

DO CLASSROOM EXPERIMENTS INCREASE STUDENT MOTIVATION? A PILOT STUDY

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Abstract:

More and more teachers of economics use classroom experiments as one of their didactic tools. Classroom experiments are short exercises in which students interact in a game-like setting that was designed to facilitate their understanding of key economic ideas. Can classroom experiments be used to boost student interest in economics, promote greater university enrollment and lower dropout rates? This paper presents a design encompassing five measures for motivation based on student behavior, to explore whether the use of classroom experiments in economics courses stimulates students to study economics. It then describes a pilot study devised to apply this design to a sample of Dutch pre-university students. Analysis of the results reveals a positive and significant treatment effect for four of the five measures. Further research applying the design more extensively should reveal whether our findings have general validity.

Key Words: Economic education, classroom experiments, student motivation, situational interest

Introduction

In response to steadily dropping enrollment in undergraduate economics programs, researchers and policymakers have been searching for ways to boost student interest in pursuing studies in economics. Although Edward Chamberlin advocated the use of experiments as a didactic tool already in 1948 when he wrote about a classroom experiment: “it was designed also as a pedagogical experiment; and in my own experience has been found stimulating and instructive to students...” (Chamberlin 1948, 95), more than forty years later, Rendigs Fels had cause to note that hardly any serious research had as yet been carried out into the effects of classroom experiments in economic education (Fels 1993). Economists have made up for lost time since then, however, and several studies carried out in the past two decades have explored how the use of experiments impacts economics education. These studies may be divided into two groups. The first focuses on the *cognitive effects* of classroom experiments.²⁰⁶ This paper belongs to the second, more recent type of research which investigates their *motivational effects*. An inquiry into the motivational effects of alternative instructional methods is important for various reasons. Emerson and Taylor (2010, 44) argue that more attractive educational tools may halt the decline of the market share of economics studies in the United States. Furthermore, other instructional methods may positively affect an individual student’s desire to continue his or her studies (Becker 1997, 1359) and may therefore lower the drop-out rate. Moreover, extra motivation may lead to “enhanced learning” (Ball et al. 2006, 446).

In line with this, the Dutch government recently mandated that pre-university programs in the Netherlands use classroom experiments in teaching economics. This decision was based not only on the expected beneficial effects of experiments on cognition, but also on their assumed motivational effects. This paper presents a design to explore whether economics courses taught with classroom experiments benefit the development of student motivation for economics. It also presents the empirical results of our pilot study, in which we applied this design to 100 pre-university students in the Netherlands.

²⁰⁶ Usually, these studies find a positive learning effect. This is the case, for instance, in the contributions by Frank (1997), Gremmen and Potters (1997), Emerson and Taylor (2004, 2007), Dickie (2006), and Ball et al. (2006). However, some studies, such as Cardell et al. (1996), find no significant effect.

The remainder of this paper is structured as follows. The section after this introduction describes what others have done to measure the impact of classroom experiments on motivation, and how this study builds upon that. The next section depicts the way in which our design measures student motivation for economics. The subsequent one gives the theories we use to identify the factors that may determine this motivation, and how we measure these possible determinants. This section also explains why classroom experiments are most suitable for students with very little background in the subject concerned, and thus why our pilot study focuses on pre-university students. The next section describes the test design and how the questionnaires that we devised can be used to quantify the indicators of both motivation and its determinants. The subsequent section presents the empirical results of our pilot study and our comments. The final section concludes.

How does our design compare to other research on motivation?

Research in this area initially used the opinions of participating students to assess the motivational effects of classroom experiments. Durham et al. (2007) conclude that students who participated in a course with classroom experiments were more positive about this course than were their fellow students who were offered only *chalk-and-talk* lectures. Similarly, Ball et al. (2006) observed that, due to the use of experiments, students found a course more “stimulating” and evaluated their teachers more favorably.

Obviously, there is no harm in experiments giving rise to more favorable student opinions. However, in their comprehensive survey of motivation in education, Schunk, Pintrich, and Meece (2008) define motivation as “the process whereby goal-directed activity is instigated and sustained” (2008, 4). In accordance with this definition, studies exploring the motivational impact of experiments should not be based on student opinions, but rather on student behavior. Schunk et al. also argue that, within that framework, greater motivation can express itself in essentially two ways: (i) demonstrations of more effort and persistence and/or (ii) other choice of tasks (2008, 12). Hence, these two behavior-related expressions of motivation should form the basis of any test devised to detect a possible impact of experiments on motivation.

Emerson and Taylor (2010) were the first to explore behavioral effects of the introduction of classroom experiments. Focusing on the second expression of motivation (choice of tasks), only, they found that students who participated in experiments in a principles course did not choose other follow-up courses than their peers who did not participate in experiments. They suggest, however, that the absence of such a motivational effect could possibly be attributed to the fact that the upper division economics courses contained no experiments and, as a result, could be expected to exert no extra attraction on students.

Just like Emerson and Taylor, we examine how the use of classroom experiments impacts student behavior. However, rather than restricting ourselves to the effect that experiments may have on the second expression of motivation, we also consider the effects that experiments may have on the amount of time students spend on their studies. In other words, we also examine the impact that experiments may have on the first expression of motivation, as given by Schunk et al: (demonstrations of) effort and persistence.

Motivation Theory: Level of motivation

As mentioned above, Schunk et al. give two expressions of motivation: the effort and persistence of students and, secondly, their choice of tasks. Our design quantifies these two expressions using five measures of motivation in total.

For the first expression, effort and persistence, we use two measures that relate to the students’ *current* behavior: the time they spend on their economics homework and the time they spend preparing for their economics test. We assume that if students devote more time to their studies of economics, they are better motivated for this subject.

The second expression, choice of tasks, is quantified here by means of three measures. The first relates to *current* student behavior: the priority that they give to their economics homework. Under the assumption that students have a positive rate of time preference, we regard students who give a higher priority to their economics homework relative to ‘other’ homework (in other words, those who do their economics homework earlier) as students who are better motivated for economics.

Obviously, the rate of time preference of students should be high enough to observe an effect of a possibly increased motivation for economics on this homework sequence.

Measures two and three for the choice of tasks relate to *intended* student behavior: the likelihood that students will choose economics as the subject of their (high school) thesis²⁰⁷ and the likelihood that, once they graduate from high school, they will enroll in economics as their tertiary education. Both an increased likelihood of an economic thesis and an increased likelihood of an economic education reflect an improved motivation for economics.

The above may be summarized as follows:

Expressions of motivation	Measured in our design by the following behaviors
(I) effort and persistence	(i) time spent on economics homework
	(ii) time spent on preparing for an economics test
(II) choice of tasks	(iii) priority given to economics homework
	(iv) likelihood of an economic thesis
	(v) likelihood of an economic study

Motivation Theory: Factors affecting motivation

As to the possible determinants of motivation, Schunk et al. also discuss various motivation theories, each focusing on specific factors that influence motivation. Some theories emphasize factors that affect motivation only in the longer term (a term longer than the duration of our period of investigation). Other theories highlight factors that may have an effect on motivation within a shorter period. We restrict ourselves here to the latter group of theories. After listing these theories, we subsequently explain how we apply them.

Motivation theories relevant to our study are *attribution theory* (asserting that motivation is determined by the perceived causes of events; motivation benefits, for instance, from positive events for which a student credits him/herself), *expectancy-value theory* (stating that motivation is affected by the combination of the value that a student attaches to a task and the expectancy for success), *theories on interest*, and *theories on intrinsic motivation*.

From these four theories we infer for our study six determinants of motivation. The first factor is the student's expectancy for success. This factor follows from attribution theory and expectancy-value theory. A student who is more confident of success will be better motivated. The second determinant of motivation is based on the expectancy-value theory. It resembles the value that a student assigns to the subject of economics: a student who finds this subject more useful will be better motivated. This usefulness may involve the relevance of economics on his or her school career but it may also have to do with how the individual expects to function after graduation. Thirdly, also intrinsic motivation factors, such as curiosity about economic aspects of life, are important to explain the student's motivation. These factors are derived from intrinsic motivation theories. The fourth motivational factor, which emerges from all theories mentioned above, is the teacher. His or her way of teaching and attitude towards the student play a prominent role in the development of the student's motivation. The fifth and sixth factors originate from the theories on interest, where we should distinguish between two types of interest. The first one has to do with interest in the school subject: a student who is more interested in economics will be better motivated than an uninterested student. The second one is referred to as situational interest: a student can suddenly be enthralled by being put into a certain situation.

The latter determinant, an increase in situational interest, may be most at play when students are faced with classroom experiments. These students are put in a certain context that confronts them with one or several dilemmas. This exposes them to situational stimuli. If experiments appear to improve motivation, this effect may therefore be attributed to an increased situational interest.

This use of experiments to generate situational interest is in line with Alexander's *Model of Domain Learning* (2003). This model states that a student, when developing from a beginner in high

²⁰⁷ One and a half years after completion of our period of investigation, the students in our sample would have to choose a subject on which to write their high school thesis. In the Netherlands such a thesis is compulsory.

school to an expert after university graduation, passes three successive stages: acclimation, competence, and proficiency. Alexander argues that students who completed pre-university education show “a movement away from acclimation and toward competence” (Alexander 2003, 12). According to this model, didactic tools aimed at stimulating situational interest (context-rich teaching formats) are best suited for students in the acclimation stage and in the early competence stage. Teaching formats that focus directly on economic concepts are more suitable for students at the end of the competence stage and during the proficiency stage. Consequently, the motivation of pre-university students as well as of freshmen at universities should, *ceteris paribus*, benefit relatively strongly from classroom experiments. This is why we tested the motivational impact of classroom experiments in a group of students with a very limited background in the subject of economics.

Test design

The pilot study we implemented to test our design, compared the development of motivation at two schools. At our request, one school used experiments. We label this school the ‘treatment’ school. The ‘control’ school used no experiments and employed a chalk-and-talk lecture format throughout. The schools used identical textbooks for economics and had the same number of economics classes per week. The 100 students in the pilot study were enrolled at the outset of the final three years of their pre-university education (these are six-year programs in the Netherlands) and all of them followed their first economics course in the fall of 2010. This was the last school year in which the use of experiments was optional. At both schools the course started with a short series of chalk-and-talk lectures which was concluded by a test. Also the subject matter for this examination was the same at the two schools. Subsequently, the students completed a baseline questionnaire (the questions of which are included in the Appendix). This questionnaire asked them, indirectly and concerning the school subject of economics, about their level of motivation as indicated by the five measures mentioned in the previous section. The questions concerning ‘time spent on homework’ and ‘time spent preparing for the test’ had to be answered in terms of minutes. The items on the questionnaire regarding the three other measures of motivation (the priority that students gave to economics homework, the likelihood that they would choose to write their thesis on an economic subject, and the likelihood that they would opt for an economic study after their graduation from high school) were all nine-point Likert statements where 1 = totally disagree and 9 = totally agree.²⁰⁸

Moreover, in order to quantify the factors that influenced motivation, the questionnaire attempted to elicit from students their expectancy for success, their task value, their intrinsic motivation, their interest in the school subject of economics, and the motivational impact of their teacher. These determinants of motivation were also measured using similar nine-point Likert statements.

Simultaneous to the student questionnaire, we asked the teachers how much time their students would need, at that point in time, to adequately do their homework and to sufficiently prepare for the test. We used this information to determine whether students, compared to the standard as indicated by their teacher, spent more or less time on the subject of economics and, therefore, put more or less effort into this subject. We refer to the time specified by the student, minus the time his or her teacher considered to be adequate, as the “net time spent”.

In the two months following this initial questionnaire, the teachers of our treatment school larded their class meetings on welfare economics (the topic that had to be covered in this period at both schools) with three classroom experiments. Since we wanted to analyze whether motivation benefitted from the Dutch law that requires pre-university teachers to use experiments, we chose experiments that many Dutch teachers currently use in this framework. The first experiment dealt with externalities, the second one had to do with public goods, and the last one was an ultimatum game. After each experiment, the students completed assignments to make them link the experiments to the topic of the course. At the control school, the lectures continued to be exclusively chalk-and-talk in nature.

²⁰⁸ Since Cook and Beckman (2009) found that nine-point Likert scales are more accurate than the usual five-point Likert scales while they score equal on interrater reliability, we use nine-point Likert scales.

Upon completion of the third experiment, the students at both schools had a second test. Again, the subject matter for this test was the same at the two schools. Next, the students completed a second questionnaire, the items of which were the same as those of the first one. As we wanted to compare the answers to the two questionnaires to see if motivation changed and why, depending on the dimension of the answers we adjusted the formulation of the items of the second questionnaire. This was not needed for the questions requesting students to state a certain number of minutes, such as time spent on homework. For these items the scores on the two questionnaires could simply be compared in order to see the extent to which motivation of the student concerned had changed. However, other answers were collected using Likert statements, such as the students' expectancy for success. Because of their ordinal nature, in the second questionnaire we could not simply ask for a rating of these items and subsequently deduct the ratings in the first questionnaire to see by how much these variables had changed. Therefore, although the two questionnaires contained the same items, when completing the second one, the students were not asked to rate the ordinal items in absolute terms. Instead, for these variables they were asked to indicate if they agreed more or less with a statement than they did when completing the first questionnaire. In other words, when completing the second questionnaire, students did not report absolute values for these items, but *changes in those values*.

This procedure allowed us to establish whether the students' motivation had changed and why. In other words, by correcting for the variables that, as discussed in the preceding section, could have affected student motivation (expectancy for success, the value attached to the task, interest in the subject, intrinsic motivation factors, and teacher quality) as well, we were able to determine whether a possible change in student motivation could actually be attributed to the introduction of experiments – causing a change in situational interest – or not. This correction for confounding variables²⁰⁹ was only one of several measures we took to promote the internal validity of our test. Another measure involved structuring the questionnaire items such that reverse causation (where expressions of motivation would affect one or more determinants of motivation, rather than the reverse) could not occur. Given our five alternative measures of motivation,²¹⁰ such reverse causation could have occurred between motivation measured as time spent on economics homework or as time spent preparing for economics test, on the one hand, and expectancy for success, on the other: students who worked harder were likely to expect better results on a test. To prevent this problem, we measured “expectancy for success” as the answer to the question: “For the *next* economics test, I expect to get a mark that makes me happy”, while the questions that asked for the time that the students spent were related to the *previous* economics test.²¹¹ As a third measure to promote internal validity, we tried to minimize the possibilities of a Hawthorne effect, both for the teachers who had to use classroom experiments and for their students, by asking the teachers of the treatment group to use three classroom experiments in a short period of time. This minimized the chances that a possible impact on motivation would result from novelty or a change in lecture method. Also, we used the same questionnaires in the treatment and in the control group, as well as the same questionnaire instructions for the teachers of these two groups and for their students. A fourth measure, aimed at preventing threats of social interaction, involved choosing groups whose teachers did not know each other and had no contacts with each other before or during the period of investigation. As a fifth measure, we made sure that the students in the two groups had no idea that they were treated differently from

²⁰⁹ We also corrected for other possibly confounding variables (i.e., for variables that have been identified by other studies as affecting the learning effects of the use of classroom experiments: gender, GPA, and the subjects that the student chose to follow; see, for instance, Durham et al. (2007), Dickie (2006), Emerson and Taylor (2004), Gremmen and Potters (1997) and Cardell et al. (1996)). Since aptitude for economics is more relevant for our study than general aptitude, we replaced GPA by “Relative mark”, which stands for the student's average mark for economics at the time when our period of investigation started minus the corresponding average of the group, expressed as a fraction of that group average. We received the data regarding these three items from the schools involved.

²¹⁰ That is: time spent on economics homework, time spent preparing for economics test, priority given to economics homework, likelihood of an economics thesis, and likelihood of an economics study.

²¹¹ The other specifications of motivation did not run this risk of reverse causation. Note that reverse causation could, for instance, not happen in the case of the time-related expressions of motivation, on the one hand, and the variable “relative mark”, on the other, since we measured the latter at the outset of our period of investigation. Extra study efforts during the period of investigation could therefore not have affected this item.

another group, and we chose schools that were remote from each other. In other words, the chances of diffusion, of compensatory rivalry or of demoralization of the control group were minimal. The teacher of the control group did not plan to use any experiments in the first place, and compensatory equalization of treatment did not occur. Finally, in an effort to prevent a selection bias, we randomly selected the schools for our pilot study and we did not leave the choice to participate in an experiment to the students.

Owing to the limited size of our data set, however, the students of the two groups may, on average, well have differed at the outset of our investigation. Although that appeared to be the case in just a few of the observed characteristics,²¹² we cannot be sure that a selection bias was fully absent and, if not absent, how this affected our empirical results. The results that we find may furthermore have been influenced by unobserved differences between the two groups in terms of other student characteristics, school characteristics, other teacher characteristics than the ones accounted for, these teachers' accuracy when estimating the required study times, or other unobserved differences. Our results can therefore only obtain more general validity if the design we propose will be applied to many more schools, teachers and experiments.

Given the above, using the students' answers to the two questionnaires, we could estimate the following equation:

$$\Delta \text{Motivation}_i = \text{Constant} + \beta_1 \text{Treatment}_i + \beta_2 \text{Gender}_i + \beta_3 \text{Subjects chosen}_i + \beta_4 \text{Relative mark}_i + \beta_5 \Delta \text{Expectancy for Success}_i + \beta_6 \Delta \text{Task value}_i + \beta_7 \Delta \text{Interest}_i + \beta_8 \Delta \text{Intrinsic factors}_i + \beta_9 \Delta \text{Teacher}_i$$

In this equation the index i represents a student. The left-hand side of the equation reflects the change in the five alternative measures of motivation described above. On the right-hand side, next to the constant, we find, first of all, three dummy variables: "treatment" equals 1 if a student is in the treatment group and equals 0 if he or she is in the control group; "gender" equals 1 if a student is male and equals 0 if a student is female; "subjects chosen" equals 1 if the subjects that the student chose are primarily social-oriented, and equals 0 if his or her subjects are primarily science-oriented. Next, we find the variable "relative mark", a measure of the student's economic literacy. The remaining items reflect the reported changes in the factors that determine motivation as described in the previous section: "expectancy for success", "task value", "interest", "intrinsic factors", and "teacher". The Appendix gives the exact ways in which we measured these items.

In keeping with the five alternative measures of motivation we developed (time spent on economics homework, time spent preparing for economics test, priority given to economics homework, likelihood of an economic thesis, and likelihood of an economic study), we ran five regressions. Since the former two measures are interval variables, we could use OLS to estimate these two equations. The final three measures are ordinal variables, which is why we used Ordered Probit to estimate these three equations.

Results of the Pilot Study

Initially, our sample consisted of 100 students: 43 in the treatment group and 57 in the control group. Five students could not complete one or two questionnaires, on account of absence from school. This resulted in a sample of 95 students: 42 in the treatment group and 53 in the control group.

The table below presents the results of the five regressions that we ran to explain the changes in the five alternative measures of motivation.^{213,214} Since we want to know if classroom experiments *increase* motivation, we report in brackets the one-tailed p-values.²¹⁵

²¹² Using a 5% criterion, in the observed variables the two groups only differed in terms of "interest" and, depending on the type of test, also in terms of "net time spent on economics homework" and "net time spent preparing for economics test".

²¹³ The residuals were not homoskedastic when we used the measures "time spent on economics homework" and "priority given to economics homework". Consequently, we estimated these equations using heteroskedasticity-consistent standard errors.

²¹⁴ In both groups most measures of motivation went down during the period of investigation. This may be attributable to the items that were covered during this period, such as welfare versus well-being, tax burden, pension systems, various Dutch laws that redistribute incomes, and internalization of external effects. These items being relatively remote from the

TABLE: REGRESSION RESULTS^a

	Δ Net time spent on economics homework	Δ Net time spent preparing for economics test	Δ Priority given to economics homework	Δ Likelihood economics thesis	Δ Likelihood economics study
Constant	-28.3248 (0.0356)	-43.3836 (0.0834)			
Treatment	15.3863 (0.0374)	34.2683 (0.0371)	0.0554 (0.4003)	0.7734 (0.0007)	0.3850 (0.0460)
Gender	-1.0806 (0.9025)	-0.3723 (0.9853)	-0.1403 (0.5814)	0.4914 (0.0506)	0.4753 (0.0504)
Subjects chosen	18.1967 (0.0352)	20.6169 (0.3252)	0.1475 (0.5223)	0.5313 (0.0400)	0.6204 (0.0139)
Relative mark	-9.8384 (0.3458)	35.9834 (0.2044)	0.3843 (0.2548)	0.7394 (0.0819)	0.5765 (0.1350)
Δ Expectancy for success	-2.7136 (0.2308)	-11.1880 (0.0608)	-0.0652 (0.2021)	-0.1483 (0.0477)	-0.0442 (0.3027)
Δ Task value	-6.6089 (0.0953)	-0.3375 (0.4869)	-0.0596 (0.3290)	-0.0526 (0.3397)	0.2101 (0.0438)
Δ Interest	3.0880 (0.2273)	15.8549 (0.0515)	0.2600 (0.0207)	0.3873 (0.0009)	0.2538 (0.0141)
Δ Intrinsic factors	1.3694 (0.3043)	-7.1353 (0.1684)	0.1054 (0.1930)	0.0820 (0.1837)	-0.0679 (0.2208)
Δ Teacher	3.6382 (0.2166)	29.1902 (0.0014)	0.3045 (0.0060)	0.3438 (0.0022)	0.0673 (0.2763)
Method	OLS	OLS	Ordered Probit	Ordered Probit	Ordered Probit
N	95	95	95	95	95
(pseudo-) R ²	0.1167	0.1559	0.0389	0.0940	0.0567

^a The specifications of the respective variables follow from the Appendix. “Δ Net time” (the net time reported in the second questionnaire minus the net time reported in the first questionnaire) is defined in minutes. All other variables preceded by a “Δ” result from Likert statements ranging from 1 to 9. For interpretational purposes, these items are rescaled in order for them to range from -4 to +4. As an example, the answer “2” regarding “change in interest” was rescaled to -3 in order to show that interest fell. “Gender” equals 1 for male and 0 for female. “Subjects chosen” equals 1 for primarily social-oriented subjects and 0 for primarily science-oriented subjects. “Relative mark” may range from -1 to +1. In brackets: one-tailed p-values, except for the items *gender* and *subjects chosen* and for the constant term.

The first two columns of the table give the results regarding the first expression of motivation as mentioned by Schunk et al.: “effort and persistence”. In both cases, the impact of the treatment is positive and significant at a 5% level. Owing to the treatment, students spent more time on their homework and to prepare for their test. In other words, the treatment raised motivation, as indicated by our measures of effort and persistence. The coefficients of “treatment” in these two equations imply that, owing to the treatment, students spent about 15 minutes extra per week on their homework and slightly more than half an hour extra preparing for their economics test.

perception of environment of a typical high school student, Alexander's *Model of Domain Learning* (2003) would expect them to be less appealing to the students concerned.

²¹⁵ Except for gender, subjects chosen and the constant term: since the theoretical signs are ambiguous here, for these items the table reports two-tailed p-values.

It should be noted, however, that we measured “net time spent on economics homework” and “net time spent preparing for economics test” as the time spent by students minus the time that their teachers deemed adequate. Therefore, these results also depend on the estimations of the latter by the teachers. Since the teachers might have been mistaken, a generalization of this conclusion requires a follow-up study with many more schools and teachers.

The final three columns of the table present the results that relate to the second expression of motivation that Schunk et al. give: “choice of tasks”.²¹⁶ As far as the treatment dummy is concerned, the most striking result here is its high significance level when explaining “change in likelihood of an economic thesis”. The treatment is also positive and significant at a 5% level when explaining “change in likelihood of an economic study”. However, for the third measure of “choice of tasks” (that is, for “change in priority given to economics homework”), we find a positive but insignificant impact of the treatment.

Two remarks should be made here. First, the latter finding -no significant impact of “treatment” on “priority given to economics homework”- might indicate that experiments do not increase motivation. However, it is also possible that students like the subject of economics better, but do not do their economics homework earlier since their rate of time preference is too low to significantly change their homework sequence.

Second, while the treatment dummy is significant at a 1% level when explaining “change in likelihood of economic thesis”, it is significant at a 5% level when explaining “change in likelihood of economic study”. This difference may well be explained on the basis of the limited implications of the first choice when compared to those of the second. The choice of the thesis subject will be primarily based on motivation, since that choice has virtually no implications for the student’s future career. However, a student’s choice between one academic study and another will not only depend on motivation, but will also be affected by factors such as future employment opportunities and future income.

Conclusion

In economics classes, the use of classroom experiments has become increasingly popular. In the recent past, most research on the impact of this use of experiments focused on cognitive performance. More recent research also studies the impact that these experiments may have on student motivation. Such research is important, since it sheds light, for instance, on the question whether the use of experiments could support the inflow of students at universities and lower the drop-out rate.

We formulated a design to analyze the impact on motivation by looking at students’ behavior. Rather than confining ourselves, as Emerson and Taylor (2010) did, to the impact that the use of experiments may have on (study-related) choices that students make, we chose to pursue the line set out in the pioneering work on motivation in education by Schunk, Pintrich and Meece (2008). Our design therefore also considers the impact that this use may have on student efforts and persistence.

Next, we applied this design as a pilot study to a sample of pre-university students in the Netherlands. This allowed us to analyze whether the introduction of three experiments that are popular amongst Dutch teachers made these students increase their efforts for the school subject of economics and/or raised the likelihood that they would study economic topics in the future. Our design uses five measures to quantify the two expressions of motivation identified by Schunk et al.. In our pilot study we found that the introduction of experiments appeared to support both expressions of motivation in the sense that we found a positive and significant treatment effect for four out of these five measures of motivation.

This finding not only supports the increasing popularity of classroom experiments in economics education, but also is in line with the theory by Alexander (2003) that states that beginning students should be given a context that fits in with their perception of the environment.

However, three notes should be added here. First, although our design contains a series of measures to promote internal validity, we can never be sure whether the impact on motivation was indeed the result of the use of classroom experiments or perhaps of some unobserved factor. Second,

²¹⁶ For reasons indicated before, these final three equations were estimated using Ordered Probit. Robustness checks in the form of OLS regressions confirmed our results.

the external validity of our empirical findings is still an open question. The findings may change if other classroom experiments are used, by other students, by other teachers or in different settings. Since the issue (how to promote student motivation for economics) is important enough, we would be happy if colleagues elsewhere would replicate our test in other settings. Finally, further study is needed to disclose whether students who faced experiments will actually choose economics more often as their field of study.

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Appendix: Questionnaire items

In the questionnaires, we used the following (translated from Dutch) formulations to quantify our measures and determinants of motivation. In questions 3 to 10: 1 = totally disagree, 9 = totally agree.

Measures of motivation

1. *Question to measure "time spent on economics homework"*:

In weeks without an economics test, how many minutes did you spend, on average per week, on your economics homework assignments? (In your estimate, please include both the time you spent at home and the time you spent in class doing these assignments.)

2. *Question to measure "time spent preparing for economics test"*:

Aside from your regular homework assignments, how many minutes did you spend preparing for your previous economics test?

3. *Statement to quantify "priority given to economics homework":*

When I have no tests to prepare for, but just have homework assignments for economics and for some other subjects, I will do the homework assignments for economics as one of the first few subjects.

4. *Statement to quantify "likelihood of an economics thesis":*

For my thesis, I intend to choose a subject related to economics.

5. *Statement to quantify "likelihood of an economics study":*

I intend to pursue a study in economics after graduating from high school.

Determinants of motivation

6. *Statement to quantify "expectancy for success":*

For the next economics test, I expect to get a mark that makes me happy.

7. *Statement to quantify "task value":*

In economics classes, I learn things that I consider useful, both now and in the future.

8. *Statement to quantify "interest":*

In economics classes, I encounter interesting issues.

9. *Statement to quantify "intrinsic factors":*

Also outside of school I sometimes think about economic issues.

10. *Statement to quantify "teacher":*

My economics teacher shows me that (s)he thinks my achievements and my behavior in class are important.