Confidentiality is Not Enough: Framing Effects in Student Evaluation of Economics Teaching

Kieron Meagher and Stephen Whelan

Abstract

Contrary to previous research we show lack of anonymity is associated with large positive shifts in student evaluation of teaching. The results are consistent with the simple observation that due to higher expected future earnings economics and business students have more at stake in terms of potential retaliation by an instructor. The observed positive bias is strongest for international students. Our analysis is based on both a comparison of distributions and ordered probit multi-variate regression. These methods overcome the statistical problems associated with previous studies which looked at differences in means for ordinal responses.

JEL classification: A22, C83, C91

1. Introduction

Student evaluation of teaching (SET) is now common in universities. Although the goal of improving teaching quality is natural, the process of evaluation is widely criticised, directly on the basis of reliability and interpretation, and more tangentially because of the way SET enters into hiring and tenure/promotion decisions. Becker and Watts (1999) show that SET was extensively used in promotion, tenure and annual raise decisions in US economics departments. Utilisation of SET is likely to have increased over the intervening decade. A number of studies have also demonstrated that teaching quality (or perceptions of it) is associated with higher salaries (Katz, 1973; Siegfried and White, 1973; Moore, Newman and Turnbull, 1998).

The way in which questions are asked (framing effects), including confidentiality and anonymity, are generally well understood as a potential source of bias in data collection in the social sciences. Presumably, it is for this reason that many universities, including my own, collect SET data anonymously. In the context of SET, confidentiality can be thought of as a situation in which identifiable student evaluations are only revealed to the data collector and not to the teacher being assessed. Anonymity implies that the respondent cannot be identified by either the data collector or the individual being assessed. The practice of anonymity is being eroded by a number of SET practices. For example, many teaching professionals currently advocate the use of more in-depth data collection methods, such as face-to-face discussions/focus groups, which may promise confidentiality to participants but do not guarantee effective anonymity as the source of any response can be identified by the data collector. See for example the discussions on evaluation methods in Harvey (1998).

McPherson and Jewell (2007) find evidence that teaching evaluations vary with student grade expectations, individual characteristics and class characteristics. Higher average grade expectations in a

class are associated with higher SET scores, an outcome interpreted by the authors as 'buying better evaluations'. Similarly, the characteristics of faculty members associated with higher SET scores included tenure status (being tenured), ethnicity (white) and being younger. The type of class taught also has an impact with compulsory, theoretical and single, three-hour lecture per week classes all having a negative effect on teaching evaluations. McPherson and Jewell (2007) conclude that *there are grounds for departments adjusting their evaluations*. Studies by Hammermesh and Parker (2005) and Lawson and Stephenson (2005) find patterns similar to those reported in McPherson and Jewell (2007).

Concerns about the technical validity of SET as a measure of teaching quality and gaming of the evaluation process are extensively discussed in the literature cited above, but are not key to the framing effects analysed in this paper. Two other issues highlighted by the reviewed literature are relevant to framing issues: sample selection bias and prejudiced responses by students. Indeed the possibility of systematic bias in SET raises serious equity issues about its use as a personnel management tool, and prompted the following statement from the Canadian Association of University Teachers:

'...anonymous student evaluations of teachers may serve as vehicles for transmitting popular misconceptions, expectations and prejudices, to the disadvantage of, for example, women and visible minorities' CAUT (2006).

The assertion is that by signing evaluations students will feel more responsibility and will more truthfully evaluate the teaching rather than distorting their evaluations according to their prejudices.

Both the bias and qualitative information collection arguments, and indeed any other proposal that does not guarantee students anonymity, assume that the distortions from non-anonymous responses are negligible. This is not an unreasonable point of view since the seminal papers in the field, Stone, Spool and Rabinowitz (1977) and Feldman's (1979) survey, failed to find clear evidence of significant positive bias introduced from non-anonymity. The main framing issue with anonymity and confidentiality is that students will be concerned about possible negative consequences for themselves, either individually or as a group, from giving low teaching ratings and hence will bias their responses upwards.

There is good reason to re-consider the applicability of previous studies to the contemporary teaching of economics and business students. First, due to the work of Becker and other labour economists, education is now viewed more strongly as an investment by students, than it was in the 1970s, when the seminal research in this area was conducted. This may cause students to be concerned not just with immediate negative retaliation by teachers but also the possible impact it might have on their life time earnings. If students are concerned about the potential impact of poor teaching evaluations on grades then students whose studies lead to the highest income occupations may be more likely to exhibit a positive bias in their SET responses. However Stone, Spool and Rabinowitz (1977, pp. 319–20) investigated bias in an evening class of jurisprudence students who already had jobs and found that although the professor was rated more positively, by students completing non-anonymous evaluations than those completing ratings anonymously, the difference was negligible. Fries and McNinch (2003) have shown that contemporary sociology students produce small but more biased responses than the Stone, Spool and Rabinowitz study, but there has been no study of economics students. We predict that the higher expected incomes of the group of economics and business students we investigate will lead them to exhibit even greater positive bias.

Second, previous studies of anonymity framing affects focus on US universities and do not concern themselves with the differing perspectives of students from international backgrounds. The economic success of a number of developing countries in recent decades has led to major changes in the global patterns and number of international students. There is a substantial literature – see Chan (1999) for an

overview – demonstrating that students from Confucian heritages have learning backgrounds which are substantially more teacher centred than those of Western students (Wong, 2004). This different educational background, plus cultural differences, potentially generate different expectations of Western university education. Chan summarises the issue nicely:

'Chinese learners have been brought up to respect wisdom, knowledge and expertise of parents, teachers and trainers. They have been socialized to respect highly those who provide the knowledge and to avoid challenging those in authority' Chan (1999, p. 298).

Whatever one concludes from the debate over cultural issues in education there is an issue relevant to our study. Specifically, since Confucian heritage students are frequently described as having a greater tendency to echo rather than challenge the opinions of those in authority, it can be hypothesised that they may respond more positively about a teachers pedagogical choice in a situation that they perceive as non-anonymous.

More pragmatically, and more independent of ethnic background, international students in Western universities are typically spending a great deal of their families' resources on an investment which is expected to lead to a higher income.[1] Thus these students may be even more sensitive to any possible negative consequence arising from a lack of anonymity in reporting a SET.

Methodologically the studies of anonymity cited above are unsatisfactorily simplistic in their statistical analysis. All the relevant data is from Likert scales, which by construction are ordinal but not cardinal. However the previous studies have all imposed a cardinal scale and analysed differences in the constructed means – an approach which is seriously flawed from a statistical point of view. We instead focus on differences in distributions caused by anonymity effects. Further more we also offer a multivariate analysis to investigate if other characteristics, especially being a foreign student, play a role in explaining the anonymity effect.

2. Empirical results –Data

In the first teaching session of 2006 we introduced an economic experiment on ultimatum bargaining into our first year undergraduate microeconomics class. We used Charlie Holt's excellent online Veconlab software to run the experiment and everything ran smoothly, as one might expect.[2] It is the student evaluations of this teaching innovation that provides the data for the following analysis.

Following their participation in the online game, students were asked to complete an online survey. The ethics information provided *before* the students completed the questionnaire indicated that the information was being collected solely for the purpose of teaching research by the author of this paper (who was not an instructor for the course), participation was voluntary, *all responses were confidential* and only aggregate results would be made available to the course instructor. No date for the release of the results was given to students.

We collected basic personal data from the students including gender, foreign/domestic status and reason for taking the course. The summary statistics for these personal data variables, which we will refer to as control variables, are given in Table 1. We also asked students about their experience of participating in the experiment. Specifically students were asked whether they 'enjoyed participating in

[1] While foreign students make up 25% of our sample of an economics principles class they are almost entirely absent from introductory humanities classes.

[2] See Meagher and Chan (2007) for a discussion of using the ultimatum bargaining experiment in an international classroom. Charles Holt's homepage and links to relevant games can be found at: http://people.virginia.edu/~cah2k/ Details of the ultimatum bargaining game can be found at this page.

the game'; the 'ease of participating in the game'; the 'clarity of instructions provided' for participating in the game; and the 'relevance of the game to their careers'. Responses were on a standard five-point Likert scale and the full distributions for the experience variables are presented in Figure 1.

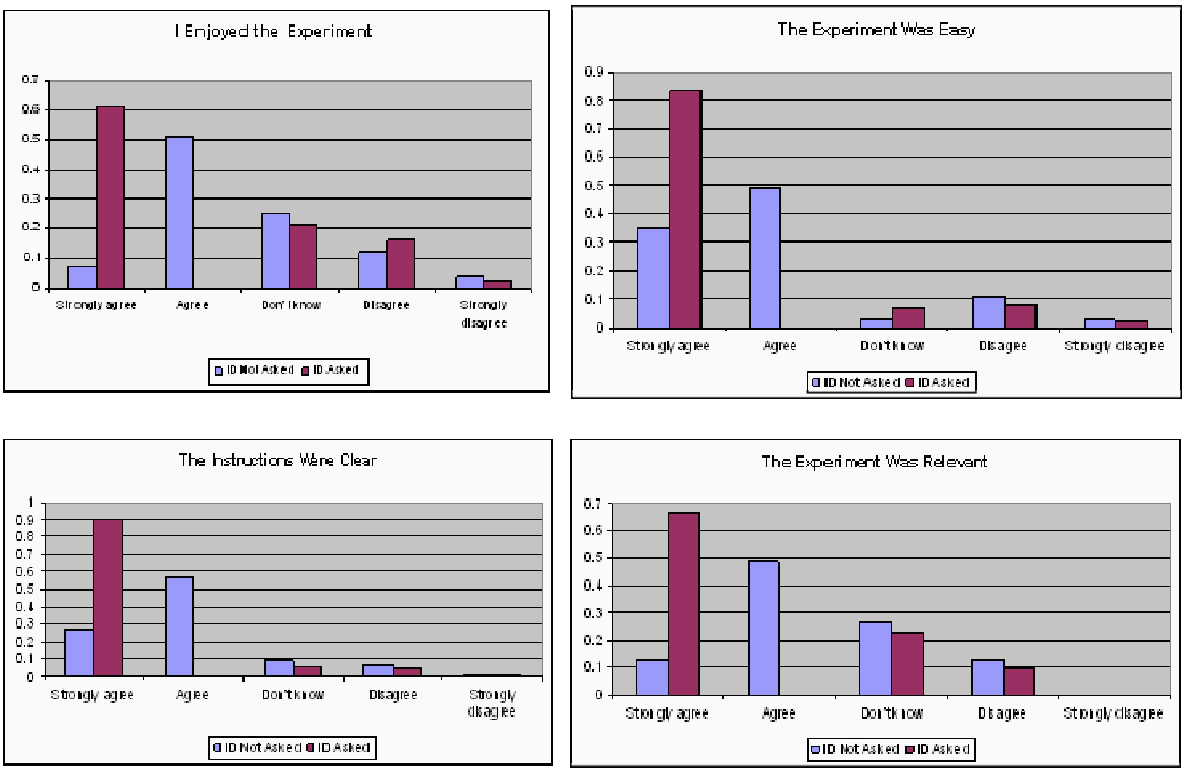
Table 1: Means of control variables (standard deviations in brackets)*

Variable	ID Not Asked	ID Asked	Pooled	Variable Definition
ID asked	0	1	0.551 (0.499)	Student ID requested in the online survey.
Male	0.550 (0.500)	0.439 (0.499)	0.489 (0.501)	Respondent is male.
Local	0.800 (0.403)	0.704 (0.459)	0.747 (0.436)	The respondent is enrolled as a local student (Australian citizenship not required).
Course compulsory	0.663 (0.476)	0.704 (0.459)	0.685 (0.466)	Reason for taking course is compulsory part of degree.
Course relevant to career	0.225 (0.420)	0.163 (0.372)	0.191 (0.394)	Reason for taking the course was relevance to career.
Course of interest	0.088 (0.284)	0.112 (0.317)	0.101 (0.302)	Reason for taking the course was interest.
Declined to give ID when asked	0	0.041 (0.199)	0.022 (0.149)	Student ID was requested in the online survey but the student left the response box blank.
N	80	98	178	

* Means and standard deviations reported to three decimal places.

It was announced that the questionnaire would be available for two weeks to complete. During the first week we did not request student identification numbers (student IDs) in the survey. *Without notification* in the second week we did request student IDs in the online survey. Thus throughout the survey students responses were always confidential but it was technically possible to identify students during the second week of data collection. *The main result of this paper is to show that despite confidentiality student responses were significantly more positive without anonymity.*

Figure 1: Distributions of student responses by question and framing method



It is important to emphasise that students were not briefed that there would be a change in the data collected and thus in particular were not aware that their choice of time to complete the survey would determine whether they would be asked for their student ID. Thus students could not have deliberately selected themselves into one or other of the treatments based on whether they were concerned about being asked for their student ID. Furthermore the exact timing of the shift in treatments was random – depending on when a research assistant (RA) updated the website late on the Thursday morning of the first week. Thus there is no natural focal point for the change in treatment either – the change did not for example occur after a reminder in a lecture. In this sense, the data generating process resembles a natural experiment in which respondents have been randomly assigned to one of two groups depending on when an RA updated the website.

While the online survey tool did not record the time of each response it did record the order of responses and we shall use this in our multi-variate analysis. In section 2.3 we report the results of ordered probit models that include a variable identifying the order in which the student entered their response to the survey. The approach is similar to the regression discontinuity design and exploits the manner in which the subjects in the data were assigned to a treatment based on the time when they participated in the survey (Thistlewaite and Campbell, 1960; Lee 2008).

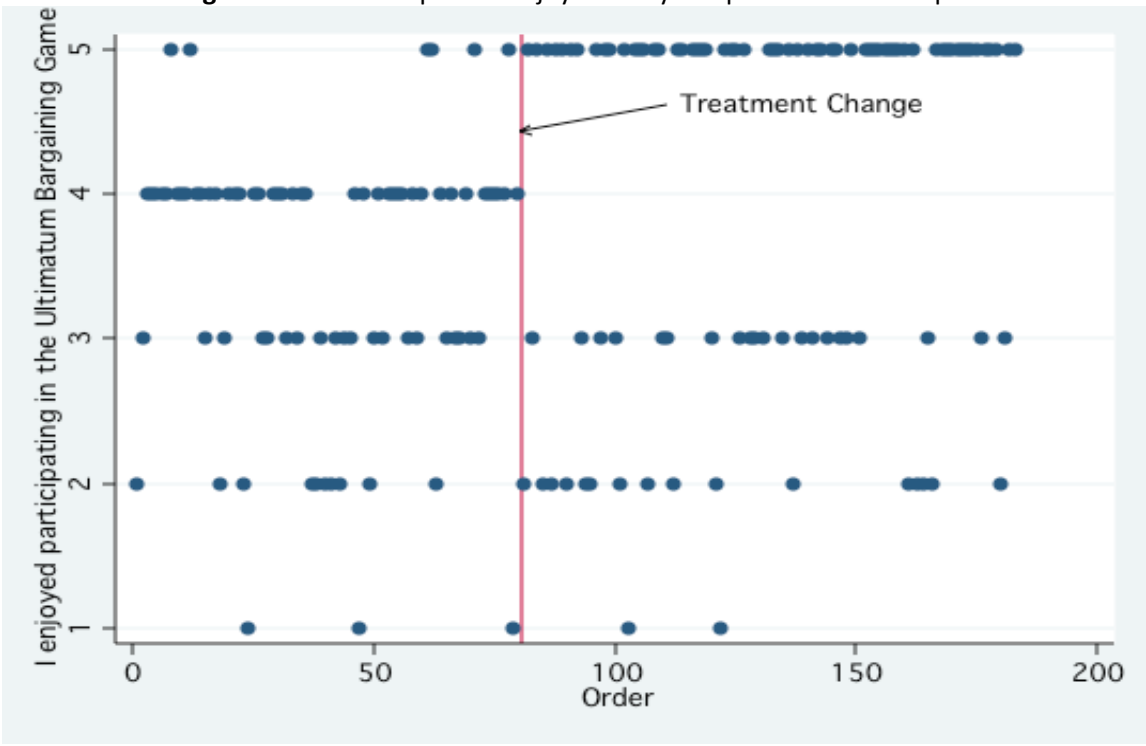
A total of 80 students responded in the first week when student ID was not requested while 103 students responded in the second week of data collection when student ID was requested. There were six students who did not provide ID when requested. Since they fell in the ID asked treatment their responses are included in that category for the construction of the sample distributions. Intuitively we interpret the behaviour of this group of six students as preferring anonymity. This issue is pursued in the ordered probit analysis below through the inclusion of a dummy variable *declined to give ID when asked.* Further, the sample is restricted to the 178 students who answered all the relevant questions rather than the 183 who answered any of the questions. There were 502 students registered for the bargaining game giving a response rate for the survey of approximately 36%. This is a typical response rate for online SET at this university.

3. Quasi random experimental design

A key point in viewing our survey as a regression discontinuity design, is the issue of whether students were able to manipulate the time of their response in order to select a specific treatment (Lee and Lemieux, 2009). This seems unlikely from the way in which the survey was constructed, thus we interpret the effect of being asked for student ID on evaluations as causal. In identifying a treatment effect we are looking for a discontinuity between the responses to the two treatments.

Figure 1 shows an apparent difference between the distributions of responses for the two treatments. Although suggestive this is not sufficient to conclude causality for two reasons: (i) responses might be a function of time independent of the treatment; and (ii) other covariates might vary between the two populations (receiving the two treatments) in some way that affects their responses.

Figure 2: Student reported enjoyment by temporal order of response



Notes: Y-axis labels, 1 = Strongly disagree, 2 = Disagree, 3 = Don't know, 4 = Agree, 5 = Strongly agree.

The scatter plot of responses based on the temporal order in which students responded to the survey in Figure 2 indicates a clear discontinuity in the positive responses (4 and 5) between the two treatments while there is no obvious change in the neutral (3) or negative responses (1 and 2). It appears that students who were inclined to 'agree' (4) that the experiment was enjoyable distorted their response upward to 'strongly agree' (5) when they were asked for their student ID in the second/right-hand half of the plot. The discontinuity is investigated more formally using an ordered probit in the next section.

An important issue when assigning a causal interpretation to differences in the responses of those students asked to complete the survey anonymously and those asked to do so non-anonymously is whether the two groups are in fact different other than for the treatment (that is, whether asked for ID) which is being analysed. The regression discontinuity approach requires that there should not be a discontinuity in the covariates associated with the change in treatments since this would suggest that

students might have been able to manipulate assignment to a treatment. Recall that whether a student was asked his or her ID depended on when the survey was completed. Reassuringly the *t*-tests in Table 2 show there is no statistically significant difference between the means of any of the covariate means between the two treatment groups. Nonetheless, we investigate whether the covariates are important for understanding the student responses by including a variety of control variables in the ordered probit regressions in the next section.

Table 2: t-test of difference in means[1] of control variables across framing[2]

Variable	Difference[3]	Std. Error	Critical Value	p-value[4]
Male	0.111	0.75	1.477	0.141
Local	0.096	0.065	1.465	0.145
Course compulsory	-0.042	0.070	-0.592	0.555
Course relevant to career	0.062	0.060	1.040	0.300
Course of interest	-0.023	0.046	-0.542	0.588

[1] H_0: means equal; H_1: means not equal (two sided test).

[2] All values reported to 3 decimal places.

[3] Difference = mean(ID not asked) – mean(ID asked).

[4] Degrees of freedom = 176.

4. Empirical results

The sample distributions in Figure 1 tell a consistent story: student responses were more positive, on average, to every question when they were also asked to provide their student ID. The variation in the neutral and negative responses is reasonably small, most of the change in the distributions is due to the increased weight on the *strongly agree* category compared to the *agree* category. A series of chi-squared goodness of fit tests, reported in Table 3, confirm that there is a very highly statistically significant difference between the distribution when ID was not asked compared to when ID was asked. Although student responses were confidential and were not supplied to a course instructor there appears to be strong evidence that the framing effect caused by asking for student IDs had a significant effect on responses between the two treatments.

Table 3: Chi-squared goodness of fit test for constant distributions across framing[a]

Question	Test Statistic	1% Critical Value
I enjoyed the experiment	435.41	χ^2(4 df) = 13.28
The experiment was easy	126.87	χ^2(4 df) = 13.28
The instructions were clear	202.66	χ^2(4 df) = 13.28
The experiment was relevant to my studies[b]	284.18	χ^2(3 df) = 11.34

H_0: With ID distribution is the same as the Without ID Distribution

H_1: With ID distribution is different to the Without ID Distribution

Right Tail Test at 1% Significance Level

[a]Expected frequencies are low in some categories but do satisfy the weak criteria of Doane and Seward (2007, p.668). Furthermore the test results on difference in distribution are driven by the *strongly agree* category in which the expected frequencies are high.

[b]For the relevance question only four categories were used in the test because there were no responses in the *strongly disagree* category.

The multi-variate analysis uses an ordered probit framework in which responses to questions about participation in the online game was ordered from strongly disagree (1) to strongly agree (5). That is, a more positive response was characterised by a higher value and don't know was considered neutral and coded as a 3. The actual estimating equation reported in Table 4 is given by:

$$y^* = x'\beta + \varepsilon \qquad (1)$$

Given that y^* is unobserved, what is observed is the responses of individuals (y) where:

$$y = 1 \; if \; y^* \le 0$$
$$y = 2 \; if \; 0 < y^* \le \mu_2$$
$$y = 3 \; if \; \mu_2 < y$$
M
$$y = J \; if \; \mu_{J-1} \le y^*$$

Where the $\mu's$ and the coefficients β are to be estimated (Greene, 2008). All estimations are undertaken in Stata using maximum likelihood. We first allow all the coefficients to vary between the two treatments by estimating two separate regressions for each treatment group. The results are reported in columns (1) and (2) of Table 4. The key result here is that the order variable is statistically insignificant, suggesting that for each of the treatment groups, the time at which individuals completed the survey (proxied by the order in which the survey is completed) had no impact on the responses. One might conjecture, for example, that 'good' students respond early in a positive way or perhaps that students with a negative experience go early because of their strong emotional response or perhaps

later because of a lack of engagement with the process. There is no evidence from these regressions to support these conjectures.

In column (3), we report the results from a model using the complete sample and which includes a dummy variable for the treatment (student *ID asked).* The key result of the paper is that the coefficient of *ID asked* is significant and positive while the coefficient on *order* is insignificant. The marginal effects from the specification reported in column (3) of Table 4 are set out in Table 5. The clear implication of the empirical analysis is that student responses are more positive when students are asked for their ID. In column (4) of Table 4 we include in the specification the *order* variable. Again, the *order* variable is statistically insignificant, while the *ID asked* variable remains marginally significant (p-value of 0.10). Again, this suggests that the non-anonymity associated with the request for student identification led to a more positive response to the question whether the student enjoyed participating in the bargaining game, rather than a continuous change in responses over time.

The multivariate analysis also indicates that students who chose not to supply identifying information, despite being in the non-anonymous treatment, gave more negative responses (columns 2-5 of Table 4). The negative responses of this group give further independent evidence of the relationship between anonymity and bias in the evaluation of teaching.

The second issue explored in this paper relates to differences in the responses of local and non-local students. In specification (4) (column 5 of Table 4) we include an interaction term between *ID asked* and *local* student status (denoted *IDlocal*). The results indicate that the term is negative and statistically significant (p-value of 0.07). The results suggest that local students when asked for identifying information are less likely to respond positively, that is non-local students respond more positively. This result is consistent with the hypothesis about international students identified in Section 1. It is not possible with our data to allocate the differences in responses for international students between economic, cultural or other causes. Nonetheless, the evidence presented in this paper suggests that the large (and in many cases growing) number of international students in Western universities mean that teaching evaluation policies based on research conducted prior to this major demographic shift may need to be re-evaluated.

Table 4: Ordered probit results for regression discontinuity model of student evaluation of enjoyment of experiment (standard errors in parenthesis)

	Coefficient Estimates				
	Model 1		**Model 2**	**Model 3**	**Model 4**
Variable	**No ID**	**ID asked**			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Male	-0.225	-0.313	-0.230	-0.234	-0.275
	(0.261)	(0.265)	(0.169)	(0.169)	(0.171)
Local	0.147	-0.541*	-0.284	-0.279	0.100
	(0.310)	(0.301)	(0.197)	(0.197)	(0.289)
Course compulsory	-0.526	0.722	0.301	0.309	0.386
	(0.821)	(0.741)	(0.520)	(0.520)	(0.522)
Course relevant to career	-0.655	1.094	0.437	0.437	0.514
	(0.853)	(0.808)	(0.545)	(0.545)	(0.548)
Course of interest	-0.663	0.768	0.306	0.301	0.388
	(0.907)	(0.816)	(0.575)	(0.575)	(0.577)
Declined to give ID		-1.531***	-1.573***	-1.553***	-1.575***
		(0.596)	(0.562)	(0.564)	(0.565)
Order	0.002	0.002		0.001	
	(0.006)	(0.004)		(0.003)	
ID asked			0.683***	0.548	1.243***
			(0.170)	(0.336)	(0.354)
ID*local					-0.729*
					(0.402)
N	80	98	178	178	178
Pseudo R^2	0.010	0.07	0.06	0.06	0.06

Table 5: Ordered probit results for student evaluation of enjoyment of experiment (Standard errors in parenthesis)

Coefficient Estimates		**Marginal Effect of a One Unit Change in ID asked**	
Variable	**Coefficient**	**Category**	**Marginal Effect**
Male	-0.230 (0.169)	Strongly agree	0.251*** (0.061)
Local	-0.284 (0.197)	Agree	0.007 (0.014)
Course compulsory	0.301 (0.520)	Don't know	-0.086*** (0.025)
Course relevant to career	0.437 (0.545)	Disagree	-0.133*** (0.037)
Course of interest	0.306 (0.575)	Strongly disagree	-0.038** (0.018)
Declined to give ID	-1.573*** (0.562)		
ID asked	0.683*** (0.170)		
N	178		
Pseudo R^2	0.06		

Marginal effects evaluated at the means of the independent variables. ***, ** indicates significance at the 1 and 5 per cent levels respectively

5. Conclusion

A student evaluation of teaching applied to the use of an ultimatum bargaining experiment in a first year microeconomics principles class was conducted by a third party with strong confidentiality guarantees. Student responses were found to be systematically, significantly more positive when their responses were confidential but not anonymous.

The evidence suggests that contemporary economics and business students, a group for whom the anonymity issue has not been previously investigated, may be more concerned about possible negative outcomes of giving bad SETs than are the groups studied in the existing literature. This result is consistent with the prevailing view of education as an investment and the greater income streams at risk for business and economics students.

The results of this study suggest that anonymity concerns can, contrary to the prevailing wisdom in the education literature, have a large impact on student responses. The positive bias introduced by confidentiality without anonymity suggests that the additional information available through focus groups and other personal interactions may be significantly tainted. Furthermore, the techniques involving third party data collection used in this study do not provide sufficient insulation to remove the bias in student responses. Finally, the differential response of local and non-local students is significant and suggests current beliefs about the use and validity of SET may need to be re-examined in light of

the large and growing number of students from non-Western cultures in Western universities, especially in business and vocational programmes.

References

Becker, W.E. and M. Watts (1999). 'How departments of economics evaluate teaching', *The American Economic Review*, Vol. 89(2), Papers and Proceedings of the 111th Annual Meeting of the American Economic Association (May, 1999), pp. 344–49.

CAUT, (2006). 'Policy on the use of anonymous student questionnaires(1) in the evaluation of teaching', Canadian Association of University Teacher (CAUT), http://www.caut.ca/en/policies/questionnaires.asp (retrieved 22 January 2007).

Chan, S. (1999). 'The Chinese learner – a question of style', *Education & Training*, Vol. 41(6/7), 294–304).

Doane, D.P. and L.E. Seward (2007). *Applied Statistics in Business and Economics*, New York: McGraw-Hill Irwin.

Feldman, K.A. (1979). 'The significance of circumstances for college students' ratings of the teachers and courses', *Research in Higher Education,* Vol. 10(2), pp.149–72.

Fries, C.J. and R.J. McNinch (2003). 'Signed versus unsigned student evaluations of teaching: a comparison', *Teaching Sociology,* Vol. 31(3), pp. 333–44.

Greene, W.H. (2008), *Econometric Analysis*, New Jersey: Pearson.

Hammermesh, D. S. and A. Parker (2004). 'Beauty in the classroom: instructors' pulchritude and putative pedagogical productivity', *Economics of Education Review*, Vol. 24, 369–76.

Harvey, J. (ed.) (1998). *Evaluation Cookbook*, Edinburgh: Learning Technology Dissemination Initiative.

Katz, D.A. (1973). 'Faculty salaries, promotions, and productivity at a large university', *The American Economic Review*, Vol. 63(3), pp. 469–77.

Lawson, R. A. and E. F. Stephenson (2005). 'Easiness, attractiveness and faculty evaluations: evidence from RateMyProfessor.com', *Atlantic Economic Journal*, Vol. 33, 485–6.

Lee, D.S. (2008). 'Randomized experiments from non-random selection in U.S. House Elections', *Journal of Econometrics*, Vol. 142(2), 675–97.

Lee, D. S. and T. Lemieux (2009). 'Regression discontinuity designs in economics', NBER Working Paper 14723, Cambridge, MA.

McPherson, M. A. and R. T. Jewell (2007). 'Leveling the playing field: should student evaluation scores be adjusted?', *Social Science Quarterly*, Vol. 88(3), pp. 868–81.

Meagher, K.J. and A. Chan (2007). 'Online economic experiments: student experiences in an international classroom?', mimeo, University of New South Wales.

Moore, W.J., R. J. Newman and G. K. Turnbull (1998). 'Do academic salaries decline with seniority?', *Journal of Labor Economics*, Vol. 16(2), pp. 352–66.

Siegfried, John J., and White, Kenneth J. "Financial Rewards to Research and Teaching: A Case Study of Academic Economists." American Economic Review 63 (May 1973): 309-15.

Stone, E.F., M.D. Spool and S. Rabinowitz (1977). 'Effects of anonymity and retaliatory potential on student evaluations of faculty performance', *Research in Higher Education,* Vol. 6, pp. 313–25.

Thistlethwaite, D.L. and D. T. Campbell (1960). 'Regression-discontinuity analysis: an alternative to the ex-post facto experiment', *Journal of Educational Psychology*, Vol. 51, pp. 309–17.

Wong, J. (2004). 'Are the learning styles of Asian international students culturally or contextually based?', *International Education Journal,* Vol. 4(4), pp. 154–66.

Author Biography

Kieron Meagher is currently Deputy Director (Research) in the Research School of Economics at the Australian National University. He previously held a position at the University of New South Wales. His main research interests are Organizational Economics, especially hierarchies, and spatial models of oligopoly and political economy. He was educated at the University of Waikato, the Australian National University and Yale University.

Stephen Whelan is a Senior Lecturer in the School of Economics, University of Sydney. Stephen joined the University of Sydney after completing his PhD at the University of British Columbia. Although his primary research interest is in labour economics and the impact of social policy on labour market outcomes, Stephen has taught undergraduate microeconomics for a number of years and has a strong interest in enhancing the learning experience of students.

Contact details

Professor Kieron Meagher
Australian National University
Research School of Economics, H.W. Arndt Building
Canberra, ACT 0200
Australia
Tel: +61-2-61259535
Fax: +61-2-61255124
Email: kieron.meagher@anu.edu.au

Dr Stephen Whelan
University of Sydney
School of Economics H04
University of Sydney 2006 NSW
Australia
Tel: +61-2-9036 9251
Fax: +61-2-9351 4341
Email: stephen.whelan@sydney.edu.au