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The Impact Of Augmenting Traditional Instruction With Technology-based Experiential Exercise

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Traditional instruction, under an experiential learning model, is neither the sole nor a sufficient means of learning. Learning systems that engage students as contributors to learning offer opportunities for educators. The availability of technology to support experiential exercises represents one opportunity to augment traditional instruction. In a quasi-experiment, traditional-only instruction was compared with traditional instruction augmented with a technology-based, experiential exercise. The two instructional methods were evaluated based on student learning, intended behavior, and satisfaction. Our findings suggest that augmented instruction significantly increased student learning and satisfaction. The implications for students are clear. However, there are also noteworthy implications for faculty.

INTRODUCTION

Business educators need to create learning systems that engage students as contributors to learning [Lengnick-Hall & Sanders, 1997]. Using technology to support experiential exercise offers one opportunity to augment traditional instruction. Students currently entering universities are familiar with computers and expect to learn in a technology-based environment. Educators can enhance learning by taking advantage of the capabilities of available technology and student skills in using that technology. Although a university faculty survey showed support for increasing the availability of technology, the same survey revealed faculty members were reluctant to actually use that technology [Pearce, 1999]. Other research has specifically found faculty resist incorporating technology into curricula [Bilmoria, 1997; Clinebell & Clinebell, 1995].

Faculty resistance to incorporating technology may be due, in part, to the fact that the benefits of alternative pedagogy over traditional pedagogy has not been convincing

[Armstrong, 1995; Druckman, 1995; Ruben, 1999], or to the fact that research has focused on benefits to students while downplaying those to the instructor. For example, while Smith [1996] found that using a technology-based exercise results in more engaged students and an increase in their knowledge, benefits to the instructor were not addressed. Further, a need remains for quantitative studies that compare the effectiveness of teaching methods [DeNeve & Heppner, 1997]. Our paper compares two methods for teaching selected personal finance topics as part of an introductory management course.

Personal finance lessons were included in all sections of the course because our academic department perceived students needed the information. For example, Chen and Volpe [1998] found the average correct score by college students at multiple universities on a 9-item finance knowledge test is 53 percent. This is a concern because personal finance decisions ultimately impact quality of life [Volpe, Chen & Pravliko, 1996]. Although perceived need drove the decision to include personal finance, all lessons were integrated within the broader course framework of the functions of management—planning, organizing, leading, and controlling. Two different pedagogical approaches were evaluated using a variety of measures. The different approaches involve traditional lecture, *Traditional-only*, and lecture augmented by a technology-based experiential exercise, *Augmented*. The study empirically compares learning, propensity to invest, and satisfaction of students in an introduction to management class with the same instructor in different semesters. The goal of the present study is to report benefits of augmenting traditional lecture with technology-based, experiential exercise from the standpoint of both students and instructors.

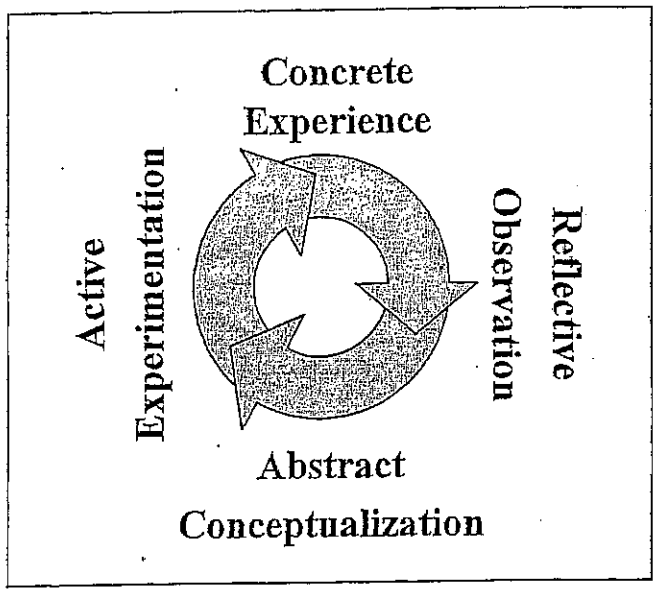
In this six-section paper, we 1) introduce the research focus; 2) develop the theoretical model and research hypotheses; 3) present our methodology; 4) report our results; 5) discuss the contributions, implications, and limitations of our research along with future research opportunities; and 6) summarize the conclusions of the study.

Theoretical Model and Hypothesis

Kolb [1984] identified learning as a social process based on cumulative experience. Under experiential learning, ideas are not fixed and immutable, but are formed and reformed through experience. Further, knowledge is created through the transformation of experience into a sense of order that guides future actions. Tension between expectation and experience is inherent in Kolb's [1984: 42] circular model of learning. See Figure 1.

The central premise in Kolb's model of experiential learning is that neither figurative representation nor transformation of experience alone is sufficient for learning to take place. Therefore, under his model traditional instruction that focuses primarily on figurative representation of material without experience is neither the sole means nor sufficient means of learning. Following Kolb's logic, the effectiveness of traditional instruction could be improved by adding experiential exercises. *Effectiveness* is defined

Figure 1. Experiential Learning Model



here as meeting the criteria of a "high-quality" education outlined by Lengnick-Hall and Sanders [1997: 1335]: 1) high levels of learning, 2) high levels of change or intention to change behavior, and 3) highly positive reactions of students towards a course.

Learning is the premise of education; turning students into active participants through experiential exercise should increase learning by providing both figurative presentation of concepts and experience acting on those concepts [*e.g.,* Ciccotello & Green, 1997; Dahlquist, 1998; Ball & Holt, 1998; and Chapman & Sorge, 1999]. Scott [1998] even argues that it is part of an instructor's responsibility to plan classroom experiences that activate learning and avoid merely presenting knowledge. Experiential exercises require students to act as more than passive receptors of knowledge presented by an instructor. Further, the addition of technology can result in more-engaged students and an increase in their knowledge [Smith, 1996]. Presentation of figurative concepts through a technology-based, experiential exercise better meets the threshold of learning implied by Kolb [1984]. Therefore, we hypothesize that:

H1: Traditional instruction augmented by a technology-based, experiential exercise will lead to higher measured learning of declarative knowledge.

synthesizing experience [Kolb, 1984]. Self-efficacy should increase when student involvement leads to active learning of figurative concepts through experience and reflection. Combined concrete experience and self-efficacy should lead to more changes in intended behavior than presentation of concepts alone. In this experiment, a safe exercise where students can synthesize experiences necessary for successful investing should increase their propensity to invest. Additionally, an assumed advantage of experiential learning is that learning will transfer to other settings where the skills can be used [Druckman, 1995]. Therefore, we hypothesize that:

H2: Traditional instruction augmented by a technology-based, experiential exercise will lead to a higher propensity to invest.

For a variety of reasons, instructors are also concerned with student satisfaction. First, classes in which students are not satisfied are less enjoyable for both students and instructors. Second, student satisfaction with a course and an instructor will most likely affect student evaluation of instruction—a major criterion used to evaluate faculty for retention, merit, promotion, and tenure decisions. According to Salemi, Saunders and Walstad [1996], increased course participation may be associated with higher student satisfaction. A potential advantage of experiential learning then is that student satisfaction relates positively to involvement. Therefore, we hypothesize that:

H3: Traditional instruction augmented by a technology-based, experiential exercise will lead to higher student satisfaction.

METHODOLOGY

Sample

Research approval was obtained from the local Institutional Review Board (IRB), and completion of survey instruments by students was voluntary. Subjects in this study included 115 students with the same instructor, enrolled in eight different sections of an introduction to management course over two semesters. This course is required of all graduates and course registration procedures randomly assign students to course sections. The sample included only full-time male and female students with varied demographic backgrounds. Table 1 shows a demographic breakdown of the sample.

The survey had three sections: demographic information, investment intent, and investment knowledge. A one-way analysis of variance (ANOVA) was conducted on student responses to pretest surveys for pre-existing demographic differences between semesters. The only statistically significant difference is that in the traditional-only semester students were predominately sophomores, while the majority of students in the augmented semester were juniors. This difference is not considered practically significant because pretest knowledge scores between the semesters are not significantly different ($p=.22$).[2]

Table 1. Sample Demographics

	Traditional Only [a] (n = 46)	Augmented [a] (n = 69)
Gender		
Male	33 (71.7%)	50 (72.5%)
Female	13 (28.3%)	19 (27.5%)
Academic Class		
Senior	None	15 (21.7%)
Junior	1 (2.2%)	31 (45.0%)
Sophomore	45 (97.8%)	23 (33.3%)
Freshmen	None	None
Parents' Annual Income		
More than $120,000	11 (23.9%)	12 (17.4%)
$75,000 to $119,999	11 (23.9%)	19 (27.5%)
$55,000 to $74,999	11 (23.9%)	19 (27.5%)
$35,000 to $54,999	5 (10.9%)	14 (20.3%)
Under $35,000	5 (10.9%)	3 (4.3%)
Left blank	3 (6.5%)	2 (3.0%)
Investing Experience [b]		
Yes	23 (50.0%)	32 (46.4%)
No	23 (50.0%)	37 (53.6%)

[a] Usable posttest responses

[b] Based on self-reporting current investments in stocks, bonds, or mutual funds significant because pretest knowledge scores between the semesters are not significantly different (p = .22).[2]

Procedures

The study reviews the results of a two-stage, non-equivalent group, pretest-posttest, quasi-experimental design [Campbell and Stanley, 1966] with semesters as the unit of analysis to examine the effects of pedagogy on student knowledge, propensity to invest, and satisfaction. The primary author was the instructor for all sections evaluated and was in his second year of full-time college teaching. To help control for increased instructor experience from one semester to the next, the augmented semester preceded the traditional-only semester, making the hypothesized results less likely if increased instructor experience is significant.

The study involved a quasi-experimental design where different semesters were compared to keep graded assignments consistent within each semester. Students are often aware of differences across sections during any given semester. Comparing different semesters limits diffusion of treatment, a threat to internal validity. Further, both stages of the experiment used the same survey (pretest and posttest) and instructional material. For example, personal finance material presented in class used the same slides and involved the same five lessons (out of a total of 42 lessons). Additionally, the same supplemental personal finance handout was used both semesters.

The primary differences between how personal finance was taught each semester involved explaining how to participate in the exercise during the augmented semester, which required 5 to 10 minutes of class time and an additional course handout. Course syllabi for each semester also were modified to reflect different assignments. These differences between the semesters are further described below. All other differences between sections and semesters were primarily the result of student questions.

The augmented semester used traditional, lecture-based instruction with a technology-based, experiential exercise. At the beginning of the augmented semester, students were asked to complete a pretest containing demographic, knowledge, and investing-intent questions. Completion of the survey was voluntary and responses were anonymous; however, completion of the technology-based, experiential exercise was mandatory. In the exercise, each student created a $100,000 "simulated" portfolio on a website [InvestSmart, 2001]. The investment exercise required students to experience investing by making them invest at least $95,000 of their $100,000 in a combination of individual stocks, bonds, or mutual funds. Four one-page papers, each worth 1 percent of the course grade, were assigned for students to demonstrate their participation, which was also tracked from the instructor account created on the website. The exercise ran between "book-end" personal finance lessons at the beginning and end of the semester. The first set of classes involved two lessons to introduce personal finance terminology and concepts. The second set of classes involved three lessons at the end of the semester designed to summarize investment options, explain the trade-off between risk and return, and reinforce the importance of using time value of money analysis in personal finance. In both semesters, students were assigned a time value of money homework set at the end of the semester worth 5 percent of the course grade and were asked to voluntarily complete an anonymous posttest. Finally, standardized course evaluations were administered at the end of the semester—separately from the posttest. Completion of course evaluations is voluntary and responses are anonymous.

The second semester used only traditional, lecture-based instruction. The same five personal finance lessons were presented in sequence at the end of the semester, using lecture-only. Before the beginning of the personal finance lessons, students were asked to complete a pretest. Completion of the survey was voluntary and responses were anonymous. To replace the 4 percent of the course grade accounted for by the first semester's investment exercise assignments, we substituted four announced quizzes on

topics other than personal finance. These quizzes were spread throughout the semester with each quiz worth 1 percent of the course grade. The difference in grading between the semesters gave more weight, 4 percent of course grade, to personal finance. However, in both semesters, at least 5 percent of a student's course grade was related to personal finance assignments. That both semesters required a 5 percent threshold helps meet Wolfe and Robert's [1986] finding that once academic credit exceeds a relatively low level, grade weights do not significantly increase student motivation. At the end of the semester, students were asked to complete a voluntary, anonymous posttest. The study's usable participation rate in the second semester was 58.2 percent, or 46 out of 79 students, compared to a participation rate in the first semester of 89.6 percent, or 69 out of 77 students. The decreased usable participation rate during the second semester resulted from providing students a numerically matched set of tests at the administration of the pretest and asking them to keep and not lose the matched posttest.[3] Again, standardized course evaluations were administered at the end of the semester—separately from the posttest. Completion of course evaluations is voluntary, and responses are anonymous.

Measures

Two measures were used in this study. First, we constructed a questionnaire that students completed as a pretest and posttest. Four items collected demographic information about students, see Table 1. Nine items related to personal finance declarative knowledge. *Declarative knowledge* is defined as explicit knowledge that we can report and of which we are consciously aware [Anderson, 1995]. The declarative knowledge questions were based on Vanguard Group's "Investor Literacy Quiz" [Vanguard, 1998] and relevant learning objectives based on course material. Knowledge items were measured based on multiple-choice questions with either 4 or 5 possible responses (Survey available from the authors). Additionally, students were asked one item each on their intent to invest in either stocks or bonds in the next year. Both items were measured on a five-point Likert scale, with 1 indicating the most positive response (strongly agree) and 5 indicating the most negative response (strongly disagree). Again, pretest knowledge scores between the semesters were not significantly different ($p = .22$).

The second measure used data from the institution's standardized student evaluation as a measure of student satisfaction. Literature supports using student evaluations as a measure of student satisfaction [Kim et al., 2000; Ortinau & Bush, 1987]. The instructor portion of the evaluations is based on 12 items with a scale of 1 (very poor) to 6 (excellent). Results provided to instructors are summarized by course section with each section containing approximately 20 students. Reliability analysis was not performed on the student evaluation because only summary data is available. Table 2 shows descriptive statistics for the instructor portion of student evaluations for experimental sections and

Table 2. Descriptive Statistics for Instructor Evaluations

	Traditional Semester Only			Augmented Semester		
Item	Instructor Average[a]	Institution Average[b]	Institution Std. Dev.	Instructor Average[a]	Institution Average[c]	Institution Std. Dev.
1	4.6	4.7	1.1	5.1	4.6	1.1
2	5.3	4.7	1.0	5.4	4.8	1.1
3	5.0	4.8	1.1	5.2	4.7	1.1
4	4.9	4.8	1.1	5.1	4.8	1.1
5	5.1	4.9	1.0	5.3	4.9	1.0
6	4.8	4.8	1.0	5.2	4.8	1.0
7	5.4	5.3	0.9	5.6	5.4	0.9
8	5.2	5.1	1.0	5.6	5.1	1.0
9	4.9	4.9	1.1	5.3	4.9	1.0
10	5.1	4.9	1.1	5.4	4.8	1.1
11	5.1	5.0	1.0	5.5	5.0	1.0
12	5.1	5.1	1.0	5.4	5.1	1.0
Avg.	5.04	4.92	1.03	5.34	4.91	1.03

[a] n = 4 sections; [b] n = 599 sections; [c] n = 562 sections

the institution as a whole.[4] Separate measurement of our variables represents a procedural remedy to common method variance [Podsakoff & Organ, 1986].

Scale reliability was analyzed for both the Knowledge and Investment Intent scales. Knowledge scale reliabilities revealed that deleting item 9 would increase scale reliability indicating it did not adequately discriminate student knowledge, because over 70 percent answered it correctly. Item 9 was deleted from the Knowledge scale, and the Cronbach alpha for the resulting 8-item Knowledge scale was 0.766. Analysis of the 2-item Investment Intent scale resulted in a Cronbach alpha of 0.727, so scale reliability for both the Knowledge and Investment Intent scales meet Spector's [1992] suggestion that scale reliability be 0.7, or higher. Because the Knowledge and Investment Intent scales were collected using a common method, confirmatory factor analysis was conducted to determine discriminant validity [Podsakoff & Organ, 1986]. The factor analysis revealed the 8 Knowledge items and 2 Investment Intent items loaded on separate factors—that is, the Knowledge and Investment Intent represent distinct factors.

Table 3. Results for the Effects of Pedagogy on the Outcome Measures

Outcome Measure	df	SS	F	p-value	Eta-Squared	Observed Power [a]
Knowledge	1	803.31	351.45	.000	.757	1.0
Knowledge*Pedagogy	1	69.82	30.55	.000	.213	1.0
Investment Intent	1	141.64	94.97	.000	.457	1.0
Investment Intent*Pedagogy	1	3.76	2.52	.115	.022	0.35

[a] Based on alpha = .05

RESULTS

We assessed the relationship between Pedagogy and its impact on student Knowledge and Investment Intent using the General Linear Model repeated measures ANOVA procedure in *SPSS 10.0*, and we tested for main effects only. In the model, Pedagogy was a two-level, between-group factor with the traditional-only semester representing level 1 and the augmented semester representing level 2. Knowledge and Investment Intent were two-level within-group factors with the pretest representing level 1 and the posttest representing level 2. Results are reported in Table 3.

The results show that the changes in Knowledge and Investment Intent, without regard to pedagogy, are significant. This finding confirms the implicit assumption that exposing students to personal finance can increase both Knowledge and Investment Intent. For example, the eta-squared, analogous to R-squared, shows that 75.7 percent of the variance in the Knowledge measure can be attributed to having exposed students to personal finance material. To aid interpretation, descriptive statistics for the measures are shown in Table 4. Based on the scales employed, larger scores for Knowledge indicate a higher level of knowledge and smaller scores for Investment Intent indicate a higher level of intent to invest.

The results of Knowledge interacting with Pedagogy indicate that traditional instruction augmented with technology-based, experiential exercise significantly increased student knowledge ($p \leq .001$). Additionally, augmentation of traditional instruction explained 21.3 percent of the variance of student knowledge between the semesters. These findings provide strong statistical support for Hypothesis 1.

The results of Investment Intent interacting with Pedagogy indicate that traditional instruction augmented with a technology-based, experiential exercise does not significantly increase student investment intent ($p = .115$). These findings provide minimal statistical support of Hypothesis 2.

Table 4. Descriptive Statistics for Knowledge and investment Intent

Measure	Treatment	Mean	Std. Deviation	N
Knowledge [a] (pretest)	Traditional-only	1.54	1.53	46
	Augmented	1.78	1.57	69
	Total	1.69	1.55	115
Knowledge [a] (posttest)	Traditional-only	3.37	1.65	46
	Augmented	4.46	1.57	69
	Total	4.03	1.68	115
Investment Intent [b] (pretest)	Traditional-only	0.52	1.13	46
	Augmented	-0.35	0.72	69
	Total	0.0	1.0	115
Investment Intent [b] (posttest)	Traditional-only	0.59	1.15	46
	Augmented	0.39	0.63	69
	Total	0.0	1.0	115

[a] Based on number correct out of 8 questions

[b] Based on factor regression weights

We assessed the relationship between Pedagogy and Student Satisfaction by standardizing the augmented and traditional-only instructor evaluation scores, then comparing them using a *t*-test for independent samples, see Table 2. Instructor evaluations for the augmented semester were significantly higher ($p \leq .001$). Similar results are achieved from a non-parametric, chi-square test comparing the number of instructor evaluation items that were higher in the augmented semester.[5] These findings provide strong statistical support for Hypothesis 3.

DISCUSSION

As faculty members, we were quite pleased with the outcomes. Our results confirm the assumption that merely presenting information to students can increase declarative knowledge. However, integrating a technology-based, experiential exercise to augment traditional instruction results in significantly higher declarative knowledge than traditional instruction alone, strongly supporting Hypothesis 1. We believe that the technology-based, experiential exercise forced students to spend more time on course material by distributing material throughout the semester. Hypothesis 2, that student propensity to invest would be significantly higher when exposed to augmented

instruction, was not supported at traditional significance levels. However, the power of the test for different student investment intent based on pedagogy was low, so a stronger relationship may exist. Further exploration the impact of experiential exercise on student intent and behavior represents an opportunity for continued research. Finally, augmented instruction, as measured by teacher evaluations, resulted in significantly higher student satisfaction, strongly supporting Hypothesis 3. The bottom line is that traditional instruction augmented with a technology-based, experiential exercise led to greater student learning, while increasing their satisfaction with the learning experience.

Our results suggest augmenting traditional instruction with an experiential exercise is more forgiving of instructor inexperience. This claim is made assuming that experience generally helps instructors. The lead author taught the augmented semester at the beginning of his second year of teaching and before the traditional-only semester—implying relative inexperience as well as less experience in the augmented semester. As measured by instructor evaluations, augmenting traditional instruction with a technology-based exercise had a larger effect than increased instructor experience on student satisfaction. Additionally, student comments (from an optional written part of the course evaluation) were more enthusiastic during the semester with the experiential exercise:

1. "Doing this finance project has been one of the greatest learning experiences for me. It has taught me that I will have to watch what I spend when I graduate, and to spend wisely, opening an IRA and starting mutual funds so I can plan for my future."

2. "I must acknowledge the fact that I feel that this class has been one of the best and most useful classes here. By far, it has been the most practical and usable class I've taken to date. Ever since I learned about the stock market and investing money I have wanted to partake in it and reap the potential rewards it offers."

A potential criticism of our findings is that students in the technology-based, experiential sections were exposed to personal finance over the entire semester, or that they exhibited maturation differences. We answer this criticism by saying that was the point. By including the semester-long exercise, students were encouraged to become active learners *outside the classroom*, because the amount of in-class time on personal finance remained the same. In other words, by including the exercise, students were forced to spend more time on and gain experience with personal finance material without sacrificing other course content.

Another potential criticism [particularly among management (vs. finance) educators] is that the exercise involved personal finance. We answer this criticism in two ways. First, there is a clear need for students to learn about personal finance [Chen & Volpe, 1998; Volpe, Chen & Pravliko, 1996], and only 5 out of 42 lessons were dedicated to personal finance. Second, the use of technology-based, experiential exercise as part of a semester-long activity outside of class is not limited to personal finance. Therefore, the relevance of the study is generalizable. For example, Dunham developed "The Manager's Workshop," a commercially available computer-based simulation, to provide students

experience with employee motivation [Manager's Workshop, 2001]. Additionally, "StratX" is a computer-based, experiential marketing and business strategy exercise, developed by Larreche and Gatignon [StratX, 2001]. Therefore, similar results could be expected on other topics from instruction augmented with experiential exercise. On the other hand, the time required for experiential exercises limits the number of topics that can utilize them in any one course.

IMPLICATIONS

Our research has important implications for theory. First, our results support Kolb's [1984] experiential view of learning showing how both concrete experience and figurative concepts are important for learning to occur. Second, there are several implications for how instructors teach, what they teach, and the impact on students.

Our research also has important implications for instructors. Our findings suggest that educator resistance to incorporating technology cited by Bilmoria [1997], Clinebell and Clinebell [1995], and Pearce [1999] limits not only student learning and satisfaction, but also potentially instructor satisfaction as well. Technology-based experiential exercise was found to impact student satisfaction significantly and positively. Student comments suggest that students enjoyed the class with the experiential exercise more. This enjoyment contributed to a more positive classroom atmosphere and higher instructor evaluations. Our results also support previous studies that show college students' knowledge about personal finance is low. Educators should be concerned about this because poor financial decisions early in life can have long-lasting, negative impacts. Hence, an important conclusion of our research is that presenting a few personal finance lessons as part of a required course can significantly increase student knowledge. Finally, the experiential exercise helped demonstrate to students the complexity and ambiguity of decisions and facilitated development of "real-world" skills.

LIMITATIONS

Like every study, this one has limitations. The primary potential limitation may involve the generalizability of our findings to other student groups. The research was conducted at a military service academy where students are simultaneously more diverse and more select than students at typical colleges or universities. On one hand, students at a service academy represent a wide cross-section of America, with students from every Congressional district and most demographic and socio-economic groups. On the other hand, service academies are ranked in the top 10 toughest schools to get into with over half of entering students representing the top 10 percent of their high school classes [*Princeton Review*, 2001]. Additionally, every student has a personal computer with Internet connection, and none of the students is married or has dependents. Further, class attendance is mandatory, and a relationship between class attendance and student

performance has been found to exist [Chan, Shum & Wright, 1997]. Finally, students are guaranteed employment upon graduation. Nevertheless, although the students in the study's sample display unique characteristics, the scope of Kolb's [1984] experiential learning theory extends beyond this group. Validating our findings at other institutions represents an opportunity for future research.

Another potential limitation is that student responses in the augmented semester were not collected in matched pairs, posing a threat to internal validity by not accounting for regression to the mean [Campbell & Stanley, 1966]. However, a computer simulation involving 1,000 comparisons of randomly generated data based on the pretest and posttest sample means and standard deviations resulted in the conclusion that knowledge was significantly different 100 percent of the time at $p < .05$. Therefore, 69 posttests were randomly matched to pretests for analysis. The observed power (see Table 3) for the variables with significant relationships was 100 percent in each case. Power of a statistical test is a function of the amount of random error, the effect size, sample size, and significance (alpha) level. Randomly matching the surveys would increase the random error and make significant results *less* likely. However, the impact of the treatment (effect size) appears to have overcome this limitation given the sample size and observed statistical significance. Therefore, the impact of not collecting data in matched pairs and of randomly assigning scores to create matched pairs in our data appears to be negligible. Additionally, the procedure used in the traditional-only semester to collect matched data artificially decreased participation.

The phrasing of the questions about investment intent may limit the generalizability of the results. Recall that the hypothesis that augmented instruction would enhance intent to invest was not supported at traditional significance levels. The phrasing of the intent questions may have driven this result. The question asked students whether they intended to invest in stocks and bonds *over the next year*. The experiential exercise may have increased student's intent to invest—had they the capacity to invest in the next year. In short, the questions did a poor job of differentiating investment intent from investment intent conditional upon investment capacity. To the extent it biased against Hypothesis 2, the poor phrasing of the intent questions does not represent a fatal methodological flaw, but care should be exercised in generalizing from our result.[6]

A final limitation relates to the between-group analysis in the separate sample design because the different pedagogical approaches involved different semesters. Comparing different semesters controlled the impact of treatment diffusion [Dudley & Shawver, 1991: 22]. Additionally, implementing the augmented semester before the traditional-only semester controlled for the impact of increased instructor experience. A more experienced instructor in the traditional-only semester should make our significant results *less* likely because increased teaching ability would suggest more learning on the part of students. Instead, our results suggest traditional instruction augmented by a technology-based, experiential exercise has a larger effect than increased instructor experience.

CONCLUSION

The key finding is that augmenting traditional instruction with technology-based, experiential exercise makes instruction more effective. Our findings suggest that augmented instruction better met the requirements of high-quality education by resulting in significantly higher student learning, intended behavior changes, and satisfaction. The implications for students are clear. There are also noteworthy implications for faculty. Augmented instruction appears to compensate for an instructor's lack of experience and leads to higher instructor evaluations.

ENDNOTES

[1] Acknowledgements: The authors would like to dedicate this paper to the memory of Dr. Christy Strbiak, a teacher, colleague and friend. Additionally, we thank John Thornton, Janet Near, Samuel Thompson, Janice Jackson and participants at the 2002 Midwest Academy of Management and the 2003 Academy of Financial Services conferences for commenting on previous drafts of this paper as well as Jack Niday and James "Micky" Norris for substantial editorial advice. This paper was written while Dave King was at Indiana University. We expect to maintain material related to tis research—including the survey—at www.williamjennings.com. DISCLAIMER: The opinions expressed are the authors and not necessarily those of the USAF Academy, U.S. Air Force, or Department of Defense.

[2] In addition to the lack of statistically significant pretest knowledge differences, other factors underscore the class-year difference is not relevant. The research was conducted at a service academy were freshman are the only academic class intentionally treated differently from the other academic classes. Additionally, drop out rates after the freshman year are consistently low, and institution-wide cumulative GPA's are comparable across academic classes.

[3] That is, the students received two copies of the test at the time the pre-test was administered. The matched numbers met IRB requirements for anonymity. The pre-test was submitted to the instructor upon completion. The students were instructed to retain (and not lose) the second copy of the test. After the five-lesson finance block, students were asked to complete the posttest (using the second copy of the test). The reason for the lower participation rate in the second semester was that students lost the second copy of the test. Because this administration of the pretest-posttest pair was conducted with matched pairs, students who lost their second test were eliminated from the sample. This explains the decline in participation rate from 89.6 percent to 58.2 percent—the participation rates quoted are for *usable* tests. We do not believe subject mortality differences between semesters to be problematic since students without posttests (who lost the second copy) asked for new copies of the posttest—unfortunately these posttests were not usable since they were unmatched.

Note further that the threat that students answered the posttest prior to its administration after the five-lesson block would have opposite the hypothesized effect.

As a recommendation for future research, we suggest that the students label the pretest and posttest with an easily remembered number (e.g., a date or telephone number). Under this design, the students are not responsible for retaining the posttest. Chosen appropriately the number abets anonymity and enhances the usable participation rate.

[4] We should clarify that the institution averages in Table 2 are for all students taking all courses. That is, they are not part of the quasi-experiment. The consistency in the institution averages between the two semesters is in indication of the reliability (i.e., repeatability) of the instructor-evaluation instrument, not a contra-indication of our results supporting H3. The results supporting H3 rely upon the instructor average, not the institutional average.

[5] One should not be concerned that this result relies on a t-test with $n = 4$. The calculations were based on the grand average of the 12 evaluation item's average scores for a minimum of 60 students. That is, the n is significantly larger than 4. However, the resulting t-test results in a value of 3.64, which is significant at a single degree of freedom. The averages should be normal under the Central Limit Theorem; however, if one questions the normality of these elements, the non-parametric Chi-Squared test (in which every evaluation element is higher in the augmented semester) is significant at $p < .001$ and should be persuasive support for Hypothesis 3. Finally, although the instructor's teacher evaluations are higher than the institutional mean, they are not statistically significantly different from the mean. This means the results supporting Hypothesis 3 are more generalizable than if the instructor were exceptional.

[6] We thank an anonymous reviewer for highlighting the issues discussed in this paragraph.

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