5 With the suggested setup defaults, the graph should show the template for a supply function; if not, it is probably because you did not set the Product Market to be 'fixed price' or because you changed the capacity cost or entry settings.

6 Free allocation in these proposals is achieved in a variety of ways including grandfathering to emitters and also to regulated local distribution companies, which are expected (with regulatory oversight) to pass on the opportunity cost of the permits to consumers.

7 See press release 20 December 2006, URL: www.bundeskartellamt.de/wEnglisch/News/Archiv/ArchivNews2006/2006_12_20.php

8 RWE is one of the largest electricity producers in Germany. RWE is subject to the EU ETS and, as all entities participating in EU ETS, has received a major share of their permits for free according to a grandfathering procedure.

9 As quoted by Thompson Reuters, 21 January 2009.

Contact details

Charles Holt
Economics
University of Virginia
Charlottesville
VA 22904-4188
Email: holt@virginia.edu

Erica Myers
University of California
Berkeley
Email: ericamyers@rff.org

Dallas Burtraw
Resources for the Future
Washington DC
Email: burtraw@rff.org

Markus Wråke
IVL Swedish Environmental Institute
Stockholm
Sweden
Email: markus.wrake@ivl.se

Svante Mandell
VTI Swedish National Road and Transport Institute
Stockholm
Sweden

Do Classroom Experiments Affect the Number of Economics Enrolments and Majors? A Study of Students in the United States

Tisha L. N. Emerson and Beck A. Taylor

Abstract

The present study follows a cohort of 290 students, at an American university, who were exposed to two different pedagogical approaches – traditional 'chalkand-talk' and classroom experiments. Although we find that the number of majors and upper division economics classes taken were not significantly different between the two groups as a whole, there were some differences across individual characteristics. For example, males who were exposed to classroom experiments enrolled in more upper division economics courses than similar males in the control group. Also, students in the experimental group who had taken economics in high school enrolled in more upper division economics courses than their counterparts in the control group. Minorities in the experimental group, however, enrolled in fewer upper division economics classes than their colleagues in the control group.

Introduction

In the United States, between 1950 and 1998 the proportion of bachelors degrees awarded in economics fell nearly 60 per cent from a high of 3.4 per cent of all degrees to a low of 1.4 per cent (Becker 2001). More recently, this decline has ebbed with considerable growth in the major in the late 1990s and early 2000s when virtually the entire decline of the early 1990s (some 27 per cent) was reversed by 2003. The number of economics degrees awarded between 2005 and 2007 changed little, but

experienced significant growth (over 6 per cent) from 2007 to 2008. Nonetheless, economics is losing market share, experiencing lower growth in undergraduate degrees awarded than mathematics and the social sciences in general and only slightly higher (1–2 percentage points) growth than business and psychology (Siegfried 2009). In response to the overall loss of market share (the recent resurgence of the economics degree numbers notwithstanding), many have called for a review of pedagogies used in economic education (e.g. see Becker 2001) – both for their effect on student learning and for effects on students' choice of major.[1]

One of the more recent pedagogical approaches employed in economics is the use of classroom experiments. In 2004, we published a related study comparing student achievement between students exposed to two different pedagogical approaches – traditional 'chalk-and-talk' and classroom experiments (see Emerson and Taylor 2004). We found that students exposed to classroom experiments experienced significantly higher levels of achievement than those in the control group. Since the time of the original study we have followed our cohort in order to observe their subsequent choices of courses and major to discern whether the experimental treatment has any longer term effects in additional to the initial differential in observed achievement. In this paper, we present the findings of our continued observation. We find no statistically significant relationship between exposure to the experimental method and either majoring in economics or the number of upper division[2] economics courses taken by students. However, we do find some differential effects of the experimental treatment conditional on student characteristics. For example, students in the experimental group who had taken economics in high school enrolled in more upper division economics courses than their counterparts in the control group. Also, males who were exposed to classroom experiments enrolled in more upper division economics courses than males in the control group. Minorities in the experimental group, however, enrolled in fewer upper division economics classes than their equivalent in the control group.

To this point the literature on the effectiveness of classroom experiments as a pedagogical tool largely focuses on student learning and achievement as opposed to more persistent outcomes such as choice of major. With regard to student learning, early studies provide little more than anecdotal evidence regarding the efficacy of classroom experiments (see, for example, Williams and Walker 1993; DeYoung 1993) and often focus on learning over periods of time as short as one class session and using only one experiment (Frank 1997). More recent studies extend this work to assess the effectiveness of multiple experiments over the course of a semester-long principles of microeconomics course. Findings from these studies are mixed with either no statistically significant effect of the experimental approach on student achievement (Cardell *et al*. 1996) or significantly positive effects (Dickie 2000; Emerson and Taylor 2004, 2007) as measured by the Test of Understanding in College Economics (TUCE).[3] As such, it would appear that the use of classroom experiments at best improves student achievement and at worst will not hurt it.

Greater student achievement is a favourable outcome, but this is not the only outcome with which economists are concerned. The fall in the proportion of undergraduate degrees in economics over the 1990s gave rise to many concerns regarding students' choices of major. The literature investigating the choice of economics as a major has primarily focused on the impact of student level characteristics. Not surprisingly, students with greater aptitude and ability in economics as measured by exam scores (Ashworth and Evans 2001) or grades (Horvath *et al*. 1992; Sabot and Wakeman-Linn 1991; Jensen and Owen 2001) as well as those with better math skills (Ashworth and Evans 2001; Jensen and Owen 2001) are more likely to major in economics. Students with prior exposure to or knowledge of economics are also more likely to study economics at the university level (Ashworth and Evans 2001; Worthington and Higgs 2004). Additionally, Horvath *et al*. (1992) and Siegfried *et al*. (1996) show that females are less likely to persist from a first to a second introductory economics course than are males. Chizmar (2000), however, finds no gender difference in persistence in the major.

In addition to the impact of individual characteristics on the choice of major, Becker (2001) and others suggest that the traditional pedagogy used in economics (i.e. 'chalk-and-talk'; see Becker and Watts 1996) may be an important (negative) factor in students' major choice and call on economics instructors to revamp their approach to teaching economics in order to make economics the 'sexy' social science. To our knowledge the only study to date that investigates the relationship between pedagogy and students' choice of economics as major (or taking additional courses in economics) is Jensen and Owen (2001). They find that the pedagogical decisions of economics instructors can have a moderate impact on student choices and that the effect may differ by gender. In particular, as the proportion of class time devoted to lecture (as opposed to all other instructional strategies) increased, the probability that males intended to take another economics course (as self-reported) increased, while it decreased for females. Jensen and Owen, however, find no effect of time spent in lecture on a student's self-reported probability of majoring in economics. Thus, it would appear that there is limited and weak evidence that pedagogy can influence student outcomes beyond the course in which the pedagogy is employed.

The existing literature examining students' persistence in economics beyond a first course or ultimately to a major has yet to investigate the role that a pedagogy

employing classroom experiments might play in student outcomes. Additionally, the literature that attempts to estimate the impact of the experimental teaching method has yet to examine any outcomes beyond the initial course. This study is the first, to our knowledge, that brings these two streams of literature together.

Data and empirical methodology

Students in our study were enrolled in one of nine class sections[4] of the core course in microeconomics principles at Baylor University during the 2002 spring semester. Two of these sections (the treatment, or experimental, group consisting of 59 students) supplemented the standard curriculum using 11 in-class experiments taken from the Bergstrom and Miller (2000) textbook. The remaining seven sections (the control group consisting of 241 students) used the traditional lecture-oriented methodology.[5]

Aside from the treatment group's use of experiments, considerable effort was made to maintain as much homogeneity as possible, both between and within the control and treatment groups. Both sections within the experimental group were organised in the same manner. Students in this group participated in one experiment per week (usually taking one full class period), while the remaining class time was devoted to lecturing on theoretical concepts and reconciling those concepts to the data generated from the experiments.[6]

After the conclusion of the course, we continued to track the students through each semester. During the ensuing five years, we recorded all subsequent economics courses taken by the students and recorded the students' major upon graduation.[7,8] These efforts allow us to construct two outcome variables for each student – (1) a dichotomous variable taking on a value of one when a student majored in economics (and zero otherwise) and (2) a count variable indicating the number of upper division economics courses taken by the student.

Modelling student outcomes

Following the literature, we hypothesise that a student's choice of courses and major is a function of student-specific characteristics, prior exposure to the discipline, performance in an introductory course, aptitude for economics, and pedagogy. In addition to students' enrolment in economics courses and ultimate major, we collected data on the students' final course grade in the principles of microeconomics course, cumulative grade point average at the beginning of the course (GPA, 4-point scale),[9] current and previously completed semester hours at the time of the principles of microeconomics course, the number of previous attempts in the same microeconomics principles course, whether the student had

Table 1 Descriptive statistics (experimental vs. non-experimental sections)

Variable	Non-experimental mean (SD)	Experimental mean (SD)
Economics Majors	0.08 (0.27)	0.07 (0.26)
Upper Division Economics Courses	0.60 (1.68)	0.38 (1.27)
Course grade	2.81 (0.95)	2.46† (1.14)
GPA	2.88 (0.68)	2.77 (0.69)
Principles of Macroeconomics	0.15 (0.36)	0.20 (0.40)
Male	0.61 (0.49)	0.60 (0.49)
Non-white	0.12 (0.32)	0.18 (0.39)
Number of previous attempts	0.09 (0.29)	0.15 (0.40)
Work hours per week	6.26 (9.76)	3.61† (8.10)
Semester hours completed	43.04 (19.24)	35.16† (19.12)
Current semester hours	14.20 (1.91)	14.07 (1.59)
Standardised number of absences	3.27 (3.14)	5.42† (5.80)
High school course in economics	0.78 (0.41)	0.85 (0.36)
Business student	0.77 (0.42)	0.82 (0.39)
Number of observations	234	56

† Experimental and non-experimental means are statistically different at the 5% (two-tailed) significance level or better.

taken economics in high school or a principles of macroeconomics course (prior to the microeconomic principles course), whether the student intended to major in an area of business, average weekly hours from employment, the total number of (standardised) student absences, gender and ethnicity, as well as instructor- and section-level effects.

Summary statistics (unconditional means and standard deviations) for outcomes as well as the explanatory variables used in our analysis, across control and treatment groups, are presented in Table 1. Students in the experimental and control sections majored in economics at similar rates of 7 and 8 per cent, respectively. Students in both sections also chose to enrol in upper division economics courses at similarly low rates (an average of 0.60 courses in the control and 0.38 courses in the experimental group). Students in the control and experimental groups were also similar in most other attributes of interest. With the exception of final course grade, average weekly hours devoted to employment, the number of semester hours completed, and the number of student absences, there were no significant differences in the means between the two groups. Students in the experimental group earned lower final course grades, devoted less time to employment, had completed fewer semester hours at the beginning of the course, and were absent more often than students in the control group. Lower course grades would tend to reduce the likelihood of students taking additional economics courses or majoring in economics. It is unclear a priori whether the other differences would have an impact on students' choices of courses or major.

A note regarding the experimental design and estimation method

As discussed in Emerson and Taylor (2004), a concern related specifically to our experimental design is the difficulty we have in measuring unobserved instructor-level effects. Although one preferred research design would include having each instructor teach both control and treatment sections, resource constraints prevented us from employing this design, or one that utilised random assignment of instructors. Instead, two instructors in our study taught the two sections in the treatment group and the remaining five instructors taught the other seven sections in the control group. As such, our treatment control variable is perfectly collinear with the instructor dummy variables, and we are unable to directly estimate via instructor dummy variables any unobserved instructor characteristics separately from the experimental effect. This becomes a problem (i.e. biased estimates of the treatment effect or any associated interactions) if, for any reason, the two instructors teaching the experimental sections had characteristics other than using the experimental methodology that were also correlated with student outcomes.

It is probably the case, however, that those unobserved instructor characteristics most likely to affect students' choice of courses and major are also correlated with observed section-level differences, namely the distributions on other student performance measures included in our data, specifically performance on the departmental final exam and student evaluations of the instructor and course. Thus, although we cannot include instructor-level dummy variables, we can include section-level information on the distributions (i.e. means and standard deviations) of the final exam and student evaluations, measures that are likely to be influenced by unobserved instructor- and section-level differences. Although not ideal, this technique will be likely to capture much of the variance in student outcomes accounted for by the variance in unobserved instructor heterogeneity.[10]

An additional concern arises with the possibility that the errors across students in the same section (with the same instructor and subject to the same environmental and peer effects) may be correlated (i.e. errors will be clustered within sections). Preferably, we would directly control for just such a circumstance in our estimation by allowing our error term to be divided into two separate components, one component common to all students in a section (cluster) that is dependent within section but independent across sections, and an idiosyncratic student × section error term that is independently and identically distributed across all students. Unfortunately, we do not have sufficient degrees of freedom to allow us to define clusters (at the section or instructor level) within our data using our estimation techniques.[11] If indeed the errors across students within a section are correlated this could result in biased standard errors.

Our empirical model may be written as student outcome$_i = \alpha + X_i\beta + \varepsilon_i$. In other words, the outcome of student *i* is a function of a constant, student-specific characteristics (X_i, including aptitude, educational background, gender, etc.) and an error term (ε_i).

Results

We estimate the impact of the experimental approach on the two measures of student outcomes for our usable sample of 290 students. Tables 2 and 3 report estimates for whether a student majored in economics and the number of upper division economics courses taken, respectively. We use a logit model to estimate whether a student majored in economics, and the number of upper division economics courses is estimated using a negative binomial model.

Tables 2 and 3 each present various specifications for each of our models. The first specification in each table controls only for the effect of being exposed to the experimental pedagogy on students' choice of major or on the number of upper

division economics course taken. Specification (2) adds controls for section-level differences by including the mean and standard deviation for the final exam and student evaluations. Specification (3) adds a control for the student's performance in the principles of microeconomics course by including the final course grade. Specifications (4) and (5) include additional student-specific controls with the two specifications differing in that specification (5) also allows for differential effects for the experimental treatment across each of our student characteristics. Note that specification (5) is only estimated for the upper division economics courses outcome. Given the dichotomous nature of the economics major dependent variable and the relative lack of variance in this measure (very few economics majors in the sample), we are unable to estimate differential effects of the experimental treatment across student characteristics.

General results

We find no direct statistically significant relationship between the experimental pedagogy and either majoring in economics or the number of upper division economics courses taken. This result stands in contrast to the significant positive effect of the experimental pedagogy on student learning from Emerson and Taylor (2004, 2007). As such, our evidence suggests that although students exposed to the experimental treatment experience higher levels of achievement (in the short term), and anecdotal evidence indicates students enjoy participating in classroom experiments, the experimental pedagogy does not directly affect students' choices of courses and major (in the longer term).

Specification (4) in Tables 2 and 3 allows us to also consider the impact of student-specific characteristics on students' choices of courses and major. The majority of the significant results are in the expected direction. First, we find that a student's grade point average (GPA) is positively correlated with the likelihood that the student selects a major in economics and the number of upper division economics courses in which they enrol. This finding is consistent with the literature that better students (as measured by GPA) are more likely to study economics. Second, we also find some support for gender differentials. Although there is no significant difference in the likelihood of a student majoring in economics based on gender, males are more likely to enrol in upper division economics courses than are females. Third, business majors are more likely to take upper division economics courses than are non-business majors, although they are no more likely to major in economics than non-business majors.[12] Fourth, students who entered the principles of microeconomics course having completed more semester hours were less likely to major in economics. These students were likely to be further along in their degree plans and less likely to change majors regardless of their experience in the course.

Table 2 Economics majors with important student characteristics and interactions

Independent variables	**(1)**	**(2)**	**(3)**	**(4)**
Intercept	–2.485***	22.750	23.402	19.736
	(0.246)	(17.387)	(17.942)	(14.733)
Experimental section	–0.080	0.233	0.288	–0.089
	(0.575)	(0.724)	(0.738)	(0.667)
Course grade			0.265	0.048
			(0.263)	(0.262)
GPA				1.288***
				(0.417)
Principles of Macroeconomics				–1.668
				(1.138)
Male				0.273
				(0.529)
Nonwhite				–0.205
				(0.801)
Number of previous attempts				–0.188
				(1.245)
Work hours per week				0.042
				(0.029)
Semester hours completed				–0.032**
				(0.016)
Current semester hours				–0.057
				(0.116)
Number of standardised absences				0.176***
				(0.065)
High school course in economics				–0.054
				(0.819)
Business student				1.740
				(1.121)
Final exam section mean		–1.222	–1.260	–1.537*
		(0.918)	(0.945)	(0.825)
Final exam section SD		–8.372	–8.559	–8.805**
		(5.825)	(6.051)	(3.988)
Student evaluation section mean		0.193	0.169	0.318
		(0.199)	(0.202)	(0.200)
Student evaluation section SD		1.174	1.172	1.587**
		(0.982)	(1.013)	(0.753)
Observations	290	290	290	280

* significant at 10%; ** significant at 5%; *** significant at 1%
Notes: Robust standard errors are in parentheses

Table 3 Upper division economics courses with important student characteristics and interactions

Independent variables	(1)	(2)	(3)	(4)	(5)
Intercept	–0.543***	8.678	4.687	–2.371	–10.390
	(0.186)	(12.575)	(12.797)	(14.171)	(18.755)
Experimental section	–0.438	–0.360	–0.271	0.108	–20.373
	(0.483)	(0.576)	(0.607)	(0.551)	(16.275)
Course grade			0.227	–0.183	–0.067
			(0.175)	(0.228)	(0.243)
GPA				1.606***	1.579***
				(0.350)	(0.407)
GPA × Experimental section					–0.851
					(2.153)
Principles of Macroeconomics				–2.198***	–1.921***
				(0.614)	(0.655)
Principles of Macroeconomics × Experimental section					–15.818***
					(1.624)
Male				0.767*	0.359
				(0.412)	(0.458)
Male × Experimental section					16.499***
					(1.232)
Non-white				–0.835	–0.380
				(0.523)	(0.590)
Non-white × Experimental section					–15.031***
					(2.001)
Number of previous attempts				0.508	1.267
				(0.712)	(0.824)
Number of previous attempts × Experimental section					–16.996***
					(2.996)
Work hours per week				0.019	0.017
				(0.018)	(0.019)
Work hours per week × Experimental section					–0.162**
					(0.071)
Semester hours completed				–0.014	–0.017*
				(0.010)	(0.010)
Semester hours completed × Experimental section					0.030
					(0.049)
Current semester hours				0.000	0.114
				(0.114)	(0.126)
Current semester hours × Experimental section					–0.645
					(0.494)
Number of standardised absences				0.026	0.060
				(0.048)	(0.055)

Table 3 (continued) Upper division economics courses with important student characteristics and interactions

Independent variables	(1)	(2)	(3)	(4)	(5)
Number of standardised absences × Experimental section					–0.172
					(0.247)
High school course in economics				0.078	–0.132
				(0.460)	(0.473)
High school course in economics × Experimental section					16.795***
					(1.871)
Business student				0.861**	0.995**
				(0.407)	(0.419)
Business student × Experimental section					–0.374
					(2.416)
Final exam section mean		–0.253	–0.027	–0.029	0.441
		(0.755)	(0.762)	(0.735)	(1.183)
Final exam section SD		–4.202	–3.103	–5.273*	–2.721
		(3.954)	(4.056)	(3.197)	(5.268)
Student evaluation section mean		0.054	–0.009	0.180	0.008
		(0.202)	(0.204)	(0.177)	(0.348)
Student evaluation section SD		0.382	0.190	1.109*	0.629
		(0.789)	(0.785)	(0.630)	(0.856)
Observations	290	290	290	280	280

* significant at 10%; ** significant at 5%; *** significant at 1%
Notes: Robust standard errors are in parentheses.

Other findings are not as intuitive. For example, we find that students with more absences in the principles of microeconomics course were more likely to major in economics.[13] We also find that students who had taken principles of macroeconomics prior to the principles of microeconomics course took significantly fewer upper division courses than those who had not already taken macroeconomics.

Differential effect of experiments across student characteristics

Although we find no significant difference across treatments in students' choices of economics as a major or in the number of upper division economics courses in which they enrolled, it is possible that the effect of both teaching methods may be student dependent, a result that would be consistent with learning theory (Fels 1993). Therefore, we estimate the potential differential effect of the experimental approach on a variety of student characteristics. These estimates, reported in specification (5) in Table 3, can provide an indication of which approach may be better at attracting certain types of students to studying economics.

We find several statistically significant differential effects of the experimental pedagogy across student characteristics on the number of upper division economics courses in which students enrol. Males and students who had taken economics in high school who were also in the experimental group enrolled in more upper division economics courses than did their counterparts in the control group. Our previous work (Emerson and Taylor 2004, 2007) demonstrated that the experimental treatment improved the performance of females in comparison to their non-experimental peers. This short-term increase in achievement does not appear to spill-over into longer-run course enrolment decisions. With regard to the high school economics result, other studies show that students with prior knowledge of or exposure to economics are more likely to major in economics (see Ashworth and Evans 2001; Worthington and Higgs 2004). Although our findings do not provide further evidence of this general result, our results do suggest that students exposed to economics at the secondary level who then experience the experimental pedagogy are more likely to enrol in upper-division economics courses than those exposed to the traditional 'chalk-and-talk' method of instruction.

The experimental pedagogy had a negative differential effect across other student characteristics, with some students enrolling in significantly fewer upper division economics courses if they were exposed to the experimental treatment (as compared to an identical student in the control group). These characteristics included minorities, students who had previously attempted taking principles of microeconomics or who had taken principles of macroeconomics, and those who spent more time in the labour market. The reasoning behind these results is not readily evident. Emerson and Taylor (2004) find that at best the experimental treatment has no differential effect on minorities in terms of short-term student achievement and has a negative effect at worst. In light of these findings, it is perhaps not surprising that minorities exposed to the experimental pedagogy were less likely to enrol in upper division economics courses than their non-experimental peers.[14] The other findings may be related to the time requirement associated with the experimental pedagogy. That is, successful employment of the experimental technique is time intensive and this time requirement may dissuade some students from further study in economics – particularly those with greater time constraints (e.g. those devoting more time to the labour market or those who have already spent time taking the course).

Conclusion

In the present investigation we track the majors and upper division economics courses taken by the cohort of 290 students at an American university from the original Emerson and Taylor (2004) study and find no significant difference

between students taught using the experimental and traditional lecture-oriented approaches. Thus, although Emerson and Taylor provide evidence that in the short run those students subject to the experimental treatment realise significantly higher levels of achievement than those in the control group, the impact of the experimental pedagogy does not generally appear to carry over to longer term measures like students' choice of upper division courses or major.

We do find some differential effects of the experimental treatment across student characteristics. Students with exposure to economics at the high school level and males in the experimental group were more likely to persist in studying economics (beyond the first principles course) than their counterparts in the control group. The differential effect on males may be due to a variety of factors. One possibility is that since many of the experiments included activities in a trading pit market setting, it may be the case that males are simply more comfortable with the loud and unstructured trading environment and thus more attracted to economics as a result of their trading experiences. With regard to the high school differential treatment effect, this finding may be the result of the reinforcing effect of students having traditional exposure to economics (in high school) followed by a laboratory or applied environment (in the treatment group). The combined effect may have spurred greater interest in this subset of students who were possibly in a better position to appreciate the experimental pedagogy.

The experimental treatment also differentially impacted the persistence of other groups like minorities and students who spent more time in employment making them less likely to continue to study economics than had they been in a traditional 'chalk-and-talk' class section. In both of these cases, the differential effect may be the result of minority students and those spending more time in employment performing less well with the experimental treatment (Emerson and Taylor, 2004). That is, they may simply be discouraged by their performance and thus reluctant to engage in continued study in economics.

Although our results may be somewhat discouraging to proponents of classroom experiments, the reader should keep several caveats in mind. First, we remind the reader that failing to reject the null hypothesis is not the same as accepting it. As Becker (1997) suggests the failure in the literature to find consistent support for active teaching methodologies may be due to reasons other than the absence of any effect. Second, while we do not find a statistically significant positive effect of the experimental pedagogy on students enrolling in upper division economic courses or majoring in economics, we do not find a negative effect. Thus, although we cannot generally demonstrate that classroom experiments lead to more majors it does not appear to decrease the number of majors either; and in the short term

students experience higher levels of achievement. Third, the experimental approach is largely confined to the principles courses in our curriculum. Students, aware of this fact, may be less influenced by the experimental pedagogy when making longer-run decisions like choice of major and upper-division course enrolments which they know will not employ this pedagogy; a pedagogy that they often find very enjoyable and helpful to developing their understanding of economic concepts. Were experiments incorporated throughout the entire economics curriculum, the effect on students' long-run decisions may be more highly significant. Fourth, we must acknowledge the specificity of our data. We recommend multi-university, multi-country studies of this type be undertaken using a variety of class sizes and research designs that would more readily allow investigators to control for instructor-level effects. Replication and expansion of this line of research would greatly improve our understanding of student major and enrolment decisions – particularly with regard to the role that pedagogy plays in the process.

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Notes

1 Readers should note that the current study investigates the effects of the experimental pedagogy on students' choice of major and enrolments in economics courses using a sample drawn wholly from the United States. To the extent that there are institutional or cultural differences that may affect student behaviour, the results of this study may not be generalisable to other countries and systems.

2 Here we use the term 'upper division' to refer to any courses beyond the principles or introductory level.

3 For a more thorough discussion of the literature see Emerson and Taylor (2004).

4 Participants in the study were all enrolled in principles of microeconomics during the spring of 2002. The students each attended one of nine classes taught by seven different instructors. Each separate class is referred to as a 'section'.

5 Although students were not randomly assigned, they did not know ex ante whether they had enrolled in an experimental section. Emerson and Taylor (2004) described data on 300 students. Of the original 300 students, 290 completed the course (234 in the control and 56 in the experimental group) and were assigned course grades. It is this group that constitutes our usable sample.

6 We refer the reader to Emerson and Taylor (2004) for a more detailed description of the original data collection process.

7 While we also recorded students' minor fields of study where applicable, no students in our sample minored in economics.

8 We were unable to track seven of the students from the original sample due to a student identification number change at the university level in 2004. At the time of this writing, however, none of the remaining students in the study were still enrolled in classes at the university. As a result, we believe that our data captures the essence of the relationships we investigate here.

9 We also collected information on students' math and verbal SAT scores. Because some students in our sample were transfer students who were not required to report

SAT scores to the university, our usable sample is smaller when we include SAT scores, rather than GPA, in the empirical estimation. Qualitative results are unchanged when using SAT scores.

[10] For a more detailed discussion of these issues, see Emerson and Taylor (2004, 2007).

[11] The model test with clustered data is distributed as chi2(k) where k is the number of constraints and d is the number of clusters. The rank of the variance-covariance matrix is at most d. Reserving one degree of freedom for the constant, at most d – 1 constraints can be tested, so k must be less than d. Since we have only 9 clusters, we can test at most 8 constraints when employing the cluster option.

[12] This result is likely due to the fact that students following a bachelor of business administration (BBA) degree plan have 15 required hours of electives – six hours of which must be in a business discipline. Consequently, they are more likely to take an upper division economics course than a student on a bachelor of arts or bachelor of science degree plan who have no such requirement.

[13] We also tested whether attendance might have a non-linear effect, but found no evidence of such an effect in our data.

[14] Students are coded as 'minority' if they self-identify as 'non-white' in that they belong to one of the following groups: Black, Asian or Other. Since Black students are systematically underrepresented in economics, it is theoretically possible that the composition of the 'minority' sub-group in the treatment and control groups differs significantly and that this difference is driving the results. This possibility is not the case in our data, however. The composition of the minority sub-group across the treatment and control groups is not significantly different, nor do the results differ if a control for 'Black' is included in the estimation.

Acknowledgements

The authors gratefully acknowledge financial support from Baylor University. They also thank Susan Armstrong, Kate McGill and the students and instructors who participated in the study at Baylor University for their patience and hard work during the data collection process. This research effort has benefited greatly from the comments of Bill Becker, Jim Cox, Eric Dearing, Steve Green, Debra Israel, Peter Kennedy, Chuck North, John Pisciotta, Todd Swarthout, Mike Watts and WEAI participants. The usual disclaimer applies.

Contact details

Tisha L. N. Emerson (Corresponding author)
Department of Economics, Hankamer School of Business
Baylor University
One Bear Place #98003
Waco, TX 76798-8003

Tel: 254-710-4180
Fax: 254-710-6142
Email: Tisha_Nakao@baylor.edu.

Experiential Learning with Experiments

Henrik Egbert and Vanessa Mertins

Abstract

This paper discusses the implementation of experiential learning techniques in a behavioural economics class. In order to deepen students' understanding of both behavioural economics and the experimental approach to research students in the course developed and conducted variants of economic experiments. We believe that the process of designing and implementing the experiments fostered a better understanding of the material than simply participating in classroom experiments would have done. Students worked in small groups to develop their versions of the experiments. Thus, the complete process promoted genuine active learning by engaging the students both individually and collectively.

Introduction

There is increasing recognition that economic education is improved by greater use of experiential learning methods, i.e. active, student-centred learning opportunities are superior to direct instruction (cf. Hawtrey, 2007; Watts and Becker, 2008). Recently, Hawtrey (2007) presented the results of a survey indicating that university students have a strong preference for experiential learning techniques (cf. also Kolb, 1984). The advantages of experiential learning include higher student motivation and better retention of knowledge compared to traditional lectures.

Hawtrey (2007) discusses various experiential learning methods, among them economic classroom experiments. Classroom experiments are an accepted method of teaching economics at the university level (see, for example, the textbooks of Holt, 2007; O'Sullivan and Sheffrin, 2006). The effects of classroom experiments have been examined for different courses (Durham *et al*., 2007; Dickie, 2006 for introductory microeconomics) and for different personality types (cf. Emerson and Taylor, 2007; Durham *et al*., 2007). Generally speaking, experiments are at least as