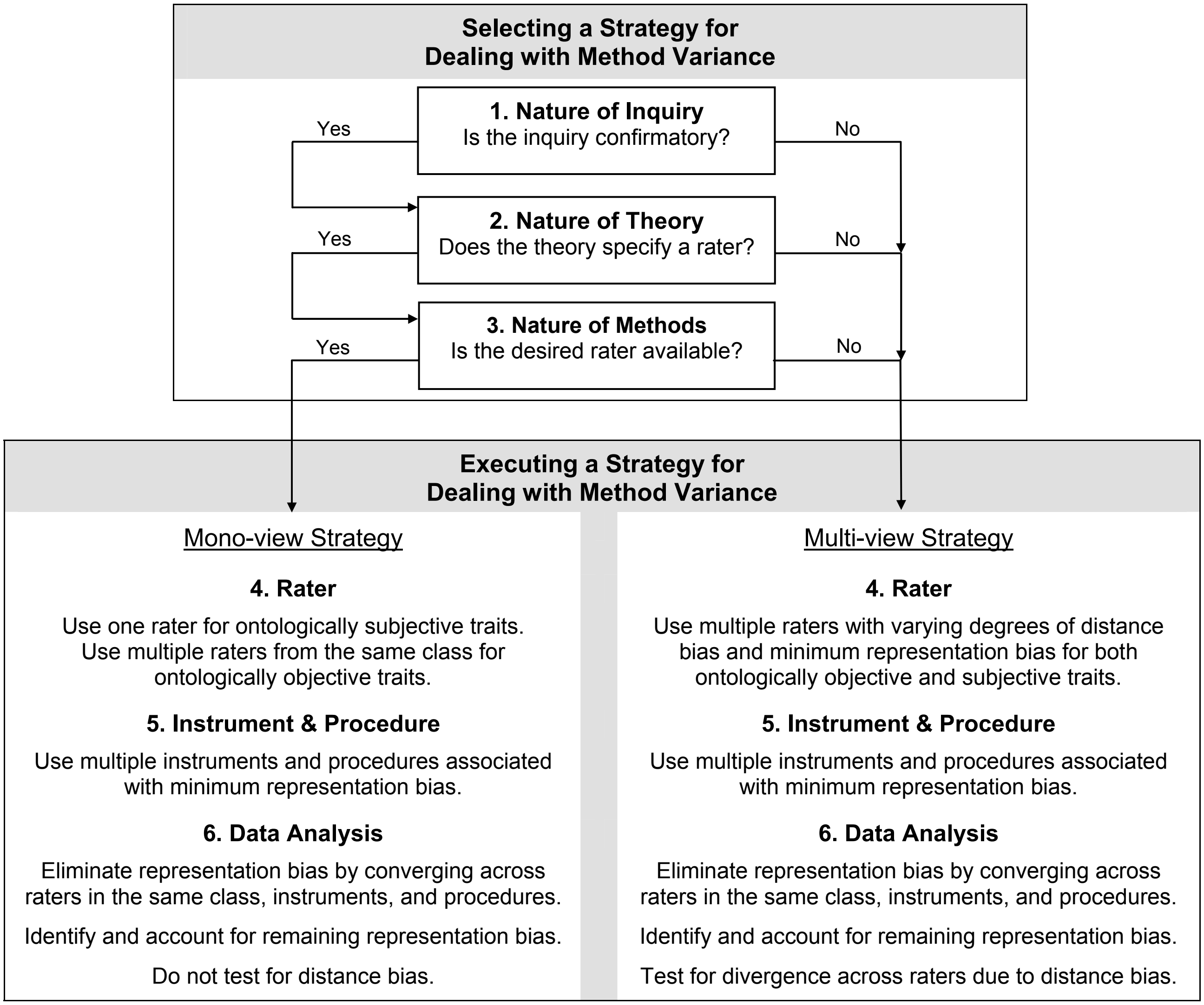


Figure 5.4: A Comprehensive Approach for Dealing with Method Variance

5.4.1 Selecting a Strategy for Dealing with Method Variance

We propose two general strategies for dealing with method variance: the mono-view strategy, which is appropriate when distance bias is not important, and the multi-view strategy, which is appropriate when distance bias is important. Three criteria determine the importance of distance bias in a study and, thereby, determine which strategy should be used.

5.4.1.1 Nature of Inquiry: Is the Inquiry Confirmatory?

The first criterion for determining the relevance of distance bias is the *nature of the* *inquiry*. Churchman's (1971) typology allows us to distinguish three types of inquiries:

- Confirmatory inquiry: aims to test a single truth (i.e., Lockean/Liebnizian inquiry)
- Pluralistic inquiry: aims to reveal multiple truths (i.e., Kantian inquiry)
- Dialectic inquiry: aims to weigh competing truths (i.e., Hegelian inquiry)

Clearly, all of these inquiries are useful. *Confirmatory* inquiry is useful because it provides direct evidence for whether a theory is supported empirically. An example would be Gefen's et al. (2003) investigation of whether theories of IT acceptance and trust predict users' online shopping behavior. *Pluralistic* inquiry is useful for extending theories by revealing the observer-dependent nature of a theory's constructs. An example would be to empirically test DeLone and McLean's (1992) argument that different observers conceive IS success differently. This argument underpins DeLone and McLean's (1992) IS success model, but to our knowledge, this issue has never been formally investigated. The usefulness of *dialectical* inquiry has long been noted (Platt 1964). An example would be to identify situations in which different observers' views of IS success are 'better' than other observers' views, in the sense that they are more predictive of certain outcomes.

As Figure 5.4 shows, if a researcher is following a confirmatory inquiry, they must look to the next two criteria (nature of theory and nature of methods) to select an appropriate strategy. However, if the inquiry is pluralistic or dialectical researchers should use the *multi-view strategy*. This is because these inquiries do not aim to *confirm* the role of a construct in a given theory. Rather, they aim to extend theory, for example, by investigating whether different classes of observers have legitimately different perspectives of the construct, or whether the construct is defined in terms of the class of observer with the 'best' perspective. Distance bias is important in all of these cases and, thus, a multi-view strategy is appropriate.

5.4.1.2 Nature of Theory: Does the Theory Specify a Rater?

The second criterion for determining the importance of distance bias is the nature of the theory. In some theories, constructs are defined in terms of a rater. For example, in the case of ontologically subjective constructs, some constructs are defined in terms of the perceptions of a specific person, such as the end-user who 'perceives' the ease of use, usefulness, or fit of a new IS (Davis 1989; Goodhue 1995). Many ontologically objective constructs are also defined in terms of specific raters because such constructs are typically observer-dependent (Searle 1995). For example, the IS success model attempted to classify the ways different observers view IS success (DeLone and McLean 1992). This is not to say that all traits are defined in terms of a rater. For example, cognitive absorption (Agarwal and Karahanna 2000) is an ontologically subjective trait that refers to the degree to which a user is engaged in his/her use of an IS, not his/her *perception* of his/her engagement. Likewise, observable usage behavior (Davis 1989), objective fit (Goodhue 1995), and individual performance (Goodhue and Thompson 1995) all refer to ontologically objective traits, but none are defined in terms of a specific class of observer.

As Figure 5.4 shows, in situations where a construct is defined in terms of a specific rater, we suggest that researchers must use the next criterion (nature of methods) to select the appropriate strategy for dealing with method variance. However, if a theory does *not* specify a particular rater, researchers must either:

(1) re-define the construct in terms of a specific rater, and/or
(2) investigate whether different raters perceive the construct differently.

If a researcher redefines a construct in terms of a specific rater, s/he would need to use the next criterion (nature of methods) to select an appropriate strategy, but if s/he wishes to investigate whether different raters perceive the construct differently, the *multi-view strategy* is appropriate.

A good example of this reasoning is the notion of 'IT spirit.' DeSanctis and Poole (1994) argued that different observers might conceive of an IT's spirit differently. In a later empirical study of spirit, Chin et al. (1997) *redefined* spirit in terms of end users' perception of spirit and obtained ratings from end users based on this redefinition. To our knowledge, no study has examined the implications that could stem from different raters (e.g., end users and managers) having different views of an IT's spirit or different views of each other's view of an IT's spirit.

5.4.1.3 Nature of Methods: Is the Desired Rater Available?

The final criterion for determining the importance of distance bias is *the nature of methods*. Even if an inquiry is confirmatory and a construct is defined in terms of a rater, a researcher may be unable to obtain data from the specified rater simply due to research realities of. In such cases, it may sometimes be better to obtain an approximation of the rater's perspective than no perspective. For example, assume that a researchers wishes to measure an employee's perception of his/her supervisor's abilities. A researcher may have difficulty obtaining this employee's perception because the employee may be unwilling to disclose his/her true perception to the researcher. In such instances, an alternative would be to obtain other raters' perceptions of the employee's perception, e.g., by asking his/her subordinates or peers. The limitation is that these ratings could suffer from distance bias. For example, these raters could be rating a different trait (e.g., his/her observable behavior, per Figure 5.3, path D_3), and they may even have different perceptions of this different trait (e.g., peers and subordinates may perceive the employee's behavior differently, per Figure 5.3, path D_1). As a result, it is important in such instances to test for distance bias rather than simply assume that the different views converge on the intended trait. Thus, as Figure 5.4 shows, a mono-view strategy is appropriate only if the desired rater is available; the multi-view strategy is appropriate if the desired rater is not available.

5.4.2 Executing a Strategy for Dealing with Method Variance

To address method variance, researchers must use methods (i.e., raters, instruments, and procedures) in such a way that unwanted biases are prevented from influencing the data and must analyze data in such a way that remaining biases are identified and accounted for (per Figure 5.4).

5.4.2.1 Executing a Strategy: Rater

As Figure 5.4 shows, the two strategies have differing requirements regarding raters. The *multi-view* strategy requires that researchers select multiple raters with varying distance bias and minimum representation bias. In contrast, the *mono-view* strategy requires that researchers use one rater for ontologically subjective traits and multiple raters from the same class of raters for ontologically objective traits. In the mono-view strategy, this rater, or class of raters, should be the one specified in the theory (e.g., the user perceiving an IT's ease-of-use with the concomitant construct called "perceived ease-of-use"). By definition, this rater will have minimum distance bias. Nevertheless, s/he may still provide ratings with representation bias. The following steps of the approach (instruments, procedures, and data analysis) offer ways to eliminate, identify, and account for such representation bias. However, if the level of representation bias is expected to be very substantial, this would signal a limitation of methods and the researcher would need to revert to the multi-view strategy, as described above.

5.4.2.2 Executing a Strategy: Instrument and Procedure

Because distance bias only relates to raters (per Table 5.3), the two strategies adopt the same approach regarding *instruments and procedures*: to use instruments and procedures that are associated with minimum representation bias. Because representation bias is unlikely to be zero with any instrument or procedure, using multiple instruments and procedures enables researchers to attempt to triangulate (i.e., converge) on the trait scores (Sechrest et al. 2000, Levins 1966).

5.4.2.3 Executing a Strategy: Data Analysis

As Figure 5.4 shows, the two strategies deal with representation bias in the same manner. First, they attempt to eliminate representation bias by converging across scores obtained from different raters (within the same class), different instruments, or different procedures. Second, they attempt to identify and account for any remaining representation bias in the data. Statistical techniques for doing so are reasonably mature, such as the use of structural equation modeling techniques to identify and account for common method bias (Podsakoff et al. 2003).

In addition to testing for representation bias, the multi-view strategy requires testing of distance bias. Techniques for doing so include tests for differences in scores of a trait by different raters (e.g., via comparison of means) and tests for differences in relationships depending on the rater(s) used for measuring each trait (e.g., via comparisons of path coefficients). New techniques for testing distance bias are emerging, though not yet mature (Austin et al. 2002). Such emerging techniques include tests for measurement invariance, which examine whether different raters interpret items in a survey in the same way (Vandenberg 2002), and cognitive modeling techniques, which scrutinize the act of rating to determine whether a rater is indeed scoring the intended trait (Borsboom et al. 2004).

Overall, reasonable data analysis techniques for addressing representation bias and distance bias exist. What has been missing is an approach for choosing an appropriate set of such techniques for a given study. The proposed approach provides a first step in this direction.

5.4.3 Using the Approach in Practice: The Importance of Judgment and Multiple Studies

The proposed approach is the first comprehensive guide for dealing with method variance. However, it has two important limitations. First, its requirements are onerous. For example, the multi-view strategy suggests that researchers consider rating each construct in a study in at least eight ways (e.g., 2 raters * 2 instruments * 2 procedures). This will generally be infeasible in any single study. Second, it provides little guidance for resolving tradeoffs. For example, if a researcher wishes to measure an employee's desire to resign, it may reduce distance bias if the rater is the employee, but it may reduce representation bias if the rater is an independent observer. The approach offers no simple rule for resolving this type of tradeoff.

These limitations highlight how the approach should be used. Like Campbell and Fiske's (1959) MTMM matrix, the approach requires researchers to use *judgment*. It provides guidance, but no simple recipe. For example, the multi-view strategy does not require a researcher to use multiple raters, multiple instruments, and multiple procedures for every construct, and to manage all the tradeoffs among these, in one study. Rather it provides guidance for what a researcher must *consider* addressing in a study, emphasizes the need to justify those aspects s/he does address, and highlights what areas will remain to be investigated in the future. In other words, the approach recognizes the inherent limitations of any single study (Schmidt 1992). By systematically varying aspects of the approach over multiple studies, however, the approach can be used to build consensus regarding the true relationships among traits in a domain over time.

In summary, we suggest that researchers use the approach in the following way: (1) follow the approach to the extent possible in a single study, using judgment to carry out each step, (2) acknowledge limitations that exist in a study's data that stem from not fully using the approach, and (3) use the approach in a systematic way across studies to build consensus, e.g., by using it in concert with an approach for replications and extensions (Berthon et al. 2002).

5.5 Dealing with Method Variance: A Demonstration Exercise

If the proposed approach is beneficial, it should enable researchers to obtain more persuasive and meaningful results if it is used than if it is not used. We conducted an empirical study to test this proposition and demonstrate the feasibility of the approach. The sections below describe the study context, detail how the proposed approach was used, and present the results.

5.5.1 Context of the Demonstration

As the proposed approach focuses solely on the selection of methods and data analysis techniques, it assumes that a researcher has already selected a theoretical and empirical context. To focus this demonstration as much as possible on the applying the proposed approach, we adopted the theoretical and empirical context of an existing study: Burton-Jones and Straub (2004) (i.e., Chapter 2). We chose this study for two reasons. First, it studies an important context: the relationship between system usage and task performance. System usage is a central construct in IS research, yet there have been repeated calls to examine it more closely (DeLone and McLean 2003; Ginzberg 1978; Trice and Treacy 1986), especially in relation to its link with performance (Chin and Marcolin 2001).

Second, Burton-Jones and Straub (2004) present a detailed account of usage *measures*, but are silent on what *methods* to use to obtain these measures. Moreover, there is little consensus in IS literature regarding the best methods for measuring system usage. Most studies use self-report questionnaires and focus on ontologically objective aspects of usage (see Appendix 5A). However, several studies have shown that results can vary depending on the aspect of use measured and the method used to measure it (Collopy 1996; Straub et al. 1995; Szajna 1996). Thus, this context serves as a good test case for examining the usefulness of the approach.

We therefore designed this study as a methodological extension of the Burton-Jones and Straub study (per Berthon et al. 2002). We describe the theoretical and empirical context of the study below.

5.5.1.1 Theoretical Context

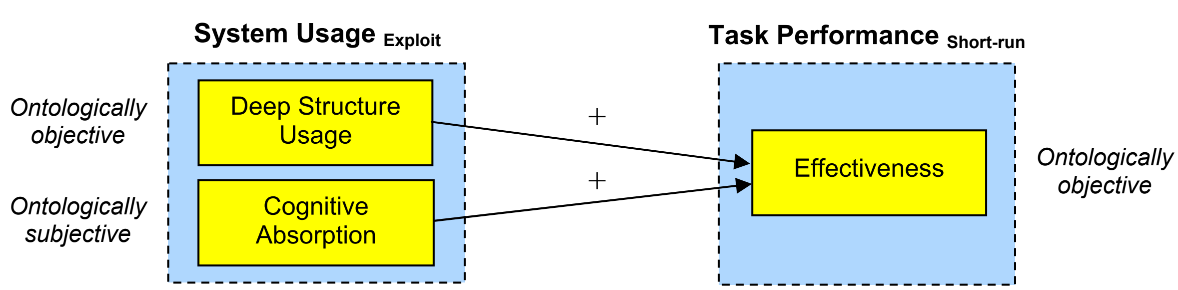
Figure 5.5 presents the theoretical model proposed by Burton-Jones and Straub (2004). They defined system usage and task performance as follows:

- *System usage*: a user's employment of one or more features of a system to perform a task.
- *Task performance*: an assessment of task outputs in terms of effectiveness.

Burton-Jones and Straub (2004) limited their account of the relationship between system usage and task performance to short-run, cognitive engaging tasks. They theorized that in this context, the type of usage that most closely relates to performance is a user's exploitation of his/her knowledge of the IS to perform the task (March 1991). They proposed that such "exploitive" usage could be modeled via two dimensions:

- *Cognitive absorption*: The extent to which the user is cognitively engaged in his/her use of the system to perform the task.
- *Deep structure usage*: The extent to which the user is using the features of the system that support the structure of the task.

Cognitive absorption reflects an *ontologically subjective* trait because it exists in the user's mind. Deep structure usage and performance are *ontologically objective, observer-dependent* traits as they are observable, but different observers may view them differently (see Figure 5.5).



Legend: the subscripts 'Exploit' and 'Short-run' indicate that this theoretical model specifies the relationship between exploitive system usage and short-run task performance.

Figure 5.5: Theoretical Model for the Demonstration Exercise

5.5.1.2 Empirical Context

To test the theoretical model in Figure 5.5, Burton-Jones and Straub (2004) conducted an experiment in which they examined accountants' use of Microsoft Excel for financial analysis and they tailored their measures for each construct to this specific domain. We adopt the same empirical context in this investigation. This allows us to use some of the measures in the Burton-Jones and Straub (2004) study and concentrate on applying the proposed approach for selecting methods and data analysis techniques. We briefly summarize the experiment below.

Task and Design. The task required a user to build a spreadsheet model in MS Excel to determine the best approach for financing an asset purchase. The task enabled a strong test of the theoretical model as it was cognitively engaging, which allows variation in cognitive absorption, the system contains features that support the task, which allows variation in deep structure usage, and it required the student to complete the task within 1.5 hours, which enabled a test of short-run task performance. The experiment used a free simulation design (Fromkin and Streufert 1976), which allows values of the IVs (i.e., cognitive absorption and deep structure usage) to vary freely over their natural range. This design was chosen rather than a factorial design because it gives an insight into nature of the IV→DV relationship without restricting the range over which it occurs.

Subjects. Subjects were 229 students in an intermediate accounting course in a southern U.S. university. Data were collected during an end-of-semester case-exam worth 10% of students' grades. The case used the same general format as in eight previous assignments that the students had completed and it involved accounting concepts learned during the course (present value, asset financing, and risk/return). Therefore, to the greatest extent possible, the system and task were believed to enable exploitive use by the subjects.

5.5.2 Demonstration of the Approach

Given the theoretical and empirical context outlined above, Figure 5.6 summarizes how we applied the proposed approach. The following sections detail our application of each step.

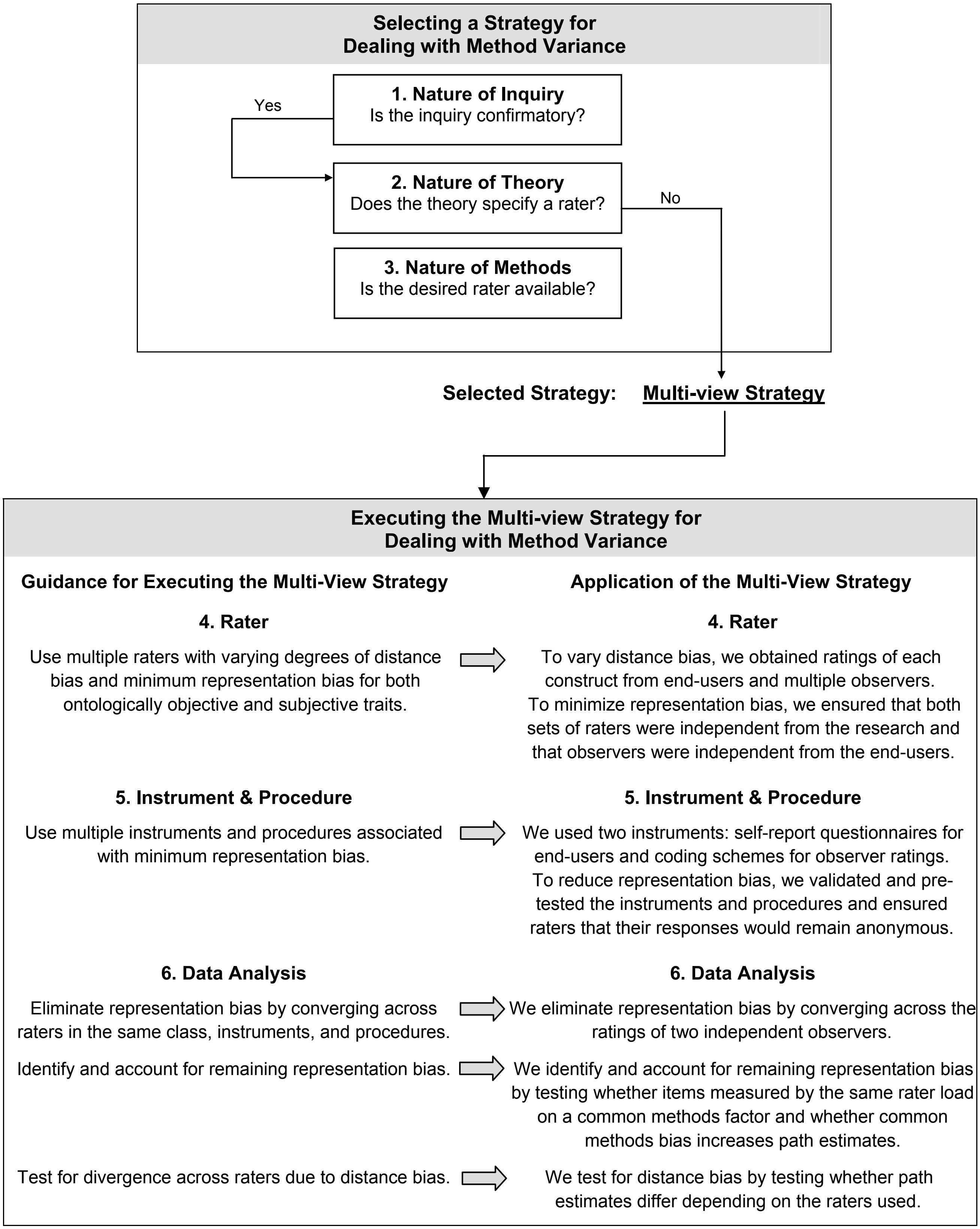


Figure 5.6: Applying the Proposed Approach in this Demonstration Exercise

5.5.2.1 Selecting a Strategy for Dealing with Method Variance

The first criterion for selecting a strategy for dealing with method variance is to consider the nature of the inquiry. Burton-Jones and Straub (2004) conducted a confirmatory inquiry, i.e., they set out to examine whether an empirical test would confirm the theoretical model in Figure 5.5. Because the aim of this demonstration is to show how the proposed approach would apply in the context of the proposed theoretical model, it adopts the same confirmatory approach as in Burton-Jones and Straub (2004). The next step is to examine whether the theory specifies a rater. This is the deciding step here because Burton-Jones and Straub (2004) did not define their theoretical constructs in terms of specific raters. Thus, as Figure 5.6 shows, the multi-view strategy is appropriate in this case, irrespective of the nature of methods.

5.5.2.2 Executing a Strategy for Dealing with Method Variance

Raters. The multi-view strategy requires researchers to use multiple raters with varying distance bias and minimum representation bias.

To achieve varying *distance* bias, we selected two classes of raters: end-users and observers. We chose these two classes because differences between their views could be relevant in practice, e.g., in the context of judging user performance at work. As Figure 5.7 shows, the use of these raters creates the possibility for many types of distance bias. For the ontologically objective traits (deep structure usage and performance), end-users and observers could differ in their scores for two reasons: (1) end users and observers may view the task differently, due to differing knowledge of the task or system (Figure 5.7, path D_1), and (2) end-users and observers may provide incorrect, but nevertheless true, perceptions of deep structure usage and performance because of memory limitations (Blair and Burton, 1987) (Figure 5.7, path D_2). Likewise, for the ontologically subjective trait (cognitive absorption), end-users and observers could provide different scores for these traits because: (1) observers (and to some

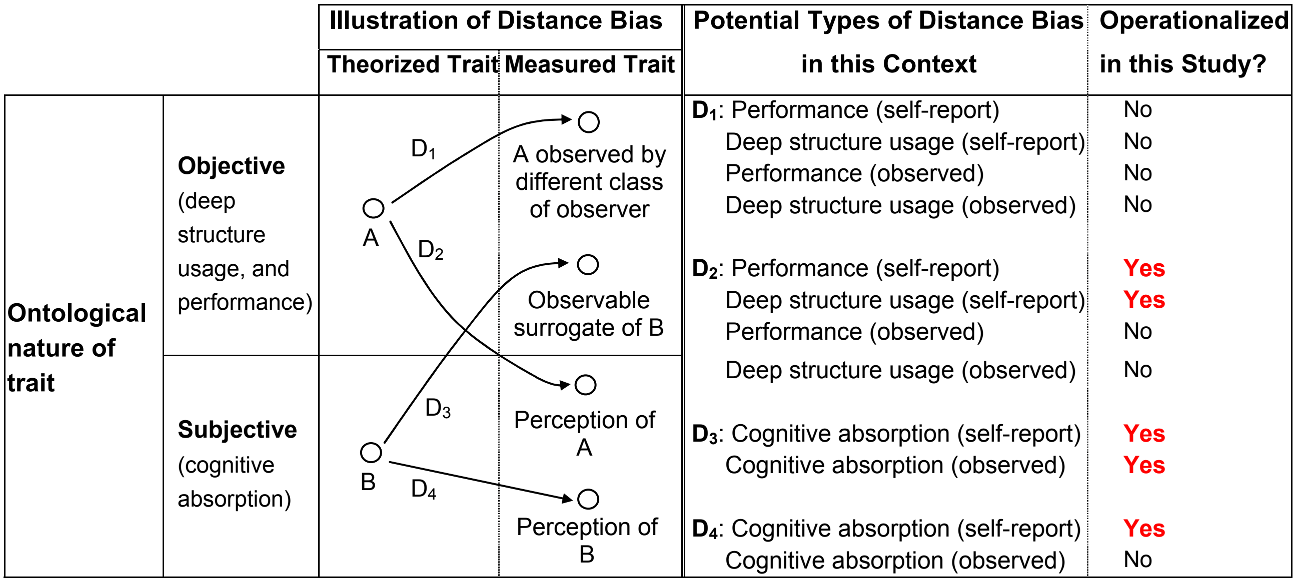


Figure 5.7: Distance Bias in the Demonstration: Potential Biases and Operationalized Biases

degree, even end-users) lack access to the end-user's actual cognition and are restricted to rating an observable surrogate (Bem 1972) (Figure 5.7, path D_3), and (2) observers may have incorrect perceptions of the surrogate they observe, and end-users may have incorrect perceptions of their cognitions as well as their own observable behavior (Wilson and Dunn 2004) (Figure 5.7, path D_4).

As Figure 5.7 shows, because of the sheer number of these potential distance biases, we restricted the types of distance bias that could occur in the study so that our data would allow a precise and conservative test, i.e., so that if results varied, we would be more able to discern which aspect of distance bias caused the difference. Specifically, we took the following actions:

- *Used a common benchmark:* Both sets of raters scored the ontologically objective traits (deep structure usage and performance) from the same perspective. This reduced the distance bias shown in Figure 5.7, path D_1. To operationalize this in the study's empirical context, each observer and student rated these traits from the perspective of course instructors (e.g., What score would the instructor likely give you for your work?).
- *Gave observers a full record:* We provided observers with a complete record of users' work, reducing the risk that observer perceptions would err due to memory limitations. This reduced the type of distance bias in Figure 5.7, paths D_2 and D_4. To operationalize

this, we provided coders with each user's task output and a video record of his/her Excel use. The video data was subject to protocol analysis and for practical reasons, it was limited to the work of 46 users. This sample is significantly larger than samples used in prior protocol analyses in IS research (e.g., n=18 (Mackay and Elam 1992), n=6 (Vessey and Conger 1994) and n= 7 (Kim et al. 2000)).[47]

To achieve minimum *representation bias*, we followed Campbell and Fiske's (1959) principle to ensure that raters were independent. This meant ensuring that end-users and observers were independent from the research (so they had no incentive to provide confirming results) and that end users and observers were independent from each other (so that observers have no incentive to misrepresent users' work). To further reduce representation bias in observer ratings, we used two raters from the same observer class to provide us with the data necessary to later eliminate idiosyncratic representation bias by converging across rater scores.

In summary, we achieved the objective of this step (i.e., having multiple raters with varying distance bias and minimum representation bias) by obtaining ratings for each construct from end-users and multiple expert observers. These classes of raters were independent from the study and from each other. Table 5.4 summarizes our operationalization of this step in the study.

Table 5.4: Raters used in the Experiment

Type of Construct	Type of Rater	Explanation of Rating
Ontologically objective traits (Deep structure usage and performance)	End-users	Each student performing the exercise rated deep structure usage and performance from the perspective of the course instructors (e.g., What percent do you think the course instructor would give you for your work?).
	Observers	Two independent coders rated deep structure usage (by reviewing video records of end-users' use of Excel) and task performance (by reviewing end-users' task outputs). These two constructs were rated according to coding schemes that matched the perspective of the course instructors.
Ontologically subjective trait (Cognitive absorption)	End-users	Each student performing the exercise rated his/her cognitive absorption.
	Observers	Independent coders used video records of end-users' Excel use to rate cognitive absorption. Coders rated cognitive absorption according to a coding schemed that matched observable correlates of this trait.

[47] The sub-sample for protocol analysis did not differ significantly from the full sample (e.g., knowledge of Excel [r = .11, p = .17], final exam performance [r = .08, p = .23], and average performance in past exercises [r = .11, p = .12]).

Instruments and Procedures. The multi-view strategy requires researchers to use multiple instruments and procedures associated with minimum representation bias.

We selected two instruments: questionnaires for end-users and structured coding schemes for observer ratings. Table 5.5 lists the instruments, which we discuss in turn below.

Table 5.5: Instruments used in the Experiment

Construct	Instrument	Items
Cognitive absorption	Self-Report Questionnaire (reflective scale)	CS1: When I was using MS Excel, I was able to block out all other distractions CS2: When I was using MS Excel, I felt totally immersed in what I was doing CS3: When I was using MS Excel, I felt completely absorbed in what I was doing CS4: When I was using MS Excel, my attention did not get diverted very easily
	Independent Observation (formative scale)	CI1: Clear strategy: Independent rater's perception of whether a user was following a clear strategy in his/her work. CI2: Activity: Percentage of a user's work time that s/he appeared to be working. (Instances of non-work were those in which no activity occurred for > 4 secs)
Deep structure usage	Self-Report Questionnaire (reflective scale)	DS1: When I was using MS Excel, I used features that helped me compare and contrast aspects of the data DS2: When I was using MS Excel, I used features that helped me test different assumptions in the data DS3: When I was using MS Excel, I used features that helped me derive insightful conclusions from the data DS4: When I was using MS Excel, I used features that helped me perform calculations on my data
	Independent Observation (formative scale)	DI1: The extent to which students used "function" features (e.g., to write formulae) DI2: The extent to which students used "fill" features (e.g., to copy or fill data and/or formulae across a spreadsheet)
Performance	Self-Report Questionnaire (formative scale)	PS1: *Overall*: My instructor would probably give me the following score in percentage terms on the case I just completed. [Open ended scale]. PS2: *Quantity*: I believe that I completed all required parts of the case PS3: *Quality*: The instructor would probably consider my work to be of very high quality
	Independent Observation (single item, reflective)	PI1: This single percentage score allocated marks for the following elements (identifying the problem, building a flexible model, correctly analyzing the data, identifying solutions, highlighting impacts, focusing the report, and giving clear recommendations).

†Unless indicated, all self-report questionnaire items used a 9-point strongly agree-strongly disagree Likert scale.

For cognitive absorption, the questionnaire used Agarwal and Karahanna's (2000) pre-validated, reflective scale, which was also the same scale employed in Burton-Jones and Straub (2004). The coding scheme for observer ratings of cognitive absorption was created afresh. Because the experimental case was a problem-solving task, we first looked to theories of cognition, which suggest that human problem-solving is carried out via a goal-directed sequence of cognitive operations (Ashcraft 2002). On this basis, we constructed one surrogate of cognitive absorption, ‘clear strategy,’ which was defined as the degree to which a user appeared to be undertaking a sequence of goal-directed (rather than haphazard) operations in his/her use of Excel. We then turned to research on flow (Csikszentmihalyi 1990). Flow theory suggests that the behavior of cognitively absorbed individuals should, as the term suggests, ‘flow.’ Such behavior occurs because the individual feels in control of his/her behavior and his/her attention is focused in the task (Agarwal and Karahanna 2000). This suggests that cognitively absorbed users should show evidence of continued, ‘flowing,’ rather than interrupted work. We therefore chose a second surrogate of cognitive absorption: ‘activity,’ defined as the proportion of a user’s work time that s/he appeared to be working. Research suggests that conscious cognitive processes take “more than a second or two for completion” (Ashcraft 2002, p. 145). Thus, short pauses cannot necessarily be considered non-work. To be conservative, we coded breaks in activity of more than 4 seconds to be instances of idle time (i.e., breaks in flow). Although theory suggests that these two surrogates (clear strategy and activity) both stem from cognitive absorption, they need not correlate highly. For example, someone could be following a clear strategy and yet break for 5 seconds, or could be working actively but haphazardly. Thus, we model these two items as formative rather than reflective items (Chin 1998) (see Table 5.5).

For deep structure usage, we used the reflective questionnaire scale from Burton-Jones and Straub (2004). They defined deep structure usage as use of features in the IS that support the structure of the task. To ensure that the scale that matched the perspective of course instructors, a course instructor supplied statements to characterize the structure of the task — which they defined as analyzing data, testing assumptions, and deriving conclusions — and following the approach in recent research (Subramani 2004), the items were adapted for this task domain (see Table 5.5). To construct a coding scheme for observer ratings of deep structure usage, we first constructed a comprehensive listing of Excel features. We then asked two instructors to indicate the degree to which each feature supported the task's deep structure (i.e., analyzing data, testing assumptions, and deriving conclusions). Their ratings converged and indicated that two features were the primary deep structure features: (1) functions (used for writing formulae) and (2) filling (used for completing rows or columns of data using absolute or relative cell references) [ICC (2,2) = .76] (Shrout and Fleiss 1979). We consider these two items to be formative because a user may trade off use of one for another (e.g., by using a 'fill' feature to copy formulae across cells rather than writing the formulae directly) (Diamantopoulos and Winklhofer 2001).

The scale for performance was created afresh. In the performance measurement literature, work effectiveness has two components: quality and quantity (Meister 1986). We therefore constructed a three-item self-report scale containing questions regarding overall performance, quality of work, and quantity of work, all adapted to ask about effectiveness from the perspective of the instructor or the course requirements. The items are considered formative because students can trade-off quantity for quality, or vice versa (Bollen and Lennox 1991). Independent from the research, the course instructors developed the coding scheme for observer ratings of performance. This scheme awarded students a single percentage score based on performance in seven aspects of the case.

Following recommended procedures (Goodhue and Loiacono 2002; Orne 1962; Podsakoff et al. 2003; Webb et al. 2000), we took steps to reduce the risk that these instruments and procedures would induce representation bias (see Table 5.6).

By following these steps, representation bias was reduced to a minimum. As noted earlier, tradeoffs have to be made among biases. One such tradeoff had to be made in this study. To vary distance bias, we determined (above) that each class of rater should rate each construct. This means that each end-user in our study rated each construct in the model. As a result, this

Table 5.6: Steps to Minimize Representation Bias in Instruments and Procedures

Instrument/Procedure	Steps to Minimize Representation Bias
Questionnaire	Ran a Q-sorting exercise, pre-test, and pilot test to eliminate ambiguous items. Randomized items in the questionnaire to reduce hypothesis guessing. Assured respondents that questionnaire responses would remain anonymous.
Procedure for responding to the questionnaire	Pre-tested and pilot-tested the questionnaire to ensure that respondents had sufficient time to complete the questionnaire and that the instructions were clear.
Coding scheme	Pre-tested the coding schemes to ensure that they enabled clear and accurate responses.
Procedure for using coding scheme	Conducted practice coding to identify and eliminate idiosyncratic coding biases and to ensure that coders had sufficient time to code the video protocols. Separated the observers from the users to eliminate demand characteristics. Used difference coders for the usage data (IVs) than the performance data (DV).

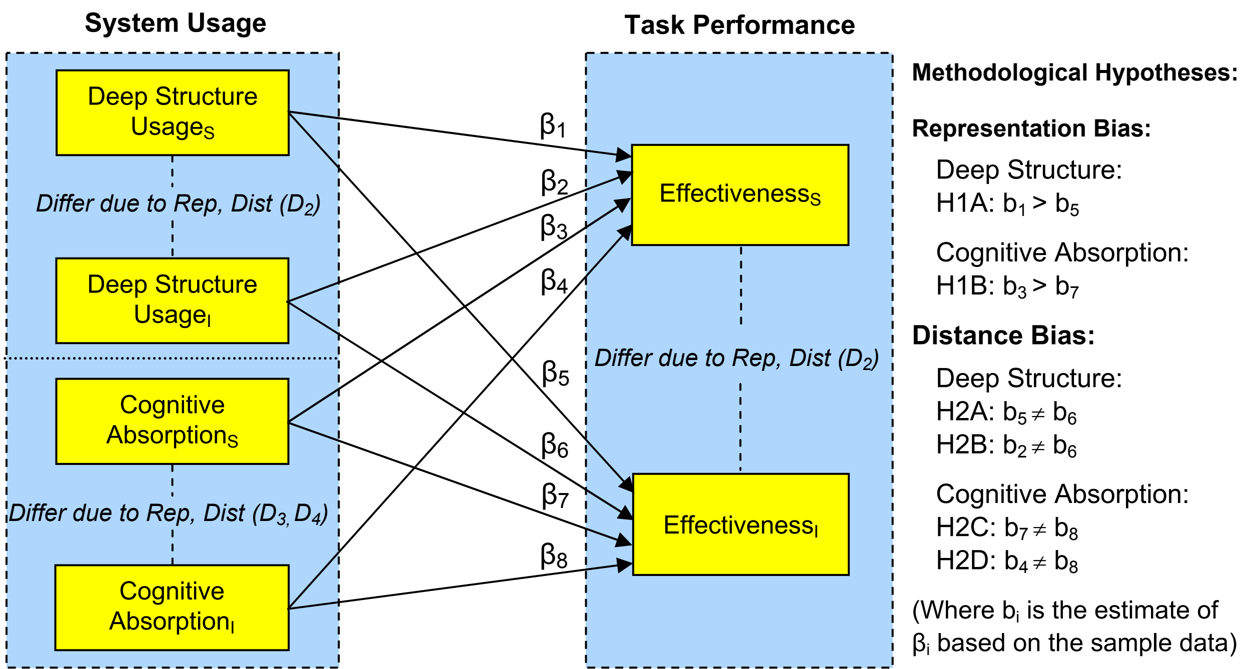
self-report data may be influenced by *common methods bias*, e.g., end-users may attempt to align their responses for the different traits to reduce cognitive dissonance. The ratings by observers are not subject to this risk because unlike the end-users, we could use different observers from the same observer class to rate the different traits.

To summarize, we followed the multi-view strategy to select raters, instruments, and procedures expected to result in data that contain varying degrees of distance bias and, with the exception of common method bias in the self-report data, minimum representation bias.

Data Analysis. The final step in the multiview strategy is to eliminate representation bias via converging across raters, instruments, or procedures; identify and account for any remaining representation bias in the data; and test for divergence due to distance bias (per Figure 5.6).

To eliminate idiosyncratic representation bias, we tested for the comparability of coder ratings (finding that they were comparable; ICC [2, 2] = .73, system usage, .87, performance), and converged (averaged) across the two coders' ratings of each construct. As noted above, the main source of representation bias that we expected to remain in the data following this step was common methods bias. To identify and account for it, we ran a model that included a "common method" factor and tested whether the items from each trait load on this factor (per Podsakoff et al. 2003). Finally, to test for distance bias, we examined whether the use of raters with different degrees of distance bias led to different estimates of the relationships between traits.

The proposed approach assumes that representation bias and distance bias are important. By carrying out the data analyses outlined above for these two sources of bias, the demonstration next can test whether distance bias and representation bias can, in practice, affect research results. To formalize this test, we operationalized the study's theoretical model in terms of methods and expected biases and created hypotheses to test the impacts of the two sources of bias (Figure 5.8).



Key:
Rep: Representation bias, Dist: Distance bias, $D_{2, 3, 4}$: Types of distance bias (per Figure 5.7)
Subscripts: $_S$: Self-report, $_I$: Independent raters (two raters for the IVs; two different raters for the DV).

Figure 5.8: Operationalized Theoretical Model

As Figure 5.8 shows, each pair of measures is expected to result in different scores, partly due to distance bias and partly due to representation bias. Nevertheless, we can draw hypotheses that allow us to maximally isolate the effect of each type of bias. The importance of *representation* bias should be apparent in the results from the self-report data. Specifically, the self-report items should load on the common method factor, and the IV→DV relationships should be stronger when self-report measures are used for both the IV and DV. We expect this pattern because the use of one questionnaire to obtain the IVs and the DV is expected to induce subjects to align responses (e.g., to avoid cognitive dissonance) (Podsakoff et al. 2003). Thus, using the notation in Figure 5.8, we pose the following methodological hypothesis:

H1: Common methods bias has a significant effect on the relationships in the theoretical model.

H1A: The items for Deep Structure Usage$_S$ and Performance$_S$ load on a common factor. Secondary test if H1A is supported: b1 > b5.

H1B: The items for Cognitive Absorption$_S$ and Performance$_S$ load on a common factor. Secondary test if H1B is supported: b3 > b7.

If distance bias is important in this study, the estimated relationships between constructs should be influenced by the distance bias in the model. To ensure that this difference stems from distance bias rather than representation bias, we test this hypothesis by comparing sets of paths where the IV and DV in each case are measured by different raters (i.e., the IV and DV are rated by different observers [IV observer/DV observer] or by different classes of raters [self/observer]). It is difficult to specify the direction of the expected difference *a priori* because distance bias results in measures of unintended traits and these traits (e.g., perceived absorption and perceived performance) may have stronger or weaker relationships than those between the intended traits. Thus, using the notation in Figure 5.8 we specify the following non-directional hypotheses:

H2: Distance bias has a significant effect on the relationships in the theoretical model.

H2A: Deep Structure Usage$_I$ will have a different relationship with Performance$_I$ than will Deep Structure Usage$_S$. In other words, b5 ≠ b6.

H2B: Deep Structure Usage$_I$ will have a different relationship with Performance$_I$ than with Performance$_S$. In other words, b2 ≠ b6.

H2C: Cognitive Absorption$_I$ will have a different relationship with Performance$_I$ than will Cognitive Absorption$_S$. In other words, b7 ≠ b8.

H2D: Cognitive Absorption$_I$ will have a different relationship with Performance$_I$ than with Performance$_S$. In other words, b4 ≠ b8.

5.5.2.3 Results of the Demonstration Exercise

We examined the results of the study in two steps. We first examined the descriptive statistics and instrument reliability and validity. Next, the hypotheses were tested. PLS was deployed for our statistical tests because it is the appropriate technique given the relatively small sample size for the video protocol data and the use of formative indicators for several constructs (Chin 1998).

Table 5.7 details the descriptive statistics. PLS does not require the data to meet strict distributional assumptions (Chin 1998). Nevertheless, the data indicate no significant violations of normality or multicollinearity, nor any significant outliers (although one case was deleted due to non-sensical responses). As Table 5.8 shows, the data from the reflective scales converged and discriminated in the expected way, indicating adequate validity, and the reliability scores exceed minimum guidelines, indicating adequate reliability (Nunnally 1967). Finally, as Table 5.9 shows, the formative items are all considered highly predictive of their respective traits, suggesting that each of these indicators is important (per Chin 1998). Overall, the data suggest that the scales have adequate validity and reliability for testing the structural model.

Table 5.7: Descriptive Statistics

Construct	Rater	Item	N	Mean	Std. Dev.
Deep structure usage	Self-report[1]	DS1	171	6.11	1.73
		DS2	171	6.08	1.68
		DS3	171	6.09	1.59
		DS4	171	6.98	1.56
	Observer[2]	DI1	45	4.40	1.12
		DI2	45	4.31	1.82
Cognitive absorption	Self-report[1]	CS1	171	5.96	1.89
		CS2	171	5.78	1.80
		CS3	171	5.94	1.65
		CS4	171	5.73	1.68
	Observer	CI1 [2]	45	4.15	0.89
		CI2 [3]	40	55.88	11.15
Performance	Self-report	PS1 [3]	166	79.34	19.75
		PS2 [2]	171	6.40	2.30
		PS3 [2]	171	5.89	1.97
	Observer[3]	PI1 [3]	166	81.01	15.87

Key:
[1] Use a 1-9 Likert scale.
[2] Use a 1-7 Likert scale.
[3] Use a 0-100% scale.

Table 5.8: Instrument Validity and Reliability for Reflective Scales

	Item	DS	CS	Reliability[2]
Loadings and Cross-loadings[1]	DS4	**0.86**	0.53	DS: Cron. α = 0.82 CR = 0.70
	DS3	**0.82**	0.31	
	DS2	**0.81**	0.39	
	DS5	**0.73**	0.38	
	CS4	0.45	**0.84**	CS: Cron. α = 0.81 CR = 0.69
	CS2	0.36	**0.81**	
	CS5	0.36	**0.81**	
	CS1	0.45	**0.73**	
Correlations and AVE[3]	DS	**0.80**	-	
	CS	0.51	**0.81**	

Key:

[1] Obtained from PLS. All loadings are significant (p<.05).

[2] Cronbach's alpha obtained from SPSS, CR obtained from PLS.

[3] The values on the diagonal are the square root of each construct's AVE (Average Variance Extracted) and should be higher than .50.

Table 5.9: Weights for Formative Scales

Construct	Rater	Item	Weight
Deep structure usage	Observer	DI1	.995
		DI2	.841
Cognitive absorption	Observer	CI1	.995
		CI2	.715
Performance	Self-report	PS	.940
		PS2	.954
		PS3	.811

The results for the hypotheses are shown in Figure 5.9 and Tables 5.10-11. The theoretical model appears to have a high level of explanatory power, given the high R^2s and small sample size (n = 45). Table 5.10 provides support for H1, as all the items that shared a common method load significantly on the common method factor. Table 5.11 provides further support for H1, as the estimated relationships between constructs increased when methods were shared (supporting H1A-B). Table 5.11 suggests that the impact of distance bias depends on the construct being considered. Specifically, distance bias appears to significantly affect the relationship between deep structure usage and performance (supporting H2A-B), but not the relationship between cognitive absorption and performance (not supporting H2C-D).

Summary of Results. The results demonstrate that representation bias and distance bias can significantly influence estimated relationships among constructs and can strongly influence a study's conclusions, e.g., determining whether or not an IV is considered to be a significant predictor of a DV (as Figure 5. 9 shows) and creating changes of up to 20% in explanations of a

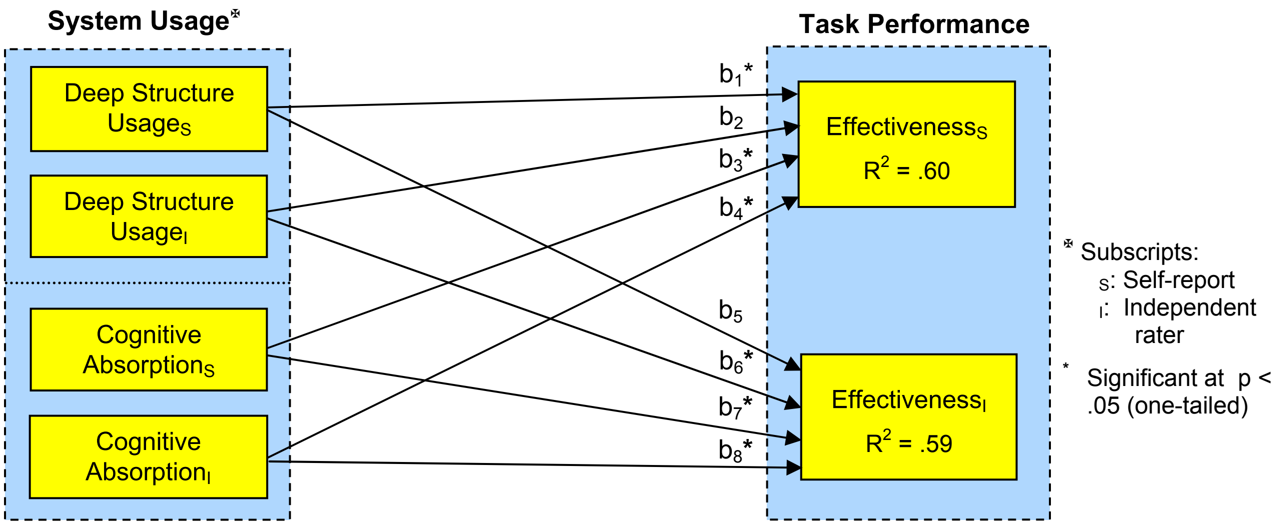


Figure 5.9: Results of Testing the Operationalized Theoretical Model

Table 5.10: Results of Hypothesis 1: Representation Bias

	Items											Hypothesis Supported?
	DS1	DS2	DS3	DS4	CS1	CS2	CS3	CS4	PS1	PS2	PS3	
Loadings**	.70*	.69*	.73*	.53*	.76*	.60*	.55*	.51*	.81*	.82*	.75*	Yes

** Loadings on the common method factor; * Significant at p < .05 one tailed.

Table 5.11: Results for Hypothesis 1 and 2: Representation Bias and Distance Bias

Hypothesis				Results							Hypothesis Supported?
	b_{X1}		b_{X2}	b_{X1}	(SE_{X1})		b_{X2}	(SE_{X2})	t	R^2 diff[2]	
H1A:[1]	b1	>	b5	.30	(.16)	>	0.14	(.14)	4.92*	.13	Yes
H1B:[1]	b3	>	b7	.32	(.15)	>	0.23	(.13)	3.03*	.07	Yes
H2A:	b5	≠	b6	.14	(.14)	≠	0.38	(.11)	-9.06*	.20	Yes
H2B:	b2	≠	b6	.17	(.16)	≠	0.38	(.11)	-7.21*	.18	Yes
H2C:	b7	≠	b8	.23	(.13)	≠	0.27	(.11)	-1.43	.09[3]	No
H2D:	b4	≠	b8	.26	(.15)	≠	0.27	(.11)	-0.21	.04	No

[1] The pattern of results for H1A and H1B are the same in the n= 45 and n = 171 samples. The results for the smaller sample are shown here to be consistent with the other tests in this table.
[2] R^2 diff.: the difference between the R^2 of each path when each path is run on its own.
[3] These paths have significantly different betas when run singly, but not when run in a full model.
* Significant at p < .05. The t-test is a two independent samples t-test; the formula is $(b_1 - b_2)/\sqrt{(s_1^2/n_1)+(s_2^2/n_2)}$.

DV (as Table 5.11 shows). Overall, the results strongly support the proposed approach because:

- The approach suggested that the theoretical model should be tested with the *multi-view* strategy because none of the constructs were defined in terms of specific raters, and
- By following the *multi-view* strategy, we found that the choice of raters can have a substantial effect on the results of testing the theoretical model.

It is important to note that although the multi-view strategy tests for differences in results due to distance bias, it does not place a value judgment on any specific difference. For example, we found that the distance biases in measures of deep structure usage and performance led to differences in the estimated relationships between these two constructs (H2A-B). However, we do not conclude that the methods that led to stronger relationships are necessarily 'better.' Rather, we conclude that the different methods may be measuring different traits, and that this difference is an important finding that requires further theoretical and empirical investigation.

Similarly, we found that differences in distance bias between different measures of cognitive absorption and performance did *not* appear to lead to differences in the estimated relationships between these constructs (H2C-D). Again, this apparent lack of difference is an important result. It may indicate that observers are able to accurately infer user cognitions despite their obvious lack of access to the cognitions. Alternatively, it may indicate that individuals' knowledge of their own cognitions is so limited that their reports of cognitive absorption are no better than observer estimates (per Wilson and Dunn 2004). Further theoretical and empirical investigation is needed to test whether either of these is the case. Nevertheless, the identification of these research opportunities is a direct and useful result of following the multiview strategy.

5.6 Discussion

This study presents a comprehensive approach for dealing with method variance in IS research. It contributes in four ways, summarized in Table 5.12, which we discuss in turn below.

The core contribution of the study is that it provides the first comprehensive approach for dealing with method variance in IS research. Despite the long-standing significance of method variance, there remains no accepted approach for dealing with it (Sechrest et al. 2000). In past

Table 5.12: Research Contributions and Implications

Element of Research	Contribution
Staged Approach	Provides the first comprehensive approach for dealing with method variance in IS research
- Selection Stage	Enables researchers to choose a strategy that is consistent with the research question, theory, and methodological constraints in a given study.
- Execution Stage	Enables researchers to deal with method variance in a manner that is consistent with their study and supports the development of cumulative progress over multiple studies.
Demonstration Exercise of the Staged Approach	Provides indicative evidence that the staged approach is feasible, demonstrates the magnitude of method effects when measuring system usage, and reveals new opportunities for future research in an important, practical domain.

research, a major impediment to developing such an approach was the conceptual complexity of method variance (Fiske and Campbell 1992; Sechrest et al. 2000). By decomposing "method" into its components and "method variance" into its sources, we were able to overcome this problem and build a comprehensive, yet parsimonious approach, consisting of two strategies, three criteria for selecting a strategy, and three areas of guidance for executing each strategy.

Because no comprehensive approach for dealing with method variance has appeared in the methodological literature, it is not surprising that IS researchers rarely deal with it, and when they do, do so only partially (Woszczynski and Whitman 2004). Nevertheless, the situation is alarming because method variance can undermine theoretical progress (by leading researchers to accept poor theories and reject good theories due to biased data), can lead researchers to make inappropriate recommendations for practice (by basing recommendations on biased data), and because partial approaches for dealing with method variance can exacerbate its problems while creating a false sense of security in the validity of results. The proposed approach offers a way to overcome these problems.

Each stage of the proposed approach offers additional contributions. For example, the selection stage contributes by providing researchers with a parsimonious set of principles for selecting a strategy for dealing with method variance that is consistent with the aim and context of a given study. The criteria we propose for selecting a strategy (i.e., nature of inquiry, nature of theory, and nature of methods) are broad enough to support any type of positivist IS research. However, in past research, these criteria have very rarely been used explicitly to justify the selection of methods in positivist IS research. For example, as the Appendix shows, most studies of system usage have focused on its objective aspects and have obtained measures via self-report questionnaires. In many of these studies, we believe that the most likely reason researchers chose to use the self-report method is because of its use in past research (and perhaps its convenience) rather than because of its appropriateness for the study's theoretical context. By encouraging researchers to justify their selection of methods in terms of study research question and theoretical context, the proposed approach should help improve theoretical tests and improve theoretical progress, e.g., by providing a basis for designing research programs and conducting meta-analyses.

The execution stage contributes by providing detailed guidance for carrying out each strategy. Of the two strategies, the mono-view strategy is perhaps the more traditional. This strategy considers method variance to be an unwanted source of error that should be eliminated. Even though this view is common (Webb et al. 2000), there has never been any comprehensive approach for addressing method variance in this context, except for Campbell and Fiske's (1959) suggestion to use multiple independent methods. The lack of detailed guidance in past research has led to the use of partial approaches for dealing with method variance, e.g., the belief that it can be addressed by using a different method for the IV and DV, or by using 'objective' rather than self-reported methods. *None of these techniques fully addresses method variance*. The guidance offered in our mono-view strategy should assist researchers deal with method variance more completely.

The multi-view strategy is novel. However, because it is the less restrictive of the two strategies, it is likely to be the appropriate strategy in more cases than the mono-view strategy.

The multiview strategy enables researchers to extend theory by examining the potential importance of distance bias in a study. Many constructs in IS (e.g., IS spirit, IS security effectiveness, IS success, IS use) are not explicitly defined in terms of one class of observers and yet could potentially be perceived differently by different classes of observers. Despite this, there has been little systematic research in IS to test the existence or effects of these differences. For example, in studies of system usage, theories of technology acceptance (e.g., TAM, Davis 1989) have propelled a great deal of research predicting when and how much people will use a new IS. Even though these theories (e.g., TAM) did not define usage in terms of specific raters, most researchers have measured system usage from the perspective of end-users (i.e., via self-reports, per the Appendix). This led to rapid progress in the development of technology acceptance theories, but at the cost of a narrow view of the dependent variable. Despite some researchers' efforts to test whether these theories are robust to representation bias (e.g., common method bias, Venkatesh et al. 2003), there has been no systematic attempt to examine the implications of distance bias on system usage, e.g., by developing theories to explain differences in views of usage held by different classes of raters. The proposed approach provides a way to systematically investigate these issues and, by so doing, enrich theories of technology acceptance. To be sure, uncovering the local meanings of constructs in this way has long been a key focus of interpretive research (Lee 1991), but we believe that positivist research can also contribute by systematically identifying relevant classes of observers and empirically testing whether their different viewpoints are significant in practice.

A final contribution of our research is that the demonstration exercise provides evidence that the approach is feasible and beneficial in an important domain: studies of system usage and task performance. The demonstration also reveals new research opportunities. For example, it appears that ratings of ontologically objective aspects of usage may be very sensitive to the choice of rater but that observations of ontologically subjective aspects of usage may not suffer as great a distance bias as initially expected. While the results require further investigation, they underscore that we are still at an early stage of understanding how best to measure system usage.

When assessing the contributions of the demonstration exercise, it is important to recognize its limitations. In terms of construct validity, future researchers could examine whether our results hold if different elements of the multi-view strategy had been implemented (e.g., if different choices of raters, instruments, or procedures had been made). In terms of internal validity, future tests of the approach could develop ways to more precisely separate the effects of representation bias and distance bias and to investigate their individual and joint impacts. The explained variance of our tests indicated adequate statistical conclusion validity, but we acknowledge that the same size was small and that a larger sample would allow a more powerful test. Finally, in terms of external validity, the empirical study comprised just one experiment; further replications and extensions in the field would, of course, be valuable.

5.7 Conclusion

To overcome the lack of an explicit approach for selecting methods in IS research, the present study has clarified the meaning of method variance and advanced the first comprehensive approach for dealing with it in IS research. The approach consists of two stages. The first stage, selection, recommends that researchers consider the nature of inquiry, nature of theory, and nature of methods in a given study to select an appropriate strategy for a study's context. The second stage, execution, provides guidance for addressing method variance in a study by choosing appropriate raters, instruments, procedures, and data analysis techniques.

In the present study, we demonstrated how the proposed approach would work through an empirical investigation in which we examined the degree to which method effects influence estimates of the relationship between individual system usage and task performance. The results strongly support the proposed approach and indicate that simplistic choices of methods can lead to significantly biased explanations of performance and mask important research opportunities.

Despite acknowledged limitations, we believe the staged approach advanced in this paper helps to clarify the meaning of method variance and the range of techniques that can be used to address it. Given the importance of research methods in positivist IS research, our focused examination of the problem should enable researchers to obtain more meaningful findings, improve cumulative progress, and lead to more informed use of IS research results in practice.

Appendix 5A: Measures and Methods for Measuring System Usage in Past Research

Study	Measure of System Usage (Number of measures, description)	Ontological Nature of Trait	Rater	Instrument
(Benbasat and Schroeder 1977)	1, Reports requested	Objective	Computer	Computer logs
(Lucas 1978b)	3, Use or non-use, direct or indirect access, and extent of use (number of searches, displays, printed reports)	Mainly objective (some omnibus)	Self-report, Computer	Questionnaire, Computer logs
(Ginzberg 1981)	3, Avg. connect time, avg. number of sessions, avg. number of functions executed per month.	Objective	Computer	Computer logs
(Alavi and Henderson 1981)	1, Use or non-use	Omnibus	Self-report, Computer	Questionnaire, Computer logs
(Benbasat et al. 1981)	3, Frequency of use, use of system defaults, use of particular commands	Objective	Computer	Computer logs
(Culnan 1983)	2, Avg. number of searches, frequency of use	Objective	Self-report	Questionnaire
(Barki and Huff 1985)	1, % time used for decision making	Objective	Self-report	Questionnaire
(Mahmood and Medewitz 1985)	1, Extent of use	Omnibus	Self-report	Questionnaire
(Srinivasan 1985)	4, Frequency of use, duration of use, extent (light/heavy), number of reports	Mainly objective (some omnibus)	Self-report	Questionnaire
(Snitkin and King 1986)	1, Hours used per week	Objective	Self-report	Questionnaire
(Green and Hughes 1986)	1, Number of features used	Objective	Self-report	Questionnaire
(Kim and Lee 1986)	2, Frequency of use, voluntariness	Objective, subjective	Self-report	Questionnaire
(Bergeron 1986)	1, Extent of use of information from system	Objective	Self-report	Questionnaire
(Nelson and Cheney 1987)	1, Extent of use for decision making	Objective and subjective	Self-report	Questionnaire
(Hogue 1987)	1, Frequency of use of information from IS	Objective	Self-report	Questionnaire
(Swanson 1987)	1, Frequency of use of information from IS	Objective	Self-report	Questionnaire
(Davis 1989)	1, Frequency of use, prediction of future use	Objective	Self-report	Questionnaire
(Davis et al. 1989)	1, Frequency of use	Objective	Self-report	Questionnaire
(Thompson et al. 1991)	3, Duration of use, Frequency, Number of packages used	Objective	Self-report	Questionnaire
(Moore and Benbasat 1991)	1, Use or non-use	Omnibus	Self-report	Questionnaire
(Nance 1992)	2, General versus specific use, Appropriateness of use	Objective	Self-report	Questionnaire
(Adams et al. 1992)	2, Study 1: number of messages sent/ received; Study 2: frequency, duration used)	Objective	Self-report	Questionnaire
(Szajna 1993)	2, Use of specific or general reports, duration	Objective	Self-report	Questionnaire
(Hartwick and Barki 1994)	2, Frequency, extent (light, med., heavy)	Objective, omnibus	Self-report	Questionnaire

Study	Measure of System Usage (Number of measures, description)	Ontological Nature of Trait	Rater	Instrument
(DeSanctis and Poole 1994)	2, Faithfulness of use, attitude during use	Subjective	Researchers, audio records	Coding scheme for observations
(Keil et al. 1995)	1, % of time that use system for task	Objective	Self-report	Questionnaire
(Taylor and Todd 1995)	3, Number of times used, time spent, number of tasks used for	Objective	Self-report	Questionnaire
(Igbaria et al. 1995)	4, Number of applications used, number of supported tasks, duration, frequency	Objective	Self-report	Questionnaire
(Goodhue and Thompson 1995)	1, Dependence	Subjective	Self-report	Questionnaire
(Straub et al. 1995)	4, Number of messages, number of hours, extent (light, med., heavy), interactivity	Objective, Omnibus	Self-report, Computer	Questionnaire, Computer logs
(Szajna 1996)	2, Number of messages sent, frequency of sending	Objective	Self-report, Computer	Questionnaire, Computer logs
(Igbaria et al. 1996)	2, Duration of use, frequency	Objective	Self-report	Questionnaire
(Igbaria et al. 1997)	4, Number of packages used, number of supported tasks, duration, frequency	Objective	Self-report	Questionnaire
(Webster 1998)	1, Type of use (complete, wary, or none)	Subjective	Researcher, Self-report	Coding schemes for observations and interviews
(Gelderman 1998)	3, Method of access, hours per week, frequency of use	Objective	Self-report	Questionnaire
(Yuthas and Young 1998)	2, Number of reports accessed, duration of use	Objective	Computer	Computer logs
(Lucas and Spitler 1999)	1, Extent (light, heavy) for different applications	Omnibus	Self-report	Questionnaire
(Dishaw and Strong 1999)	1, Frequency	Objective	Self-report	Questionnaire
(Compeau et al. 1999)	2, Frequency, duration	Objective	Self-report	Questionnaire
(Karahanna and Straub 1999)	1, Number of messages sent	Objective	Self-report	Questionnaire
(Venkatesh and Davis 2000)	1, Duration of use per day	Objective	Self-report	Questionnaire
(Jarvenpaa and Staples 2000)	3, Frequency of use, use of features to search, use of features to store/publish	Objective	Self-report	Questionnaire
(Mathieson et al. 2001)	2, Frequency, duration	Objective	Self-report	Questionnaire
(Rai et al. 2002)	1, Dependence	Subjective	Self-report	Questionnaire
(Venkatesh et al. 2003)	1, Duration of use	Objective	Computer	Computer logs
(van der Heijden 2003)	2, Frequency of use, intensiveness of browsing (number of pages)	Objective	Self-report	Questionnaire
(Vlahos et al. 2004)	3, Duration of use of hardware, software, and IT in general (over a week)	Objective	Self-report	Questionnaire
(Wu and Wang 2005)	1, Frequency of use	Objective	Self-report	Questionnaire

Chapter 6

Conclusion

6.1 Reprise: Objective

Motivated by the importance of system usage in practice and the need for a deeper understanding of the system usage construct in IS research, this thesis set out to address the following research question:

What principles can be used to conceptualize and measure system usage in an appropriate way for a given theoretical context?

To answer this question, I drew upon critical realist assumptions and studies of research diversity to develop a framework to clarify key epistemological factors that enable the system usage construct to have diverse meanings (i.e., definitions, theories, and methods) and ontological factors that constrain the potential meanings of the system usage construct (i.e., elements, properties, and values). I then proposed an approach for conceptualizing and measuring the system usage construct that accounted for these epistemological and ontological factors. The approach consists of four steps—*definition, structure, function, and method*—with each step comprising principles and guidance to enable researchers to conceptualize and measure system usage in an appropriate way for a given theoretical context.

To determine whether the approach and its underlying principles were useful, I carried out three empirical tests to examine the feasibility of the approach and to judge whether measures of system usage that were selected according to the approach enabled better explanations of the relationship between system usage and downstream outcomes than other measures of system usage. The results for these tests are detailed in Chapters 2, 4, and 5.

6.2 Contributions

The dissertation makes several contributions to research and practice. These contributions were highlighted in each chapter of the thesis. Table 6.1 presents a summary of these contributions and I discuss them briefly in turn.

Table 6.1: Contributions of the Thesis

Target of Contribution	Contribution
Contributions to research	Provides an explicit set of steps and principles that researchers can use to select or evaluate measures of system usage for a given theoretical context. Instantiates a new approach for conceptualizing and measuring constructs in IS research that is consistent with critical realist assumptions. Demonstrates the usefulness of the proposed approach by empirically identifying the degree to which explanations of theoretical models can be improved by: - Selecting elements, properties, and measures of system usage that are appropriate for a theoretical context - Selecting methods for measuring system usage that are appropriate for the nature of a study's inquiry, theory, and practical constraints Provides validated measures of individual and collective usage for a specific theoretical context.
Contributions to practice	Provides an approach that practitioners can tailor to select metrics of system usage that enable them to explain how systems are used and explain how system usage is associated with downstream outcomes in practice.

6.2.1 Contributions to Research

The central contribution of the thesis is that it offers a comprehensive approach that researchers can use to select measures of system usage that are appropriate for a given theoretical context. This has two benefits. First, at the level of a single study, the approach can help a researcher select measures of system usage that will enable him/her to improve explanations of the relationship between system usage and other constructs in his/her theoretical model. Second, at the level of the entire community of researchers who study system usage, the approach provides a way to improve the discipline of creating system usage measures while facilitating the generation of diverse measures appropriate for different research contexts. In other words, the approach provides a way to improve the *disciplined diversity* of system usage measures in IS research. Disciplined diversity has been argued to be an important goal in IS research (Benbasat and Weber 1996; Landry and Banville 1992; Robey 1996; Weber 2003a), but there are no systematic approaches for conceptualizing or measuring constructs in IS research that enable researchers to achieve it. The proposed approach provides one way to do so.

Each step of the approach offers additional contributions. These contributions are outlined in depth in each chapter, but briefly summarized here. The *definition* step enables researchers to clarify what system usage is and how it differs from conceptually related constructs (e.g., information usage, adoption, acceptance, and so on). By encouraging researchers to clarify their definitions and to explicate the assumptions underlying their definitions, this step should help researchers ensure that they are studying the phenomena they intended to study.

The *structure* step illuminates the elements of system usage and helps researchers select relevant elements for a given context. One simple benefit of this step is that it highlights elements of usage that have received insufficient research attention. For example, many researchers have ignored the "user" element of system usage, such as user cognitions-in-use. Including such elements in measures of system usage not only leads to a more natural conception of how people use systems but can also improve the degree to which system usage measures can explain downstream outcomes (see Chapter 2). Similarly, as Chapter 3 highlighted, past research has ignored the differences between individual system usage and collective system usage. By highlighting the importance of interdependencies-in-use, this step will help researchers determine when collective system usage exists and how it differs from individual system usage.

The *function* step helps researchers select measures of system usage that are appropriate for a given theoretical context, thereby avoiding errors of omission and inclusion in measurement. The function step also highlights the importance of the distinction between shared and configural conceptions of collective system usage. Configural measures of collective system usage have never been used in IS research, but there are many reasons to suggest that collective system usage will often be configural in practice.

The *method* step helps researchers account for method variance when obtaining measures of system usage, thereby ensuring that their measures of system usage are both more accurate and more appropriate for a study's context. The method step especially highlights the relevance of distance bias for many constructs in IS research and provides a framework to consider the different types of properties that distance bias can reveal and the situations in which distance bias is likely to be most relevant for a study (see Chapter 5).

A more general contribution of the approach is that it instantiates a new way to develop and study constructs in IS research. As argued in Chapter 1, many "working positivists" in IS implicitly use critical realist assumptions. Nevertheless, I agree with Mingers (2004b) that the full implications of a critical realist position have not been widely acknowledged or discussed. The proposed approach provides a systematic way to conceptualize and measure constructs from a critical realist position.

In addition to the contributions that stem from the approach, the empirical tests offer additional contributions. These tests contribute by: (1) demonstrating that the approach is feasible in practice, (2) demonstrating that the principles underlying the approach are important empirically (not just theoretically), (3) revealing new findings regarding the relationship between system usage and performance at the individual level of analysis, collective level of analysis, and across levels of analysis, and (4) by providing validated measures of system usage that future researchers can employ.

6.2.2 Contributions to Practice

Even though the focus of this dissertation is a research "construct," this does not limit the contributions to research. Many practitioners also create constructs in the guise of "metrics" that they use to make decisions, test lay theories, and monitor resources (Kaplan and Norton 1992). In IS practice, there has long been a lack of good metrics (Strassman et al. 1988). According to Chidambaram et al. (2005), one of the underlying problems "is the ad hoc manner in which IT metrics are often selected, devised, and used" (p. 4).

Given the importance of system usage as a necessary link between IS investments and IS outcomes, it might be expected that organizations might employ advanced techniques for choosing and assessing metrics for system usage in practice. This is not the case. Surprisingly, in their review of IT metrics in practice, Chidambaram et al. (2005) found 31 widely-used metrics, of which only two related to system usage. These two metrics of system usage were measures of duration: CPU hours utilized and hours logged per employee. The approach proposed in this thesis (and the empirical results in Chapter 2) would suggest that such metrics will not provide very meaningful insights into how usage of an organization's information systems lead to important outcomes such as employee, workgroup, or organizational performance.

I suggest, therefore, that the approach proposed in this thesis can contribute to practice by helping organizations select metrics of system usage that can help explain relevant organizational outcomes, such as individual or collective performance, staff satisfaction, quality of work life, and so on. The approach would certainly need to be tailored to cater for the practical realities of organizations. Nevertheless, I suggest that much of the approach and its principles would stay the same. Organizations still need to define the metrics they wish to measure, still need to select the elements and properties of system usage that are relevant for explaining the desired outcomes, still need to ensure that their metrics accurately reflect the chosen properties, and still need to determine what methods to use for obtaining the metrics. The goals of the approach would also likely remain unchanged because practitioners, just like researchers, can benefit from a better understanding of how systems are actually used in practice (i.e., the ontological imperative) as well as how system usage leads to relevant outcomes (i.e., the epistemological imperative).

6.3 Limitations and Future Research

Like all research, this thesis could be improved and extended. Table 6.2 summarizes its limitations and describes avenues for future research that could address these limitations.

6.4 Conclusion

Motivated by the importance of understanding system usage in practice and the lack of explicit conceptualizations of system usage in past research, this thesis advanced a comprehensive approach for conceptualizing and measuring system usage in an appropriate manner for a given theoretical context. Three empirical tests were carried out to test the approach. The empirical tests demonstrated that the approach is feasible and can help researchers reach better explanations of the relationship between system usage and downstream outcomes. Overall, the thesis contributes by (1) providing an explicit and systematic way to improve the discipline and diversity of measures of system usage in IS research, (2) instantiating a new approach for construct development and investigation, (3) providing new empirical findings and validated measures, (4) providing practitioners with a starting point for selecting metrics of system usage in practice, and (5) by illuminating many new directions for research on the nature of system usage, its antecedents, and its consequences.

Table 6.2: Limitations and Future Research Opportunities

Component of Thesis	Limitations	Future Research Opportunities
Entire approach	Constrained assumptions	Could conduct investigations of system usage from other meta-theoretical assumptions, research targets, or forms of theory. Could conduct a multi- or meta-paradigmatic inquiry on the nature of system usage. Could extend the conceptual basis of the work by deepening the use of critical realist assumptions.
	Limited application (only applied to system usage)	Could tailor the approach to apply to other constructs in IS research, whether those with similar definitions to system usage (e.g., information usage) or distinctly different constructs (e.g., effective IS security, project escalation, and so on).
- Definition step	Limited application (only one definition provided)	Could explicate other definitions of system usage with different assumptions and clarify situations in which one definition may be more acceptable than another (e.g., depending on the theoretical context).
- Structure step	Limited application (based on one definition)	Could examine whether different definitions of system usage imply different elements rather than, or in addition to, the user, system, and task.
	Limited scope (limited to one system)	Could investigate the differences between individual or collective use of *one* system versus individual or collective use of a *portfolio* of systems.
	Limited investigation (of collective system usage)	Could investigate how strong interdependencies have to be before collective usage can be said to exist. Could develop principles for determining to what extent every member of a collective must use a system for collective usage to exist. Could investigate the differences between multilevel and social network conceptions of structure.
- Function step	Limited scope (only one theoretical context)	Could determine whether the approach assists researchers in other single-level or multilevel theoretical contexts.
	Limited scope (only dependent variable)	Could examine whether the approach enables researchers to choose more effective measures of system usage when it is an independent or mediating variable rather than a dependent variable

Component of Thesis	Limitations	Future Research Opportunities
	Limited investigation (did not examine conflicting outcomes)	Could examine models in which there are conflicting outcomes (e.g., organizational versus individual goals; performance goals versus quality of life goals).
	Limited validation (little guidance for collective-level validation)	Could improve techniques for determining when measures of collective system usage can be deemed to have sufficient construct validity.
- Method step	Limited scope (only examined distance and representation bias)	Could attempt to determine whether there are other general sources of method variance in addition to distance bias and representation bias.
	Limited investigation (of distance bias)	Could extend the framework of types of distance bias in Chapter 5 (Figure 5.3) to identify other general types. Could categorize different constructs in IS research according to the importance of distance bias when studying them.
Empirical tests	Limited scope (demonstrations only)	Could use the steps and principles of the approach (e.g., the categorization of different elements of use) to provide a structure for a meta-analysis of the system usage literature.
	Internal validity	Could include other antecedents, moderating variables, or mediating variables in the theoretical models to determine if system usage still has the same effect on performance. Could use a factorial experimental design to improve the ability to rule out correlated omitted variables when testing models of system usage and when testing the relative effects of distance bias and representation bias.
	Construct validity	Could investigate whether the measurement models for system usage in the thesis exclude important properties of individual or collective system usage. Could investigate whether the unexpected lack of effect of collective collaboration-in-use on individual deep structure usage in Chapter 4 were due to measurement problems.
	Statistical conclusion validity	Could increase the sample size used in each empirical test. Could replicate the hierarchical linear modeling results with multilevel structural equation modeling techniques that account for measurement error (e.g., MPLUS).
	External validity	Could replicate the empirical tests in other theoretical contexts or methodological (e.g., field or survey) contexts.

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Education

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1997 – 1998	December, 1998, Tutorial Assistant, Department of Commerce, University of Queensland
1999 – 2001	Consultant and Senior Consultant, PriceWaterhouseCoopers, Brisbane, Australia
2001 – 2005	Graduate Research Assistant to Dr. Peter Meso, Dr. Jeff Hubona, and Dr. Detmar Straub, Department of Computer Information Systems, Georgia State University

Journal Publications

1. Burton-Jones, A. and Hubona, G.S. "Individual Differences and Usage Behavior: Revisiting a Technology Acceptance Model Assumption," *The DATABASE for Advances in Information Systems*, (36:2), 2005, pp. 58-77.
2. Burton-Jones, A., Storey, V.C., and Sugumaran, V., and Ahluwalia, P. "A Semiotic Metrics Suite for Assessing the Quality of Ontologies," *Data and Knowledge Engineering,* (In press), 2005.

Conference Publications

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3. Burton-Jones, A., Purao, S., and Storey, V.C. "Context-Aware Query Processing on the Semantic Web," in *Proceedings of the Twenty-third International Conference on Information Systems (ICIS),* L. Applegate, R. Galliers and J. I. DeGross (eds.), Barcelona, Spain, Dec. 16-19, 2002, pp. 195-208.
4. Hubona, G.S., and Burton-Jones, A. "Modeling the User Acceptance of E-Mail," in *Proceedings of the Thirty-sixth Annual Hawaii International Conference on System Sciences (HICSS)*, 2003.

5. Burton-Jones, A., Storey, V.C., Sugumaran, V., and Ahluwalia, P. "Assessing the Effectiveness of the DAML Ontologies for the Semantic Web," in *Proceedings of the Eighth International Conference on Applications of Natural Language to Information Systems (NLDB)*, A. Düsterhöft and B. Thalheim (eds.), Burg (Spreewald), Germany, 2003, pp. 56-69.
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9. Storey, V.C., Sugumaran, V., and Burton-Jones, A., "The Role of User Profiles in Context-Aware Query Processing for the Semantic Web," in *Proceedings of the Ninth International Conference on Applications of Natural Language to Information Systems (NLDB)*, F. Meziane and E. Métais (eds.), Salford, UK, June 23-25, 2004, pp. 51-63.

Awards

1. Accepted to the Doctoral Consortium of the International Conference on Information Systems, 2004
2. Best Research Paper Award, International Conference on Information Systems, Barcelona, 2002
3. Phi Beta Kappa, National Honor Society, Georgia State University, 2002-2003
4. Thomas Brown and Son's Memorial Thesis Prize for best Honors Thesis, Department of Commerce, University of Queensland, 1998
5. Dean's List, Department of Commerce, University of Queensland, 1997-1998
6. Butterworths Book Prize, Department of Commerce, University of Queensland, 1997
7. Shell Prize in Accounting, Department of Commerce, University of Queensland, 1997

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Journals: *Management Science*, *MIS Quarterly, Information Systems Research, Information Systems Journal, Requirements Engineering Journal, Journal of Strategic Information Systems, Data & Knowledge Engineering, Information Technology and Management, Journal of Information Technology, International Journal of Accounting Information Systems, and Journal of Database Management.*

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