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Happiness and Productivity

Andrew J. Oswald, Eugenio Proto, and Daniel Sgroi
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Andrew J. Oswald*, Eugenio Proto**, and Daniel Sgroi**

*University of Warwick and IZA Bonn
**University of Warwick

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Address: Department of Economics, University of Warwick, Coventry CV4 7AL, United Kingdom.
Telephone: (+44) 02476 523510
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Happiness and Productivity

Abstract

Some firms say they care about the happiness and 'well-being' of their employees. But are such claims hype? Or might they be scientific good sense? This study provides evidence that happiness makes people more productive. First, we examine fundamental real-world shocks (bereavement and family illness) imposed by Nature. We show that lower happiness is associated with lower productivity. Second, within the laboratory, we design two randomized controlled trials. Some individuals are deliberately made happier, while those in a control group are not. The treated individuals have 10-12% greater productivity than those in the control group. These complementary kinds of evidence, with their different strengths and weaknesses, point to a consistent pattern. They suggest that happiness raises human performance.

At Google, we know that health, family and wellbeing are an important aspect of Googlers' lives. We have also noticed that employees who are happy ... demonstrate increased motivation ... [We] ... work to ensure that Google is... an emotionally healthy place to work. Lara Harding, People Programs Manager, Google.

Supporting our people must begin at the most fundamental level – their physical and mental health and well-being. It is only from strong foundations that they can handle ... complex issues.
Matthew Thomas, Manager – Employee Relations, Ernst and Young.

Quotes from the report Healthy People = Healthy Profits Source:
http://www.dwp.gov.uk/docs/hwwb-healthy-people-healthy-profits.pdf

1. Introduction

There is a large literature on productivity at the personal and plant level (for example, Caves 1974, Lazear 1981, Ichniowski and Shaw 1999, Siebert and Zubanov 2010, Segal 2012). There is a growing one on the measurement of human well-being (for example, Easterlin 2003, Van Praag and Ferrer-I-Carbonell 2004, Layard 2006, Ifcher and Zarghamee 2011, Benjamin et al. 2012). Yet economists and management scientists still know relatively little about the causal links between these two variables.

This study examines a question of interest to a range of social scientists, corporations, and policy-makers. Does 'happiness' make people more productive? We provide evidence consistent with the view that it does. We demonstrate this in a piece-rate setting[1] with otherwise well-understood properties. In a series of three experiments, we exploit data on major real-life (un)happiness shocks and also experimentally 'assign' happiness in the laboratory.[2] The combination allows us to consider the distinction[3] between long-term happiness and short-term positive affect. We concentrate on graphical demonstrations of the study's results (in Figures 1 to 6). This is because our key points can be made with elementary t-tests. The appendix lays out, in a more formal way, a supplementary set of regression results.

Seminal research by the late Alice Isen has dominated this field. The closest previous paper to our own is arguably Erez and Isen (2002). The authors wish to assess the impact of positive affect on motivation. In their experiment, 97 subjects -- half of them mood-manipulated by the gift of a candy bag-- are asked to solve 9 anagrams (three of which are unsolvable) and are rewarded with the chance of a lottery prize. This framework can be seen as a special kind of piece-rate set-up. The subjects who receive the candy solve more anagrams.

There are three main differences with our contribution. First, Erez and Isen (2002) does not examine the long-run effect of shocks (it is based on shocks generated within the laboratory); second, the authors do not collect longitudinal happiness data; third, the focus of their paper is on motivation and persistence. In later work, Isen and Reeve (2005) show that positive well-being induces subjects to change their allocation of time towards more interesting tasks, and that, despite this, the subjects

[1] Such as Niederle and Vesterlund (2007).
[2] The relevance of this effect is witnessed by a widely advertised business-press literature suggesting that employee happiness is a common goal in firms, with the expectation that happier people are more productive. The economics literature has largely ignored this idea.
[3] A distinction emphasized in Lyubomirsky et al. (2005).

retain similar levels of performance in the less interesting tasks. More generally, it is now known that positive well-being can influence the capacities of choice and innovative content.[4] Such results emerge from unpaid experimental settings.[5]

Conceptually, our work relates to Dickinson (1999), who provides evidences that an increase of a piece-rate wage can decrease hours but increase labor intensity, and to Banerjee and Mullainathan (2008), who consider a model where labor intensity depends on outside worries and this generates non-linear dynamics between wealth and effort. Recent work by Segal (2012) also distinguishes between two underlying elements of motivation. Gneezy and Rustichini (2000) show that an increase in monetary compensation raises performance, but that offering no monetary compensation can be better than offering some.[6] Such writings reflect an increasing interest among economists in how to reconcile external incentives with intrinsic forces such as self-motivation.[7]

We draw upon empirical ideas and methods used in sources such as Kirchsteiger, Rigotti and Rustichini (2006) and Ifcher and Zarghamee (2011). Our paper lends theoretical support to concepts emphasized by Kimball and Willis (2006) and Benjamin et al. (2012). A key idea is that happiness may be an argument of the utility function rather than solely a proxy for it.[8] Like Oswald and Wu (2010), later results are consistent with the view that there is real informational content in well-being data.

2. A Series of Experiments

Three kinds of experiment were done. In total, 565 subjects took part.[9] The experimental instructions, a GMAT-style math test, and the questionnaires are explained in the appendix.

2a. Design

Our experiments varied in design. Here we list the main features upon which we draw. In different experiments, we chose different combinations of the following features:

Feature 1: An initial questionnaire when the person arrived in the laboratory. This asked: *How would you rate your happiness at the moment? Please use a 7-point scale where 1 is completely sad, 2 is very sad, 3 is sad, 4 is neither happy nor sad, 5 is fairly happy, 6 is very happy and 7 is completely happy.*

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[4] A body of related empirical research by psychologists has existed for some years. We list a number of them in the paper's references; these include Argyle (1989), Ashby et al. (1999), and Isen (2000). See also Amabile et al. (2005). The work of Wright and Staw (1998) examines the connections between worker well-being and supervisors' ratings of workers. The authors find mixed results. Our study also links to ideas in the broaden-and-build approach of Frederickson and Joiner (2002) and to material examined in Lyubomirsky et al. (2005).

[5] See also Baker et al. (1997), Estrada et al. (1997), Forgas (1989), Jundt and Hinsz (2001), Kavanagh (1987), Melton (1995), Patterson et al. (2004), Sanna et al. (1996), Sinclair and Mark (1995), Steele and Aronson (1995), Tsai et al. (2007), and Zelenski et al. (2008).

[6] See also Benabou and Tirole (2003) who examine the relationship between both types of motivation formally.

[7] Diener et al. (1999) reviews the links between choices and emotional states.

[8] A considerable literature in economics has studied happiness and wellbeing as a dependent variable – including Blanchflower and Oswald (2004), Clark et al. (2008), Di Tella et al. (2001), Frey and Stutzer (2002), Luttmer (2005), Senik (2004), Powdthavee (2010), and Winkelmann and Winkelmann (1998). See Freeman (1978) and Pugno and Depedri (2009) on job satisfaction and work performance. Other relevant work includes Compte and Postlewaite (2004).

[9] All were students, as is common in the literature.

Feature 2: A mood-induction procedure that changed the person's happiness. This was used in Experiments B and C. The treatment was a 10-minute clip of sketches enacted by a well-known comedian.[10] As a control, we used either a calm "placebo" clip or no clip.[11]

Feature 3: A mid-experiment questionnaire. This asked the person's happiness immediately after the movie clip.

Feature 4: A task designed to measure productivity. We borrowed ours from Niederle and Vesterlund (2007). The subjects were asked to answer correctly as many different additions of five 2-digit numbers as possible in 10 minutes. This task is simple but is taxing under pressure. We think of it as representing -- admittedly in a stylized way -- an iconic white-collar job: both intellectual ability and effort are rewarded. The laboratory subjects were allowed to use pen and paper, but not a calculator or similar. Each subject had a randomly designed sequence of these arithmetical questions and was paid at a rate of £0.25 per correct answer. Numerical additions were undertaken directly through a protected Excel spreadsheet, with a typical example as in Legend 1.

31	56	14	44	87

Legend 1: *Adding 2-digit Numbers under Timed Pressure*

Feature 5. A short GMAT-style math test. This had 5 questions along similar lines to that of Gneezy and Rustichini (2000). Subjects had 5 minutes to complete this and were paid at a rate of £0.50 per correct answer. To help to disentangle effort from ability, we used this test to measure underlying ability.[12]

Feature 6. A final questionnaire. This took two possible forms. It was either (a) a last happiness report of the exact same wording as in the first questionnaire and further demographic questions or (b) the same as (a) plus a number of questions designed to reveal any bad life event(s) (henceforth BLE) that had taken place in the last 5 years for the subject. Crucially, we requested information about these life events at the end of the experiment. This was to ensure that the questions would not, through a priming effect, influence reported happiness measures taken earlier in the experiment. The final questionnaire included a measure of prior exposure to mathematics and school exam performance, which we could also use as controls to supplement the GMAT results from Feature 5.

The precise elements in each experimental session differed depending upon the specific aim. They can be grouped into three:

- "Experiment A" on serious real-life happiness shocks from the real-world;
- "Experiment B" on short-run happiness shocks induced within the laboratory;

[10] The questionnaire results indicate that the clip was generally found to be entertaining and had a direct impact on reported happiness levels. We also have direct evidence that the clip raised happiness through a comparison of questionnaire happiness reports directly before and after the clip.

[11] See James Gross's resources site (http://www-psych.stanford.edu/~psyphy/movs/computer_graphic.mov) for the clip we used as a placebo.

[12] We deliberately kept the number of GMAT MATH-style questions low. This was to try to remove any effort component from the task so as to keep it a cleaner measure of raw ability: 5 questions in 5 minutes is a relatively generous amount of time for an IQ-based test, and casual observation indicated that subjects did not have any difficulty giving some answers to the GMAT MATH-style questions, often well within the 5-minute deadline.

- "Experiment C" which asked happiness questions throughout a lab experiment.

We randomly assigned subjects to different treatments. No subject took part in more than a single experiment; individuals were told that the tasks would be completed anonymously; they were asked to refrain from communication with each other; they were told not to use electronic devices for assistance. Subjects were told in advance that there would be a show-up fee (of £5) and the likely range of bonus (performance-related) payments (typically up to a further £20 for the hour's work). Following the economist's tradition, a reason to pay subjects more for correct answers was to emphasize they would benefit from higher performance. We wished to avoid the idea that they might be paying back effort -- as in a kind of 'reciprocity' effect -- to investigators. That concern is not relevant in Experiment A because productivity was measured before the question on bad life events.

2b. Experiment A: Family Tragedies as Real-life Happiness Shocks

For ethical reasons, it is not feasible in experiments to induce huge changes in the happiness of people's lives. Nevertheless, it is possible to exploit data on the naturally occurring shocks of life. This is where we begin. In Experiment A we study real-life unhappiness shocks assigned by Nature rather than by us. These shocks -- for which we use the generic term Bad Life Events -- are family tragedies such as recent bereavement.

The design here was to use a short questionnaire asking for people's happiness, then to initiate the productivity task, then to have the GMAT-style math test, and then to finish with a questionnaire. Hence we used features 1, 4, 5 and 6(b|) from the earlier Features list. One aspect is particularly important. We asked subjects to report their level of happiness at the start of the experimental session.

We informed the subjects of the precise payment system prior to features 4 and 5 (amounts £0.25 and £0.50 per correct answer, respectively). The final questionnaire included supplementary questions designed to find out whether they had experienced at least one of the following BLEs: close family bereavement, extended family bereavement, serious life-threatening illness in the close family, and/or parental divorce. Although we did not know it when we designed our project, the idea of examining such events has also been followed in interesting work on CEOs by Bennedsen et al. (2010), who suggest that company performance may be impeded by traumatic family events.

All the laboratory subjects were young men and women who attend an elite English university with required entry grades amongst the highest in the country. Compared to any random slice of an adult population, they are thus -- usefully for our experiment -- rather homogenous individuals. Those among them who have experienced family illness or bereavement are, to the outside observer, approximately indistinguishable from the others.

In the empirical work, we define a bad life event BLE to be either bereavement or illness in the family.[13] The data suggested that it was appropriate to aggregate these happiness-shock events by using a single variable, BLE.

There were 8 sessions across two days. More detail on the dates and times is provided in the appendix. Table A2 summarizes the means and standard deviations of the variables.

2c. Experiment B: Mood Induction and Short-run Happiness Shocks

In Experiment B, we use a short-run happiness shock (a comedy clip) within the laboratory (feature 2 in the earlier list). The control-group individuals were not present in the same room with the treated subjects; they never overheard laughter or had any other interaction. The experiment was carried out with deliberate alternation of the early and late afternoon slots. This was to avoid time-of-day effects.

Here we use features 2, 4, 5 and 6(a) from the Features list.[14] The final questionnaire inquired into both the happiness level of subjects (before and after the clip for treatment 1), and their level of mathematical expertise. In day 5 and day 6, we added extra questions (as detailed in the appendix) to the final questionnaire. These were a check designed to inquire into subjects' motivations and their own perceptions of what was happening to them. The core sessions took place over 4 days. We then added 4 more sessions in two additional days designed to check for the robustness of the central result to the introduction of an explicit payment and a placebo film (shown to the otherwise untreated group). More detail on the dates and times can be found in Table B1.

Subjects received £0.25 per correct answer on the arithmetic task and £0.50 on each correct GMAT-style math answer, and this was rounded up to avoid the need to give them large numbers of coins as payment.

We used two different forms of wording:

- For days 1-4 we did not specify exact details of payments, although we communicated clearly to the subjects that the payment did depend heavily on performance.
- For days 5-6 the subjects were told the explicit rate of pay both for the numerical additions (£0.25 per correct answer) and GMAT-style math questions (£0.50 per correct answer).

This achieved several things. First, in the latter case we have a revealed-payment setup, which is a proxy for many real-world piece-rate contracts (days 5-6), and in the former we mimic those situations in real life where workers do not have a contract where they know the precise return from each productive action they take (days 1-4). Second, this difference provides the opportunity to check that the wording of the payment method does not have a significant effect -- thereby making one set of days a robustness check on the other.

[13] In the questionnaire, we also asked about parental divorce, but it turned out to have a small, ambiguous effect on the individual.
[14] In this experiment, we choose not to measure the happiness level at the beginning; this is to avoid the possibility that subjects treated with the comedian clip could guess the nature of the experiment.

2c. Experiment C: Before-and-After Happiness Measurements in the Laboratory

On the suggestion of referees, we constructed one further experiment. In this, which we denote Experiment C, we asked happiness questions repeatedly throughout the course of the experiment. This was done to assess what was happening longitudinally to individuals.

We asked subjects' happiness on three occasions. The first measurement was at the start of the experiment. The second was immediately after the comedy or placebo film. The third time was at the end of the experiment. Experiment C used explicit payment instructions and a placebo clip (without a placebo clip there would have been no gap between Features 1 and 3 for the control subjects). The timeline was thus features 1, 2, 3, 4, 5 and 6(a) from the earlier list. The appendix provides further details.

3. Empirical Results

Our results point to the existence of a positive association between human happiness and human productivity.

Figures 1 and 2 illustrate Experiment A. In this experiment, we think of Nature as allocating extreme 'unhappiness' shocks. The sample size is 179; the mean of productivity in the sample is 18.40 with a standard deviation of 6.71. Those subjects who have recently been through a bad life event are noticeably less happy and less productive. Compared to the control group, they mark themselves nearly half a point lower on the happiness scale, and they achieve approximately 2 fewer correct additions. As shown in the final column of Figure 1, they also make fewer attempts. These are noticeable differences when compared to individuals in the no-BLE group. The effects in Figures 1 and 2 are statistically significant in the full samples; they are also statistically significant in the majority of the subsamples. It is not possible to reject the null hypothesis that the effects are of the same size for males and females. Further results are laid out in the appendix. Regression equations are given there in which a variety of covariates are included. The conclusions remain the same. There is also the result that family tragedies that happened longer ago seem to have smaller consequences for current happiness and productivity.

It is possible to think of objections to Experiment A. One is that the happiness shock is assigned by Nature rather than us. This means that it is not necessarily randomly distributed across the sample. For example, those families most prone to bad life shocks such as bereavement could, in principle, also be ones where unhappiness is intrinsically more common and where productivity is intrinsically lower. This criticism is perhaps likely to have less force among a group of elite university students than in a cross-section of the population, but it is nevertheless a potential weakness of Experiment A. Hence the association in Figures 1 and 2 could be real in a statistical sense but illusory in a causal sense. A second difficulty is that it is not possible in Experiment A to be certain that lower happiness causes the lower productivity. Both might be triggered by the existence of the Bad Life Event. A third difficulty is that, strictly speaking, the experiment demonstrates that unhappiness is associated with lower productivity in the additions task. It does not show the reverse, namely, that a boost to happiness promotes a boost in productivity.

To deal with these, we run experiments in the laboratory in which we raise the happiness of certain randomly selected individuals and then compare the outcomes with those of a control group. In Experiment B, 276 subjects participated. A breakdown of the numbers per day and per session is contained in the appendix. Our productivity variable in the analysis is again the number of correct additions in the allotted ten minutes. It has a mean of 17.91. Our key independent variable is whether or not a person observed the happiness movie clip.[15]

The findings are illustrated in Figure 3. Here the treated group's mean performance in Experiment B is higher by 1.71 additions than the performance of the control group. This productivity difference is approximately ten percent. It is significantly different from zero (p=0.04). Both male and female groups have a similar increment in their productivity (1.90 additions for males, 1.78 for females). One sub-group was noticeable in the data. Encouragingly for our account, the performance of those 16 subjects in the treated group who did not report an increase in happiness is not statistically different from the performance of the untreated group (p=0.67). The increase in performance thus seems to be linked to the rise in happiness rather than merely to the fact of watching a movie clip. However, we return to this issue later and examine it more systematically. The results in Figure 3 draw on simple t-tests. In the appendix there are further robustness tests, using regression equations, to check both the inclusion of a placebo clip and explicit payment.

A possible concern -- raised by referees -- in Experiment B is that we are unable to observe in real time the happiness levels of individuals before and after the comedy movie clip. To deal with this, we constructed Experiment C. A group of 52 males and 52 females participated. Differently from the past experiment, we ask happiness questions before playing the movie clip, and then longitudinally. The appendix describes the data.

The chief finding in Experiment C is illustrated in Figures 4 and 5. Individuals exposed to the comedy clip made 22.96 correct additions; those in the control group, who watched only a calm placebo film, scored 18.81. This difference is significant (p-value< 0.01) and is found in both genders although is larger among men. The number of attempts made is significantly higher among the individuals treated with the comedy clip (p-value = 0.018). In contrast to experiment B, in this experiment the precision is slightly higher among the individuals treated with the comedy clip, 0.88, than in individuals treated with the placebo, 0.83. This difference is statistically significant (p-value 0.03). As we argued above, this effect might be due to the happiness question being asked at the beginning. In the appendix we present a formal regression showing the determinants of subjects'

[15] Our movie clip is successful in increasing the happiness levels of subjects. The subjects report an average rise of almost one point on the scale of 1 to 7. Moreover, comparing the ex-post happiness of the treated subjects with that of the non-treated subjects, we observe that the average of the former is higher by 0.85 points. Using a two-sided t-test, this difference is statistically significant (p <0.01). Finally, it is useful to notice that the reported level of happiness before the clip for the treated group is not statistically significantly different (the difference is just 0.13) from the happiness of the untreated group (p = 0.20 on the difference).

productivity for this experiment.

Is it really extra happiness that causes the enhanced productivity? The design of experiment C allows us to check. It asks the happiness question three times.

Because people are randomly assigned to the treatment group in Experiment C, we know that the baseline levels of productivity of the treatment and control group are identical. It is therefore possible to find out for these subjects whether there is link between their measured rise in happiness and their measured productivity. A formal test is given in Table 1. Here we have to instrument the change in happiness, because that change is endogenous. Under the null hypothesis, the treatment dummy variable is an appropriate instrumental variable. Table 1 shows that the change in happiness -- here between the start and middle of the experiment -- is positive and statistically significant in an equation for the number of correct additions. The key coefficient is 8.89 with a t-statistic of 3.67. This implies that a (large) one-point rise in happiness would be associated with almost 9 extra correct answers in the productivity task. The lower half of Table 1 reveals that, as might be expected, the comedy-clip treatment does make subjects happier.

We can get an alternative sense of the longitudinal patterns in Experiment C by dropping the instrumental-variables approach. The scatter plot in Figure 6 reveals that those subjects with the largest increase in happiness -- this time measured not from the start to the middle but from the start to the end -- have the largest improvement in productivity. This claim is open to the criticism, unlike Table 1, that there is no correction for endogeneity or for covariates. Nevertheless, Figure 6 is evocative. This is despite the fact that, superficially, the y axis in Figure 6 is the level of productivity not its change. The reason that the y-axis is in effect a change variable is that treatment and control groups start from the same underlying distribution. Therefore, Figure 6 is further evidence that a rise in happiness is associated with a rise in productivity.

4. Conclusions

This study finds evidence of a causal link between human happiness and human productivity. To our knowledge, it is the first such evidence -- though we would like to acknowledge the work of the late Alice Isen who was a pioneer in this field -- for a true piece-rate setting. We believe ours is the first in such a framework to exploit information on tragic family life events as a ‘natural’ experiment.

Three kinds of trial (denoted Experiments A to C) have been described. The first estimates the repercussions of major life-events assigned by Nature. This design has the disadvantage that we cannot directly control the happiness shock, but it has the advantage that it allows us to study large shocks of a fundamental kind in real human beings’ lives. The second and third experiments examine the consequences of randomly-assigned happiness. These experiments have the advantage that we can directly control the happiness shock but the disadvantage that shocks are inevitably small and of a special kind in the laboratory. It is conceivable in the first experiment that there is some unobservable feature of people that makes them both less productive and more likely to report a bad life event. Yet such a mechanism cannot explain the results in the other two experiments. By design, the three Experiments A, B, and C have complementary strengths and weaknesses. We have not, in this study, attempted to discriminate between different theoretical explanations for the key result, but Tsai et al. (2007) and Hermalin and Isen (2008) discuss potential mechanisms, and the results of Killingsworth and Gilbert (2010) suggest the possibility that unhappiness may lead to a lack of mental concentration. [16]

Various implications emerge. First, it seems that economists and other social scientists will need to pay more attention to emotional well-being as a causal force. Second, better bridges may be required between currently disparate scholarly disciplines. Third, if happiness in a workplace carries with it a return in productivity, the paper's findings may have consequences for firms' promotion policies[17], and may be relevant for managers and human resources specialists. Finally, if well-being boosts people's performance at work, that raises the possibility of self-sustaining spirals between human productivity and human well-being.

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[16] An early version of this paper, available on request, included a mathematical model in which people's attention is subconsciously distracted by unhappiness.

[17] Over and above so-called neoclassical pay-effort mechanisms discussed in sources such as Lazear (1981) and Oswald (1984).

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FIGURES

Figure 1: Individuals with a recent Bad Life Event (BLE) have lower productivity in Experiment A [Here a bad life event is bereavement or family illness.]

[95% confidence intervals]

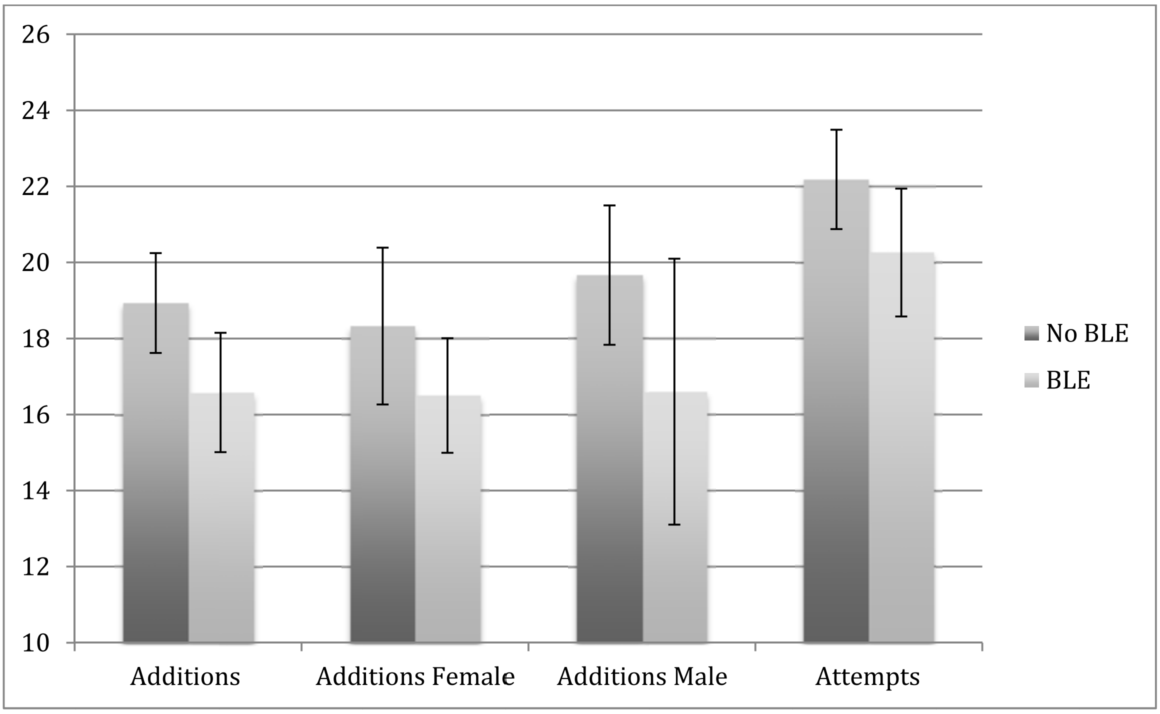

Figure 2: Individuals with a recent Bad Life Event (BLE) report lower happiness in Experiment A

[95% confidence intervals]

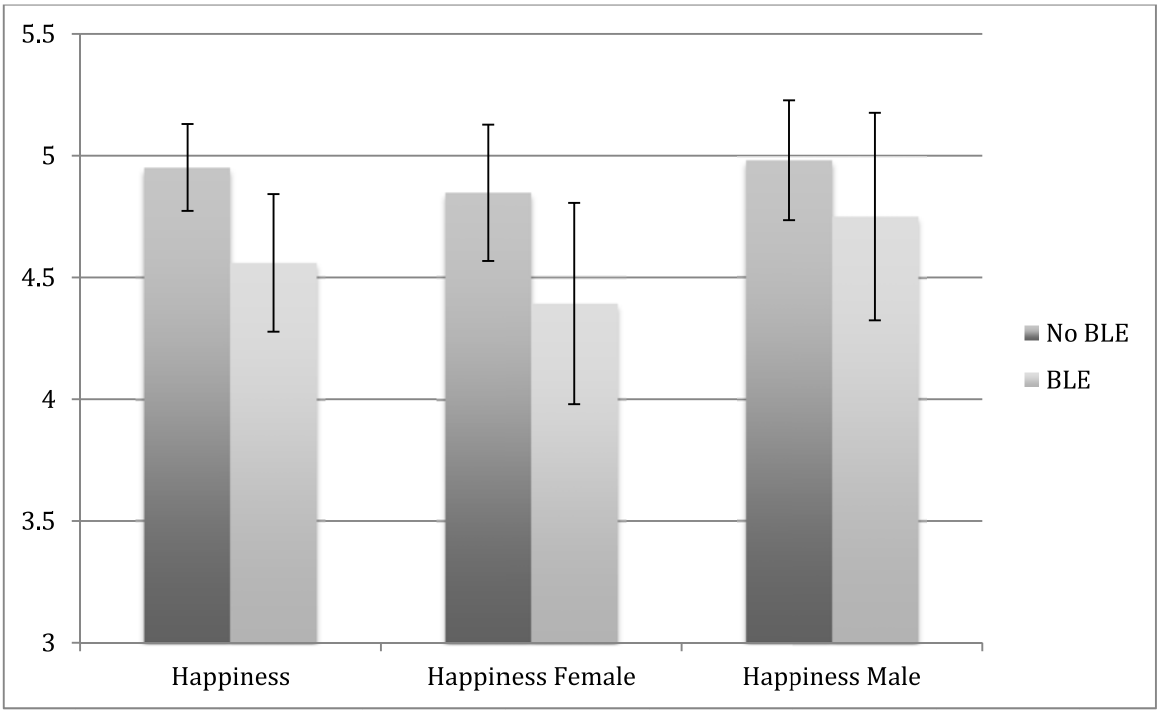

Figure 3: Those exposed to the randomized happiness treatment in the laboratory have higher productivity in Experiment B [Here the happiness treatment is a comedy movie clip in the laboratory.]

[95% confidence intervals]

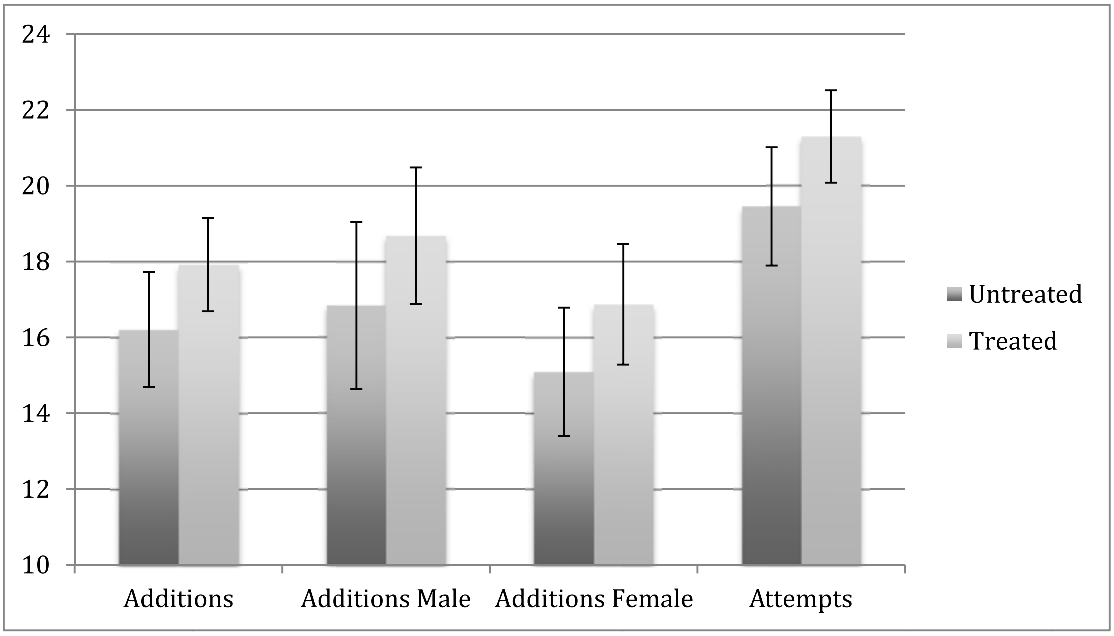

Figure 4: Those exposed to the randomized happiness treatment in the laboratory have higher productivity in Experiment C ***[95% confidence intervals]***

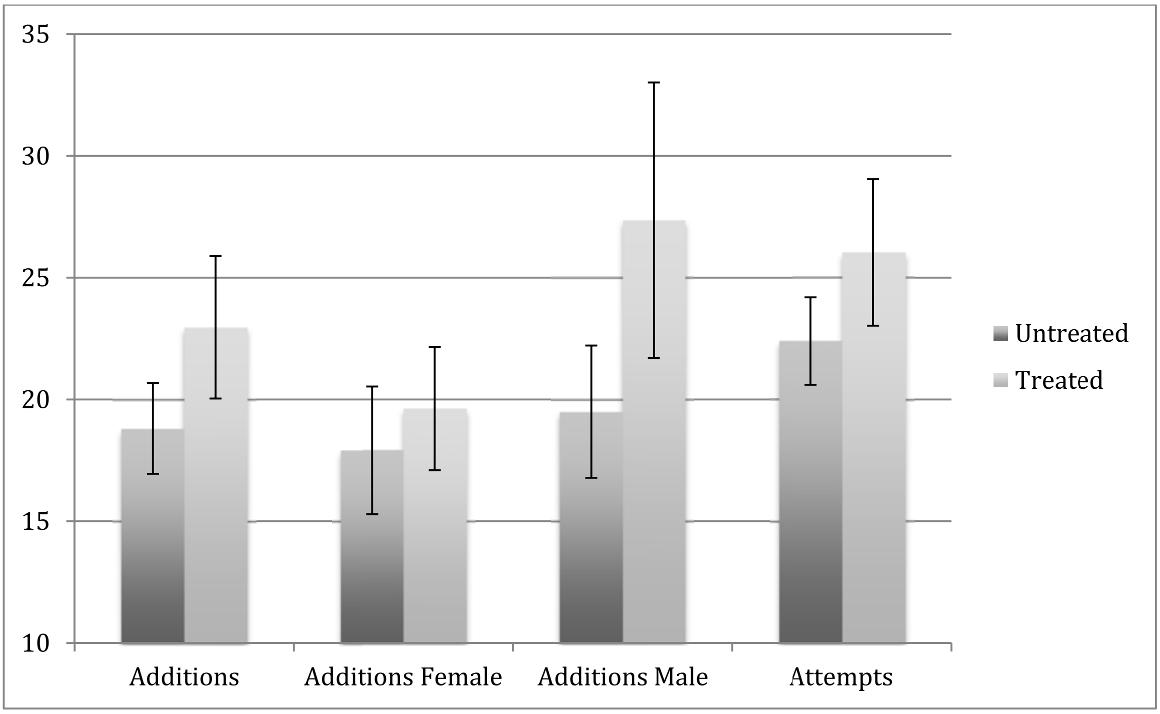

Figure 5: Those exposed to the randomized happiness treatment in the laboratory have higher happiness in Experiment C ***[95% confidence intervals]***

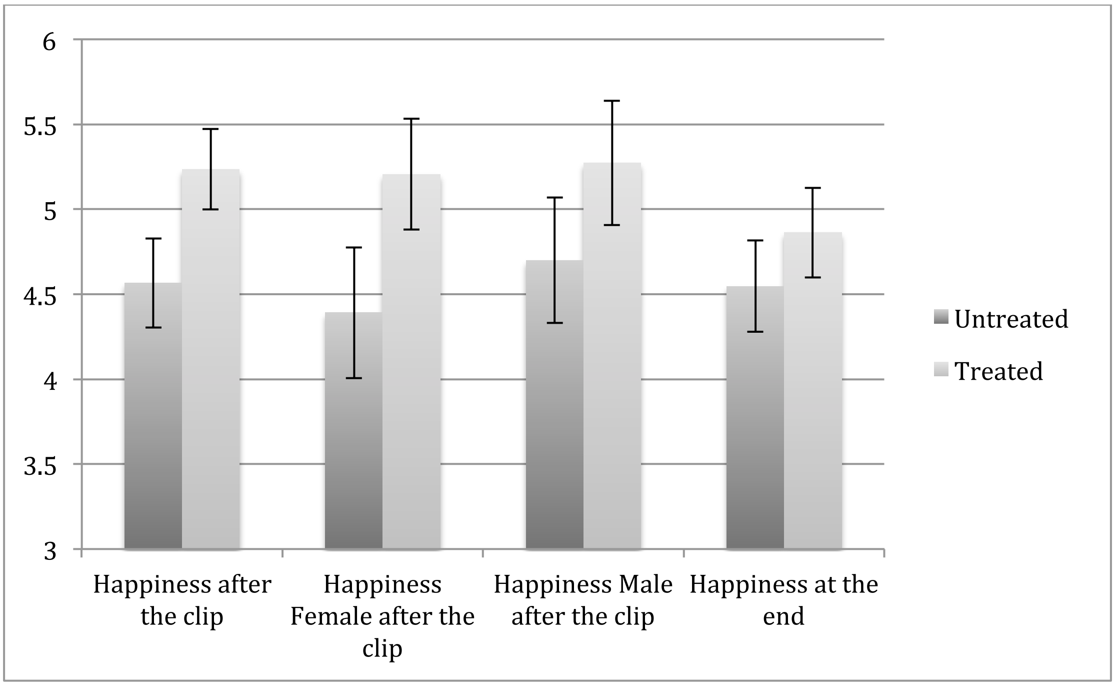

Figure 6: Those who have the greatest rise in happiness during Experiment C have the greatest productivity gain [Here those not exposed to the happiness treatment have the same baseline productivity; hence the y axis can be viewed as a change in productivity from the common baseline.]

TABLES

Table 1: Productivity with instrumented happiness in Experiment C

Second Stage	Additions
Change in Happiness	8.89**
	(3.67)
Happiness Before the Clip	1.68
	(1.56)
Male	1.34
	(1.60)
Age	-0.040
	(0.33)
High School Grades	3.52
GMAT	2.49***
	(0.74)
Day Dummies	Yes

First Stage	Change in Happiness
Treatment dummy	0.57***
	(0.13)
Happiness before the clip	-0.27***
	(0.08)
Male	0.22
	(1.133)
Age	0.02
High School Grades	-.03
	(0.25)
Day Dummies	Yes
Constant	0.75
Observations	100

Note: The change in happiness is that between the start of the experiment and the middle of the experiment (that is, after the happiness treatment but before the additions productivity task). It is instrumented here with a treatment 0-1 dummy.

APPENDIX

The purpose of this appendix is to give more details on the data, to provide supplementary regression equations, and to provide some evidence of hedonic 'adaptation' to shocks.

EXPERIMENT A

Table A1: Experiment 1 Dates

Day/Session	Date	Time	Subjects
1/1	21 October 2009	2.30-3.30pm	19
1/2	21 October 2009	4.00-5.00pm	25
1/3	21 October 2009	2.30-3.30pm	25
1/4	21 October 2009	4.00-5.00pm	23
2/1	25 November 2009	2.30-3.30pm	19
2/2	25 November 2009	4.00-5.00pm	19
2/3	25 November 2009	2.30-3.30pm	26
2/4	25 November 2009	4.00-5.00pm	24

Table A2: Data description in Experiment A (where Bad Life Event is family illness or bereavement)

Variable	#Observations	Mean	Std Error	Min	Max
#Correct Additions	179	18.40	6.71	1	47
Happiness	179	4.82	0.95	2	7
GMAT MATH	179	3.63	1.46	0	5
High School Grades	164	0.57	0.25	0	1
No Bad Life Event	179	0.7	0.46	0	1
Bad Life Event less than 1 year ago	154	0.06	0. 23	0	1
Bad Life Event 1 year ago	154	0.19	0.23	0	1
Bad Life Event 2 year ago	154	0.06	0.23	0	1
Bad Life Event 3 year ago	154	0.05	0.22	0	1
Bad Life Event 4 year ago	154	0.08	0.26	0	1
Bad Life Event 5 year ago	154	0.08	0.25	0	1
Male	170	0.5	0.5	0	1
Age	169	19.49	1.48	18	30

Table A3, below, reports a regression equation for Experiment A. It gives the statistical impact of a BLE bad life event in each year from year 0 to year 5 (as declared at the end of the experiment) upon the individuals' levels of happiness (as declared at the beginning of the experiment). At -0.55 in the upper left-hand column, the estimate is large and negative. Therefore, although our subjects may not be aware of it, their happiness answers at the start of the experiment are correlated with whether later they report that a BLE recently occurred in their family. The pattern in the coefficients is consistent with hedonic adaptation -- an effect that is declining through time. The consequence of a bad life event is empirically strong if it happened less than a year ago, and becomes insignificantly different from zero after approximately 3 years. These experimental results are consistent with a range of adaptation findings in the survey-based research literature on the economics of human well-being (e.g. Clark et al. 2008).

Table A3: Happiness equations in Experiment A with a variable for a Bad Life Event (BLE) defined as family illness or bereavement (Ordered Probit)

VARIABLES	(1) Happiness	(2) Happiness	(3) Happiness	(4) Happiness
BLE in the last 2 years	-0.55***			
	(0.21)			
BLE in the last 2 years^		-0.54***		
		(0.20)		
BLE in the last 5 years			-0.47**	
			(0.19)	
BLE less than 1year ago				-1.09**
				(0.42)
BLE 1 year ago				-0.41
				(0.25)
BLE 2 years ago				-1.14***
				(0.40)
BLE 3 years ago				0.19
				(0.42)
BLE 4 years ago				-0.42
				(0.37)
BLE 5 years ago				-0.57
				(0.42)
Male	0.20	0.35*	0.18	0.23
	(0.21)	(0.19)	(0.21)	(0.22)
Age	-0.12	-0.088	-0.12	-0.13
	(0.080)	(0.061)	(0.080)	(0.081)
High School Grades	-0.043	0.016	-0.032	-0.12
	(0.38)	(0.36)	(0.38)	(0.38)
GMAT	-0.099	-0.12*	-0.087	-0.10
	(0.070)	(0.065)	(0.070)	(0.071)
Session Dummies	Yes	Yes	Yes	Yes
Observations	142	164	142	142

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses. "BLE in the last 2 years^" is a variable set equal to 1 when a bad life event happened in the last two years and set equal to 0 when no bad life event happened or the year is missing.

We are especially interested in the effects of a bad life event upon human performance. The first regression in Table A4 gives the impact of BLE on productivity. Having had a bad life event in the previous two years is associated with lower performance on the additions task. The size of the productivity effect is large; it is more than two additions and thus greater than 10%; the effect is significant at the five per cent level. As can be seen from column 2 in Table A4, the effect of the elapsed-years variable is as might be expected from the results on hedonic adaptation. The extent of the deleterious effect of a Bad Life Event upon subjects' productivity is a declining function of the elapsed time since the event.

Table A4: Productivity in Experiment A where Bad Life Event (BLE) is defined as family illness or bereavement

VARIABLES	(1) Additions	(2) Additions	(3) Additions	(4) Additions
BLE in the last 2 years	-2.31**			
	(1.12)			
BLE in the last 2 years^		-2.05**		
		(1.04)		
BLE in the last 5 years			-0.73	
			(1.04)	
BLE less than 1 year ago				-3.81*
				(2.25)
BLE 1 year ago				-0.50
				(1.35)
BLE 2 years ago				-2.64
				(2.13)
BLE 3 years ago				-0.52
				(2.25)
BLE 4 years ago				4.97**
				(1.98)
BLE 5 years ago				-1.20
				(2.22)
Male	-0.77	-0.83	-0.72	-0.77
	(1.15)	(1.03)	(1.16)	(1.15)
Age	0.30	-0.14	0.26	0.18
	(0.44)	(0.33)	(0.44)	(0.43)
High School Grades	3.75*	3.20*	3.73*	3.87*
	(2.04)	(1.92)	(2.07)	(2.03)
GMAT	1.22***	0.98**	1.27***	1.09***
	(0.38)	(0.35)	(0.39)	(0.38)
Session Dummies	Yes	Yes	Yes	Yes
Constant	5.79	15.22**	6.22	7.70
	(9.04)	(7.15)	(9.18)	(9.02)
Observations	142	164	142	142
R-squared	0.218	0.143	0.195	0.266

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses. "BLE in the last 2 years^" is a variable set equal to 1 when a bad life event happened in the last two years and set equal to 0 when no bad life event happened or the year is missing.

EXPERIMENT B

Table B1: Experiment B Dates

Experiment 2 was at first carried out on four separate days, as follows:

Day/Session	Treatment	Date	Time
1	Treatment 0	21 May 2008	2.30-3.30pm
1	Treatment 1	21 May 2008	4.00-5.00pm
2	Treatment 0	18 June 2008	2.30-3.30pm
2	Treatment 1	18 June 2008	4.00-5.00pm
3	Treatment 1	10 October 2008	2.30-3.30pm
3	Treatment 0	10 October 2008	4.00-5.00pm
4	Treatment 1	15 October 2008	2.30-3.30pm
4	Treatment 0	15 October 2008	4.00-5.00pm
Extra sessions			
5	Treatment Placebo	3 December 2008	2.30-3.30pm
5	Treatment 1	3 December 2008	4.00-5.00pm
6	Treatment 0	14 January 2009	2.30-3.30pm
6	Treatment 1	14 January 2009	4.00-5.00pm

Table B2: Summary Statistics by Treatment

Day\Session	Subjects	Additions	Attempts	Payment
1 Treatment 0	24	15.38	17.67	Implicit
1 Treatment 1	24	18.21	21.33	Implicit
Treat 1-Treat 0		*1.18*	*3.66*	
(Ha: diff > 0)		(0.0476)	(0.0126)	
2 Treatment 0	23	16.85	20.92	Implicit
2 Treatment 1	20	16.48	19.39	Implicit
Treat 1-Treat 0		-0.37	0.81	
(Ha: diff > 0)		(0.5669)	(0.6393)	
3 Treatment 0	23	16.26	19.73	Implicit
3 Treatment 1	24	19.52	23	Implicit
Treat 1-Treat 0		3.26	3.26	
(Ha: diff > 0)		(0.0521)	(0.0513)	
4 Treatment 0	24	16.04	20.36	Implicit
4 Treatment 1	25	17.72	21.45	Implicit
Treat 1-Treat 0		1.68	1.09	
(Ha: diff > 0)		(0.3109)	(0.3018)	
5 Treatment Placebo	25	14.84	17.8	Implicit
5 Treatment 1	25	19.8	23.8	Explicit
6 Treatment 0	23	18.52	20.90	Explicit
6 Treatment 1	21	19	22.26	Explicit
Treat 1-Treat 0		0.90	1.78	
(Ha: diff > 0)		(0.3426)	(0.2003)	

Table B3: Data description (Experiment B)

Treated individuals					
Variable	#Observations	Mean	SD	Min	Max
#Correct Additions	94	17.91	5.99	7	39
GMAT	94	3.48	1.39	0	5
High School Grades	93	0.50	0.27	0	1
Enjoyment-of-Clip	94	5.93	0.68	5	7

Non-treated individuals[18]					
Variable	#Observations	Mean	SD	Min	Max
#Correct Additions	88	16.20	7.16	2	43
GMAT	88	3.36	1.37	1	5
High School Grades	85	0.48	0.24	0	1

Individuals treated with placebo clip[19]					
Variable	#Observations	Mean	SD	Min	Max
#Correct Additions	25	14.84	6.43	5	34
GMAT	25	3.08	1.63	0	5
High School Grades	24	0.47	0.23	0.06	0.93
Enjoyment-of-Clip	24	3.67	1.27	1	6

Treated individuals (precise-payment case)					
Variable	#Observations	Mean	SD	Min	Max
#Correct Additions	48	19.41	8.88	0	42
GMAT	48	3.54	1.30	0	5
High School Grades	47	0.48	0.24	0.06	1
Enjoyment-of-Clip	48	5.81	1.04	2	7

Non-treated individuals (precise-payment)					
Variable	#Observations	Mean	SD	Min	Max
#Correct Additions	21	18.52	7.08	7	34
GMAT	21	3.38	1.60	0	5
High School Grades	20	0.58	0.25	0.14	1

[18] The measure called "High School Grades" asks students to consider all of their qualifications and gives a percentage of those qualifications that are at the highest possible grade. It therefore measures their past performance against the highest possible performance.

[19] More precisely, on the questionnaire we asked two questions: *"How many school level qualifications have you taken (including GCSEs, A-levels and equivalent)?"* (forming the denominator) and *"How many of these qualifications were at the best grade possible? (e.g. A* in GCSE, A is A-level, etc.)"* (forming the numerator).

The next tables, Table B4 and B5, give regression equations in which Additions and Attempts are the dependent variables.

Table B4: Productivity in Experiment B

VARIABLES	(1) Additions	(2) Additions	(3) Attempts
Treatment Dummy	1.41*	1.75**	1.69**
	(0.83)	(0.80)	(0.82)
Explicit Payment	2.71**	2.72**	2.85**
	(1.24)	(1.19)	(1.22)
Placebo Clip	0.012	0.10	0.45
	(1.66)	(1.59)	(1.63)
Male	1.58*	1.31	1.35
	(0.85)	(0.82)	(0.84)
High School Grades	7.82***	7.82***	7.78***
	(1.64)	(1.57)	(1.61)
GMAT	1.33***	1.32***	1.37***
	(0.31)	(0.30)	(0.31)
Day Dummies	Yes	Yes	Yes
Constant	6.06***	6.15***	8.66***
	(1.56)	(1.51)	(1.54)
Observations	269	267	269
R-squared	0.248	0.265	0.258

Notes: In equation 2, the best and the worst performances have been dropped. *** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses.

Table B5: Checking the robustness of the results to an explicit payment in Experiment B

VARIABLES	(1) Additions	(2) Attempts
Treatment Dummy	2.01**	2.21**
	(0.98)	(0.98)
Treat.*Exp.Payment	-1.12	-0.43
	(2.09)	(2.08)
Explicit Payment	2.62	1.82
	(1.68)	(1.67)
Constant	15.9***	19.1***
	(0.66)	(0.66)
Observations	276	276
R-squared	0.036	0.037

Notes: *** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses.

EXPERIMENT C

Table C1: Experiment C Dates

Day/Session	Treatment	Date	Time	Subjects
1/1	Treatment Placebo	17 October 2011	2.00-3.00pm	17
1/2	Treatment 1	17 October 2011	3.00-4.00pm	17
2/1	Treatment 1	13 November 2011	12.00-1.00pm	19
2/2	Treatment 1	13 November 2011	2.00-3.00pm	15
2/3	Treatment Placebo	13 November 2011	3.00-4.00pm	16
2/4	Treatment Placebo	13 November 2011	4.00-5.00pm	20

Table C2: Data description (Experiment C)

Treated Individual					
Variable	#Observations	Mean	SD	Min	Max
#Correct Additions	51	22.96078	10.39223	4	75
Happy Before	51	5.019608	.8121624	3	7
Happy After	51	5.235294	.8387666	3	7
GMAT	51	3.921569	1.074116	0	5
High School Grades	48	.5913232	.2537363	.0714286	1

Individuals treated with placebo clip					
Variable	#Observations	Mean	SD	Min	Max
#Correct Additions	53	18.81132	6.75977	7	35
Happy Before	53	4.849057	.863718	3	7
Happy After	53	4.566038	.950899	3	7
GMAT	53	3.943396	1.133665	0	5
High School Grades	52	.6622454	.2965477	0	1.411765

The following table shows that the comedy clip continues in Experiment C to have significant effects on subjects' performance once a range of covariates are included.

Table C3: The determinants of subjects' performance in Experiment C

	(1)	(2)	(3)
VARIABLES	Additions	Additions	Attempts
Treatment Dummy	5.06***	3.96***	4.51***
	(1.68)	(1.36)	(1.70)
Male	4.05**	3.05**	5.59***
	(1.67)	(1.35)	(1.69)
Age	0.16	0.15	0.20
	(0.26)	(0.21)	(0.26)
High School Grades	3.48	3.48	3.55
	(3.11)	(2.50)	(3.14)
GMAT	2.24**	1.82**	2.10**
	(0.87)	(0.70)	(0.88)
Day Dummy	Yes	Yes	Yes
Constant	0.67	3.74	3.94
	(7.12)	(5.73)	(7.18)
Observations	100	99	100
R-squared	0.215	0.208	0.220

Notes: In equation 2, the outlier visible in figure 6 of the main text has been dropped. *** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses.