

Linking Relevance to Practical Significance

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Abstract

Researchers in academic disciplines, including but not limited to information systems, have long been aware of, but have not linked, two research issues: one issue is the lack of relevance, despite the plethora of rigor, in their research; the other issue is the distinction between statistical significance and practical significance, where the latter is no less important than the former. In this essay, we link the two issues by examining and revealing the practical significance of the research reported in a well known, published article and stating the questions that this examination raises.

1. Introduction

The purpose of this essay is to link two issues that researchers already consider to be important: relevance and practical significance. Researchers have long been aware of both issues, but have not linked them.

Much, if not most, academic business-school research tends to produce theory that rightfully deserves accolades for being methodologically rigorous, but the applicability of such theory in the everyday practice of business has not been readily demonstrable or evident. The academic discipline of information systems, which refers to such applicability under the heading of “relevance,” is well aware of this issue [1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, 15, 16].

At the same time, much of the rigor in the theory produced in information systems (and other business-school disciplines) is based on the concept of statistical significance – a venerable methodological concept whose importance is indisputable, but which is different from practical significance. Statisticians and even elementary statistics textbooks take pains to distinguish between statistical significance (which researchers often use to describe the quality of a hypothesis about whether or not two factors are

related) and practical significance (which, in this essay, refers to the measurable amount that a change in an independent or moderating variable brings about in a dependent variable). Academic researchers are aware of the distinction, yet in their published statistical research, they routinely address statistical significance and not, if at all, practical significance.

This distinction between statistical significance and practical significance is not merely academic, as it were. One pair of factors could be related to each other at a better level of statistical significance than are another pair of factors, but the practical significance, as just described, could be greater in the situation for the latter pair than the former pair. The idea in the preceding sentence is not new, but it has obvious ramifications for addressing the issue of relevance in statistically conducted research. Published statistical research articles in information systems and other business-school disciplines can put this idea to good use by reporting not only the statistical significance of their analyses (as has traditionally been done), but also their practical significance. It could be the case that some statistically conducted research is or harbors great potential to be relevant to practice, but this has not been known because, at least in part, the practical significance was not reported. It could also be the case that some statistically conducted research, despite the excellent levels of statistical significance that it reports, bears little practical significance and, hence, little relevance.

After this introductory section of the essay, we proceed, in the second section, to examine the practical significance of relationships between factors in the well cited study, “User Acceptance of Information Technology: Toward a Unified View” (Venkatesh, Morris, Davis & Davis, 2003), and also to state questions pertaining to statistically conducted research that emerge in general from our examination of the specific case of Venkatesh *et al.* In the essay’s third and final section, we propose how future studies

may give appropriate treatment to relevance and practical significance in statistically conducted research.

2. A case for studying relevance and practical significance

The 2003 article by Venkatesh *et al.* [17] has been cited over 1,100 times according to the ISI Web of Knowledge and over 3,500 times according to Google Scholar. It is a seminal study in the stream of information-systems research in the area of technology acceptance. In this essay, we will be bringing attention to what their article, representing excellent information-systems research in general, has not addressed regarding relevance and practical significance. This is not intended to be, and should not be taken as, a negative criticism of their excellent article. In this essay, we are articulating ideas that were not articulated in 2003; it would not be reasonable to hold the research of Venkatesh *et al.* to a framework, and to any criteria or standards that the framework implies, that did not even exist at the time of their research. Rather, we regard the article by Venkatesh *et al.* as providing a solid foundation upon which to build additional ideas about relevance and practical significance, which we offer in this essay.

The seminal study by Venkatesh *et al.* formulates and validates what it calls a “unified theory of acceptance and use of technology” or UTAUT. It posits two dependent variables: a person’s “behavioral intention” to use a technology and the person’s “use behavior” or actual usage of the technology. The four independent variables are the person’s “performance expectancy” (“the degree to which an individual believes that using the system will help him or her to attain gains in job performance” [17, p. 447]), the person’s “effort expectancy” (“the degree of ease associated with the use of the system” [17, p. 450]), the “social influence” on the person (“the degree to which an individual perceives that important others believe he or she should use the new system” [17, p. 451]), and the “facilitating conditions” experienced by the person (“the degree to which an individual believes that an organizational and technical infrastructure exists to support use of the system” [17, p. 453]). There are, moreover, four interactive or moderating variables: the person’s gender, the person’s age, the person’s experience, and whether the usage of the technology is voluntary or mandatory.

We will not summarize Venkatesh *et al.*’s entire analysis, but instead focus on the following specific finding that we will examine further in this essay’s

next section. Among Venkatesh *et al.*’s conclusions is the finding of statistical significance in the relationship hypothesized to exist between “performance expectancy” and “behavioral intention.” For good reasons (having to do with epistemological difficulties in stating causality, whether in the social or natural sciences), Venkatesh *et al.* appropriately refrain from making the statement that their evidence *proves* that greater “performance expectancy” *causes* or *leads to* greater “behavioral intention”; however, one may still properly make the statement that Venkatesh *et al.*’s evidence *is consistent with* the assertion that greater “performance expectancy” *is associated with* greater “behavioral intention.”

2.1. Descriptions of statistical significance and practical significance

Statistical significance, in this context, means the following. Because the researchers Venkatesh *et al.* collected data from only a sample of individuals in a population, rather than data from all the individuals in the population, there exists the possibility that any subsequent finding of a relationship between “performance expectancy” and “behavioral intention” could be the result of error due to the sample’s only being an approximation, or not being sufficiently representative, of the entire population. Generally, the greater the statistical significance (where greater statistical significance is indicated by a p-value’s having a smaller numerical value), the more confident a researcher can be that the finding of a relationship is not due to the result of sampling error.

However, better statistical significance associated with the relationship between two factors *never* means that the impact or influence of one of the factors (such as “performance expectancy”) on the other factor (“behavioral intention”) is greater than if the statistical significance is worse. Better statistical significance only means that one may place more confidence in the sampling-based decision to reject the null hypothesis, which is the hypothesis that no relationship exists between the two factors; this is distinct from what the size of the relationship (if it exists) is. The size of this relationship is what statistically oriented researchers have long referred to, but not necessarily paid attention to, as *practical* significance.

In their Table 21, Venkatesh *et al.* report the results of their final analysis (i.e., what they call the “cross-validation of UTAUT,” which involves data pooled across three time periods). They mention that the direct effect of “performance expectancy” on “behavioral intention” bears a high level of statistical

significance (specifically, $p < .001$) and they indicate that the numerical value of the standardized coefficient is .53 (where “performance expectancy” is the independent variable and “behavioral intention” is the dependent variable). In general, an estimated coefficient (such as .53) can be regarded as the size of the effect (here, the size of the direct relationship between “performance expectancy” and “behavioral intention”), but in this essay, we define the term **practical significance** to refer to the size of the relationship *as measured in the data’s original metric* (here, both variables are measured on a scale of 1 to 7), not as measured with standardized data as is done in the Venkatesh *et al.* study. Practical significance in this case refers to the following: when “performance expectancy” changes by 1 unit along its scale from 1 to 7, how much is “behavioral intention” expected to change along its own scale, which also happens to go from 1 to 7?

2.2. Practical significance, in the original metric, and the questions that this raises

Where (1) the standard deviation of “performance expectancy” is 1.20 units along a scale from 1 to 7 and the standard deviation of “behavioral intention” is 1.07 units also along a scale from 1 to 7,¹ (2) the standardized coefficient for the effect of “performance expectancy” on “behavioral intention” is 0.53, and (3) there is a change in “performance expectancy” of 1 unit along its scale from 1 to 7, the resulting change in “behavioral intention” along its own scale of 1 to 7 can be calculated to be 0.47. *In other words, based on Venkatesh et al.’s research, we can describe the practical significance that “performance expectancy” has for “behavioral intention” as follows: a person whose “performance expectancy” increases by 1 unit (as measured along a scale that goes from 1 to 7) will experience an increase of 0.47 units in his or her “behavioral intention” to use the system (where the 0.47 units also happen to be measured along a scale that goes from 1 to 7).*

The empirical finding of 0.47 (or, in standardized terms, 0.53) can boast an excellent level of statistical significance (as mentioned, Venkatesh *et al.* found it to be $p < .001$); when it comes to desired levels of statistical significance, a level of $p < .001$ is more

than sufficient. *However, when it comes to desired levels of practical significance, would a manager find an improvement of 0.47 units in the behavioral-intention scale (which goes from 1 to 7), resulting from or at least associated with a change in 1 unit in the performance-expectancy scale (which also goes from 1 to 7) to be sufficient and, therefore, relevant? And even if the manager considers this to be sufficient and therefore relevant, this assumes that the manager already “buys” the proposition that she ought to motivate her employees to accept and use the new technology and that she has the administrative power and the means to carry out an action that will increase an employee’s “performance expectancy.”*

This discussion raises, in turn, other questions. Would the manager even be interested, in the first place, in influencing what the researchers Venkatesh *et al.* are calling a person’s “behavioral intention” to use a technology? Indeed, in the history of technology acceptance research, have practitioners ever been asked this question? From the manager’s perspective, what might be the payoff (in terms of improvements in productivity, revenues, profits, and so forth) associated with an increase of 0.47 units – or, for that matter, an increase of any other amount – in any of her employee’s “behavioral intention” to use the technology that she wants her employees to use? Also from the manager’s perspective, what would be the cost (in dollar terms, in terms of time and effort, in terms of political capital to be expended, and so forth) of increasing an employee’s “performance expectancy” by 1 unit – or, for that matter, an increase of any other amount – which is needed for the associated, desired change in the employee’s “behavioral intention”? Would the manager consider the total payoff to outweigh the total cost? Does the article published by Venkatesh *et al.* provide sufficient information for the manager to be aware of or even formulate these questions? Would the manager even accept the assumption, which is foundational in Venkatesh *et al.’s* research, that it is worthwhile to do something about “behavioral intention” in the first place?

Regarding the last question, Venkatesh *et al.* deserve credit for examining the relationship between a person’s “behavioral intention” to use a technology and the person’s actual “use behavior.” In the immediately preceding discussion, “behavioral intention” played the role of the dependent variable while “performance expectancy” was the independent variable, but here, we now switch “behavioral intention” to the role of independent variable where “use behavior” is the dependent variable. *All the same questions and considerations about relevance*

¹ Venkatesh *et al.* do not provide the standard deviations of “performance expectancy” and “behavioral intention” using data pooled across the three time periods. We obtained the figures of 1.20 and 1.07 units by taking the mean of the three standard deviations for the three time periods (1.20, 1.22, and 1.17 for “performance expectancy” and 1.19, 0.99 and 1.02 for “behavioral intention,” in Venkatesh *et al.’s* Table 19).

and practical significance in the preceding discussion regarding “performance expectancy” and “behavioral intention” therefore can be mapped directly onto “behavioral intention” and “use behavior,” respectively.

In fact, just as we converted the standardized coefficient of 0.53 to its original-metric value of 0.47 where “performance expectancy” was the independent variable and “behavioral intention” was the dependent variable, we can do the same for all other salient independent-variable/dependent-variable pairings in the article by Venkatesh *et al.* Thus, all the same questions and considerations about relevance and practical significance in the preceding discussion can also be mapped directly onto any independent-variable/dependent-variable pair in Venkatesh *et al.*'s article and, indeed, in all information-systems research that uses multivariate analysis such as regression analysis and structural equation modeling.

To summarize, in our linking of relevance to practical significance in the situation involving multivariate analyses and factors that are measured along scales such as 1 to 7, the following questions (stated more generally than above) emerge:

1. Does a manager have any interest, in the first place, in influencing, changing, or otherwise having an impact on the factor that researchers consider to be the dependent variable? Might managers be asked about their interest in this factor, whether prior to or subsequent to a research study that examines this factor as a dependent variable?
2. Does a manager have the administrative power and the means to carry out an action that will change the factor that researchers consider to be the independent variable, where such a change is needed to bring about any desired change in the other factor that researchers consider to be the dependent variable? Might managers be asked about the availability to them of the required administrative power and means, whether prior to or subsequent to a research study that examines this factor as an independent variable?
3. Does a manager find an improvement of x units (such as 0.47 units in our example, above) in one factor (assuming that the manager has an interest in improving this factor), resulting from or at least associated with a change in 1 unit in another factor (assuming that the manager has the administrative power and the means to bring about this change), to be sufficient and therefore relevant?

4. From a manager's perspective, what is the payoff (in terms of improvements in productivity, revenues, profits, and so forth) associated with a unit improvement in the factor that the researchers consider to be the dependent variable?
5. From a manager's perspective, what is the cost (in dollar terms, in terms of time and effort, in terms of political capital to be expended, and so forth) of bringing about a change in the factor that researchers consider to be the independent variable, where the amount of this change is enough for the manager to consider the associated change in the accompanying factor (the factor that researchers consider to be the dependent variable) to be relevant?
6. From a manager's perspective, does the above-described payoff outweigh the above-described cost?
7. Does or should the research article which examines the factors/variables mentioned above provide sufficient information for the manager to answer, to be aware of, or even to formulate the preceding questions?

Worth noting is that the above seven questions all go beyond considerations of statistical significance. Findings of statistical significance certainly deserve to be reported in research published in academic journals. However, for research produced in academic disciplines that also serve professions (such as information systems and other disciplines also found in business schools), relevance and practical significance are no less important qualities for research to have than statistical significance.

At the same time, we acknowledge that, in the situation where the genre or the form taken by academic research is an article published in an academic journal, one may question the desirability of the course of action in which a researcher attempts to address all seven questions in the article that he or she is authoring. We make the case, below, for why such a course of action is not desirable or even feasible, and propose two other courses of action that appear more promising.

3. How future research may give appropriate treatment to relevance and practical significance

We examine three strategies by which future research might proceed to address the concerns embodied in the seven questions: (1) requiring research articles, of which Venkatesh *et al.*'s article is an exemplar, to address relevance and practical significance by answering questions such as the seven raised at the end of the preceding section, (2) developing a new genre for research and publication that would address relevance and practical significance, much as *MISQ Executive* has come to embody a new research-and-publication genre different from that of standard research articles, and (3) studying the matter further, by pursuing theoretically-based and empirically-based positivist research, interpretive research, action research, and design research on how academic-research knowledge is, is not, can be, cannot be, should be, and should not be used by managers, executives, consultants, and other practitioners. We recommend the second and third strategies, but not the first.

As for the first strategy – requiring research articles to address relevance and practical significance – we can take advantage of a lesson learned in the experience of *MIS Quarterly*. In its early years, it assigned five reviewers to every paper submitted for publication consideration: three researchers and two practitioners for papers submitted to the journal's "theory and research" track, and two researchers and three practitioners for papers submitted to the journal's "application" track. Crafting a paper to satisfy both audiences proved, over time, to be seriously problematic. Over time, *MIS Quarterly* came to adopt a basic-research approach and practitioners were no longer routinely assigned, if at all, as reviewers. Whereas crafting an academic research paper to address a practitioner audience, in addition to an academic audience, is a noble goal, the lesson from *MIS Quarterly*'s experience is that the goal's feasibility cannot be taken for granted. Indeed, along these lines, Straub and Ang [15, p. v] state that "any academic journal written by researchers for researchers as the primary audience is simply not targeted for practitioners." They continue: "the apparatus of scientific reasoning is not familiar to them... The entire point of a doctoral degree is to enable readers to follow, to some extent at least, the reasoning of scholarly journals. Doctoral degrees are primarily degrees in research methods and without a thorough infusion of these methods, it is challenging, to say the least, to appreciate the value of a scholarly article." Thus, to pursue the strategy of requiring research articles to address relevance and practical significance can be judged to be difficult, if not outright infeasible.

The second strategy – developing a new research-and-publication genre different from that of standard research articles for the purpose of addressing relevance and practical significance – appears to be more promising, with the success of *MISQ Executive* serving as an "existence proof" that such a strategy is feasible. The genre of *MISQ Executive* itself, however, is not necessarily appropriate, considering that its articles are largely non-statistical. A more appropriate research-and-publication genre could be one similar to that of the teaching cases available from Harvard Business School, Darden School of Business, and Ivey School of Business. Such teaching cases are not necessarily, but can be, used for the purpose conveying academically developed theory (including statistically conducted research) to current and future practitioners, where the case analysis could provide opportunities for the teacher and the students to consider questions such as the seven mentioned at the end of the preceding section of this essay. Of course, the publication of a teaching case does not garner as much credit for a researcher (if indeed any credit is garnered at all) as does the publication of a research article in a research journal; however, the situation for the publication of an article in *MISQ Executive* is similar. Thus, pursuing a new research-and-publication genre for addressing relevance and practical significance is a strategy better pursued by tenured than untenured professors.

There is much to be said in favor of the third strategy, which is to pursue further study. All seven questions listed in the previous section are worthy of empirical investigation. For instance, managers could be interviewed or observed to determine: the interest that they have, or do not have, in the dependent variables used in past information-systems research; the factors that they are interested in influencing, regardless of whether past information-systems research has identified these factors as dependent variables; the administrative power and the means that they do or do not have to influence the factors that past information-systems research has treated as independent variables; whether managers consider, to be relevant, the statistically significant coefficients in past information-systems research; what payoff (in terms of improvements in productivity, revenues, profits, and so forth) they would expect to be associated with a unit improvement in a given factor that past information-systems research has treated as a dependent variable; what cost (in dollar terms, in terms of time and effort, in terms of political capital to be expended, and so forth) they would expect to be associated with bringing about a change in a given factor that past information-systems research has treated as an

independent variable, where the amount of this change is enough for the manager to consider the associated change in the accompanying factor (the factor that researchers consider to be the dependent variable) to be relevant; whether they would expect, for a given independent-variable/dependent-variable pair in a past information-systems research study, the payoff to outweigh the cost; and whether past, statistically conducted information-systems research has provided sufficient information for managers to answer, to be aware of, or even to formulate the preceding questions. *Indeed, how may statistically conducted information-systems research ever achieve true relevance to the world of practice unless these and similar questions are first investigated empirically?* These questions are suitable for investigation by positivist research, interpretive research, action research, and design research.

However, as suggested in this essay's introduction, there is one small but significant step that can now be taken and is compatible with the current genre of academic journal articles. The small but significant step is for published statistical research articles in information systems and other business-school disciplines to report not only the statistical significance of their analyses (as has traditionally been done), but also their practical significance. This could be done in the form of a table where, in a given row in the table,

- the first column states the name of an independent variable (e.g., "performance expectancy"),
- the second column describes the original metric in which a unit change of this variable is measured (e.g., "a unit change of 'performance expectancy' is a change of 1 along a scale of 1 to 7),
- the third column identifies the dependent variable (e.g., "behavioral intention") that the independent variable affects, and
- the fourth column indicates, in the dependent variable's original metric, the amount by which the dependent variable changes as a result of a unit change in the independent variable (e.g., "there is a change of 0.47 in 'behavioral intention' along a scale of 1 to 7 where there is a unit change in 'performance expectancy'").

Such a table would help to offer a full disclosure of whatever practical, real-world difference is made, or not made, by what is being presented as the factor of interest to a manager (i.e., the independent variable).

There is a final caveat that a full disclosure would require. It is one that follows naturally from

the emphasis that this essay places on the measurement of one factor's impact on another factor (where, in statistical terminology, this impact refers to the unstandardized estimate of the beta coefficient in a multivariate analysis). Consider a manager who is presented with the statistical results of a research article and genuinely desires to apply these results. Suppose that the population from which the sample in the research article was taken does not include the people in the manager's organization (e.g., the population in the research article consists of individuals in manufacturing firms in the Midwest region of the United States whereas the manager's organization is a federal agency in Washington, D.C.). Suppose, further, that the measurement of interest, reported in the article, is the measurement that a person whose "performance expectancy" increases by 1 unit (along a scale that goes from 1 to 7) experiences an increase of 0.47 units in his or her "behavioral intention" to use the technology (where the 0.47 units is also measured along a scale that goes from 1 to 7). Will the measurement of 0.47 also hold for the individuals in the manager's own organization? The answer is "not necessarily" or, most likely, "no." The reasoning behind this is that the population relevant to the manager is different from the population in the research article, thereby requiring a new sample – one from the manager's own organization – to be taken. Then, the statistical model in the research article would need to be fitted to the new population (the population relevant to the manager) with the data in new sample, with the result that the measurement of interest would not necessarily (and, most likely, would not at all) be 0.47. For this new measurement, whatever its value, the level of statistical significance would not necessarily (and, most likely, would not at all) be the same as the one associated with the original measurement of 0.47; indeed, the new measurement could even turn out to lack statistical significance. Any assessment of practical significance would require consideration of this final caveat.

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Arguably, more information-systems research follows statistical approaches than any other approach. The relevance of statistically conducted information-systems research is therefore a key part of the relevance of information-systems research in general. Statistically conducted information-systems research, however, has focused on statistical significance and ignored practical significance. This essay has demonstrated how to link relevance to practical significance so that the latter is explicitly

recognized as part of the former. Future information-systems research needs to take practical significance seriously. We recommend that this be attempted in the short run by the inclusion of tables, such as the one just described, in statistical research articles and, in the long run, by pursuing two strategies: one strategy is to develop a new research-and-publication genre different from that of standard research articles for the purpose of addressing relevance and practical significance; the other strategy is to perform empirical research to answer the seven questions.

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