Happiness and Productivity

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Abstract

We show that happiness raises productivity. In Experiment 1, a randomized trial is designed. Some subjects have their happiness levels increased, while those in a control group do not. Treated subjects have 12% greater productivity in a paid piece-rate task. They alter output but not per-piece quality of work. To check the robustness and lasting nature of such effects, a second experiment is designed. Major real-world unhappiness shocks -- bereavement and family illness -- are studied. The results from (real-life shock) Experiment 2 are consistent with those from (random-assignment laboratory shock) Experiment 1.

1. Introduction

There is a large economics literature on individual and economy-wide productivity. There is also a fast-growing one on the measurement of mental well-being. However, economists currently know little about the interplay between emotions and human productivity. Although individuals' happiness and their effort decisions are likely to be deeply intertwined, evidence is lacking on whether, and how, they are causally connected.

This paper tries to shed new light -- via randomized trials in two kinds of laboratory experiment -- on the question of whether happiness (or 'positive affect' in the terminology of psychology) induces better intrinsic motivation or instead promotes less careful behaviour. In a setting in which people are paid for their effort, this study finds large positive effects from happiness on to productivity.

The paper:

(i) experimentally 'assigns' happiness in the laboratory and, in the spirit of a natural experiment,

(ii) exploits major real-life (un)happiness shocks stemming from close bereavement and family illness.

Empirically, in the first experiment, we draw upon ideas and methods used in recent work by Kirchsteiger, Rigotti and Rustichini (2006). The assignment of happiness was undertaken using a traditional psychological method – mood induction, more specifically a movie clip chosen to operate as both audio and visual stimulus. This enabled us to use exposure to the clip as our key dependent variable. Our paper thus also relates closely to Ifcher and Zarghamee (2009), which slightly post-dates our own. We also have reported-happiness data from our subjects, which was used in supporting regressions and verified that our mood induction procedure was successful. In the second experiment we used reported happiness data gathered before the experimental task were undertaken as our dependent variable. Like Oswald and Wu (2010), later results provide empirical support for the reliability and informational content of happiness data.

In spite of its relevance in a wide range of settings (such as the workplace and the classroom), the concept of the happy-productive worker has often been relegated to the folklore of management as an unsubstantiated claim made only by practitioners or the popular press (Wright and Staw, 1998). Some psychologists have addressed this question by examining self-control and performance. Isen and Reeve (2005) show that positive affect induces subjects to change their allocation of time towards more interesting tasks, and that, despite this, the subjects retain similar levels of performance in the less interesting tasks. This hints at individuals becoming better able to undertake repetitive tasks as they become happier -- though the authors do not discuss exactly why this might be true or how this interacts with performance-related payment. More generally, psychologists have shown that positive emotion influences the capacities of choice and innovative content (Isen, 2000), improves memory recall (Isen et al. 1978; Teasdale and Fogarty 1979), and leads to greater altruism (Isen and Simmonds 1978).[1] These findings apply to unpaid settings.

The present paper implements an empirical test that has *not* been performed in the psychology literature. By doing so, we address a question that is of special interest to economists (and perhaps arguably also to economic policy-makers): *Does happiness make people more productive in a paid task?* The paper finds that it does. We demonstrate this -- using two different setups -- in a piece-rate 'white-collar' setting[2] with otherwise well-understood properties.[3] Interestingly, the effect operates through a rise in sheer output rather than in the per-item quality of the laboratory subjects' work. Effort increases. Precision remains unaltered. In the first part of the paper, we do not distinguish in a sharp way between happiness and mood. We take the distinction, in a short run experiment like the one initially described later, to be predominantly semantic. Nor do we explore the possibility that other stimuli such as music, alcohol or sheer relaxation time -- all mentioned by readers of early drafts -- could have equivalent effects. Nor can we assess exactly how long-lasting are the effects of emotion upon labor productivity. However, in a second experiment, we then turn to such issues. Here we draw upon important external shocks from Nature as a real-life source of variation.

In the next section we describe the existing related literature. Section 3 is devoted to a theoretical model that provides a conceptual framework. Sections 4 and 5 describe the first experiment. Section 6 presents the main results and Section 7 some empirical checks. In Section 8, we examine questionnaire responses that shed

[1] 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 a series of papers in the 1980s, Ashby et al. (1999), Erez and Isen (2002), and the recent work of Hermalin and Isen (2008). A survey is available in Isen (1999). Our study also has links to ideas in the broaden-and-build approach of Frederickson and Joiner (2002) and to the arguments of Lyubomirsky et al. (2005).
[2] Such as Niederle and Vesterlund (2007).
[3] The analysis draws on a kind of mood induction procedure that is uncommon in the economics literature but is more familiar to researchers in social psychology. One exception is Kirchsteiger, Rigotti, and Rustichini (2006) who find a substantial impact in the context of gift-exchange.

light on subjects' self-perception and related issues. Section 9 describes a second experiment -- on large emotional shocks from the real world -- that confirms the results from the first experiment. Some further results are given in an appendix.

2. Background

The links between productivity and human well-being are potentially of interest to many kinds of social scientists. Argyle (1989, 2001) points out that little is understood about how life satisfaction affects productivity, but that there is (some) evidence that job satisfaction exhibits modestly positive correlations with measures of worker productivity. Wright and Staw (1998) find a significant and sizeable effect of long term happiness on productivity. More specifically, Boehm and Lyubomirsky (2008) preliminarily define a happy person as someone who frequently experiences positive emotions like joy, satisfaction, contentment, enthusiasm and interest. Then, by drawing on both longitudinal and experimental studies, they show that people of this kind are more likely to be successful in their careers.[4]

Together with the works mentioned in the previous section, a number of other papers have been interested in positive affect and performance. Work by Wright and Staw (1998) examines the connections between worker affect and supervisors' ratings of workers. Depending on the affect measure, the authors find mixed results. Amabile et al. (2005) uncovers evidence that happiness provokes greater creativity. In contrast to our paper's later argument, Sanna et al. (1996) suggests that those individuals in a negative mood put forth a high level of effort. [5]

However, these results are all for unpaid activities in the sense that the laboratory subjects' marginal wage rate is zero.

Our work is also related to two contributions in the economics literature, which specifically introduce the concept of distraction in the labour supply decisions. Dickinson (1999) provides evidences that an increase of a piece-rate wages can decrease working ours, but increase labour intensity. Banerjee and Mullainathan (2008) consider a model where labour intensity depends on outside worries; this generates a highly non-linear dynamics between wealth and effort on the job. Both these abstract from any effect due to happiness or other emotions.

[4] See Pugno and Depedri (2009) for an extensive survey of this argument.

[5] See also Baker et al. (1997), Boehm and Lyubomirsky (2008), Paterson et al. (2004), Steele and Aronson (1995) and Tsai et al. (2007).

More generally, a small analytical literature in economics is relevant to our later empirical findings. Although not directly about happiness, it examines intrinsic motivation -- i.e. motivation based on internal psychological incentive -- as opposed to the extrinsic motivation (incentivized payments) normally considered in economics. [6] A paper by Benabou and Tirole (2002) focuses on the interactions between self-deception, malleability of memory, ability, and effort. The authors consider the possibility that self-confidence enhances the motivation to act, so their framework is consistent with the idea that there can be a connection between mood and productivity. They develop an economic model of why people value their self-image, and they use this specifically to justify seemingly irrational practices such as handicapping self-performance or the practising of self-deception through selective memory loss. Compte and Postlewaite (2004) extend this line of work; they seek to identify circumstances in which biased perceptions might increase welfare. The authors treat perceptions as an accumulation of past experiences under gradual adjustment. Benabou and Tirole (2003) provide a formal reconciliation of the importance of intrinsic motivations with extrinsic (incentivised) motivations. Such writings reflect an increasing interest among economists in how to reconcile external incentives with intrinsic forces such as self-motivation.[7]

Finally, Gneezy and Rustichini (2000) examine the relationship between monetary compensation and performance. They provide contrasting kinds of evidence. They show that increasing the size of monetary compensation raises performance, but they also find that offering no monetary compensation can be better motivation than offering some. Gneezy and Rustichini discuss how to rationalize this finding, and offer several possible explanations. One is based on the notions of intrinsic and extrinsic motivation developed within psychology. Put simply, subjects may be intrinsically motivated to do well, but this is displaced when they are offered a form of extrinsic motivation (monetary compensation). In these terms, our later analysis examines the impact of mood on intrinsic motivation (by holding constant the level of monetary compensation) and so builds upon Gneezy and Rustichini's contribution.

[6] As described in sources such as Laffont and Tirole (1993).
[7] A review paper examining the links between choices and emotional state in psychology is Diener et al. (1999). 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), Easterlin (2003), Frey and Stutzer (2002, 2006), Kahneman and Sugden (2005), Luttmer (2005), Oswald (1997), Van Praag and Ferrer-I-Carbonell (2004), and Winkelmann and Winkelmann (1998). For related work on emotions, see Frank (1988), Elster (1998), and Loewenstein (2000).

3. <u>A model of work and distraction</u>

This section describes a theoretical framework. It seeks to address the question of whether happiness can be expected to induce better intrinsic motivation or on the contrary to promote less careful behaviour at work. The main comparative static result stems from a form of internal resource-allocation by a worker. A later empirical section discusses the theoretical model in the light of the answers that (a subset of) laboratory subjects gave to a questionnaire presented to them at the end of the experiment. The modeling structure we sketch is potentially complementary to Ashby et al.'s (1999) neurobiological one, where the emphasis is on a route from positive affect to increased dopamine, but ours is framed in the choice-theoretic style of neoclassical economics -- as in Dickinson (1999) and Banerjee and Mullainathan (2008).

Think of individuals as having a finite amount of energy. Within any period of time, they must choose how to distribute that across different activities. Denote *u* and *v* as two different sources of utility to the individual. Let *e* be the energy the worker devotes to solving tasks at work. Let *w* be the energy the worker devotes to other things -- to 'distractions' from work. Let *R* be the worker's psychological resources. Hence *(e + w)* must be less than or equal to *R*.

We assume that *u*, the utility from work, depends on both the worker's earnings and the effort put into solving work problems. Then *v* is the utility from attending broadly to the remaining aspects in life. For concreteness, we could think of this second activity as a form of 'worrying'. But it can be viewed as a generalized concern for issues in the worker's life that need his or her cognitive attention. In a paid-task setting, it might be realistic to think of a person as alternating, during the working day, between concentrating on the work task and being distracted by the rest of his or her life. There is a psychic return from the energy devoted to distraction and worry -- just as there is a return from concentrating on the paid task.

Consider an initial happiness shock, *h*. For the sake of clarity, assume separability between the two kinds of utility going to the individual. People then solve the problem: choose paid-task energy *e* to

Maximize $u(p,e,h,z)+v(w,h)$ subject to $R \geq e + w$ (1)

where the first-order condition for a maximum in this problem is simply

$$Eu_e - v_w = 0. \qquad (2)$$

The comparative-static result of particular interest here is the response of productivity, given by work effort *e*, to a rise in the initial happiness shock, *h*. Formally, it is determined in a standard way. The sign of *de*/dh* takes the sign of the cross partial of the maximand, so that:

Sign *de*/dh* takes the sign of $Eu_{eh} - v_{wh}$. (3)

But without more restrictions, this sign could be positive or negative. A happiness shock could increase or decrease the amount of effort put into the work task.

To get some insight into the likely economic outcome, consider simple forms of these functions. Let *R* be normalized to unity. Assume that the *u* and *v* functions are concave and differentiable. This is not strictly necessary, but it leads to natural forms of interior solutions. The analysis is easily generalized.

How then might an exogenous happiness perturbation, *h*, enter a person's objective function?

The additive model has a maximand as follows

u(.) + v(.) + h

and is -- we conjecture -- what most economists would write down when asked to think about exogenous emotions and choice. They would view a happiness shock as a vertical shift upwards in the utility function, so that the worker gets the *h* happiness shock whether or not he or she subsequently works or instead worries about other things. Therefore, the optimal work effort *e** is independent of the happiness shock, *h*, or, put in other words, happier people are neither more careless nor more productive.

Another, and arguably more plausible, form of utility function has a happiness shock operating within a concave structure. Imagine the worker solves

Maximize $u(pe+h)+v(1-e+h)$ (4)

which is the assumption that *h* is a shift variable inside the utility function itself, rather than an additive part of that function.

Now the first-order condition is

$u'(pe+h)p - v'(1-e+h) = 0$.

(5)

In this case, the optimal level of energy devoted to solving work problems, *e**, does depend on the level of the happiness shock, *h*:

The sign of *de*/dh* takes the sign of $u''(pe+h)p - v''(1-e+h)$.

Its first element is thus negative and its second is positive. By the first-order condition, we can replace the piece rate wage term *p* by the ratio of the marginal utilities from working and worrying.

Hence, after substitution, the sign of the comparative static response of *work effort, e, with respect to the size of the happiness shock, h, is greater than or equal to zero* as

$$\frac{u''(.)}{u'(.)} - \frac{v''(.)}{v'(.)} \geq 0. \tag{6}$$

These terms can be viewed as versions of the degrees of absolute risk aversion in two domains -- the utility from work and the utility from worrying. If the marginal utility of worry declines quickly enough as energy is transferred from working to worrying, then a positive happiness shock will successfully raise the worker's chosen productivity, *e**.

Put intuitively, as the individual become happier and condition (6) holds, that allows him or her to divert attention away from other issues in life and become more focused on their job. On the contrary, if condition (6) does not hold, the shock in happiness would lower the marginal utility from working and divert more energy to worry -- hence the level of effort, *e*, would decline.[8]

This analytical approach, in which effort is not independent of *h*, potentially offers economists a way to think about the role of stress in the workplace. Work-life strain could be conceived of as the (rational) need to devote energy and attention away from the job. Happier workers need to do so less, and thus they have higher productivity.

In the experiment described next, we will see that a shock to happiness somehow does allow individuals to work harder in aggregate. Hence, given the concave model outlined in this section to be correct, condition (6) could be thought of as holding for our set of subjects.[9]

4. <u>Experimental design</u>

We start with a motivation for the choices made within the design, and then provide a description of the tasks and a time-line for the trial. The experimental

[8] Interestingly enough, as we will see in section 9, many respondents to the subject questionnaire used phrases like "distracting" (twinned with a reported expectation that our mood induction technique would lower productivity) and "relaxing" (twinned with a reported expectation that our mood induction technique would raise productivity) to describe the effect of the mood-inducement device used during the experiments -- though we found that they were not necessarily capable of consciously and correctly estimating the direction of the effect on their performance.

[9] Since our experiment is between-subject, we will not disentangle the effect of the shock on a particular subject or explore if there is a degree of heterogeneity between them.

instructions, the GMAT MATH-style test, and the questionnaires are set out in an appendix. The structure was built around the desire to understand the productivity of workers engaged in a task for pay. Our focus is the consequences, for their output, of different starting levels of happiness.

This study employs a task previously used in Niederle and Vesterlund (2007). It entails asking subjects to add sequences of five 2-digit numbers under timed conditions. The task is a simple one but is taxing done under time pressure. It might be thought of as representing -- in a highly stylized way -- an iconic white-collar job: both intellectual ability and effort are rewarded. Subjects were allowed to use pen and paper, but not a calculator or similar.

We wish ideally to disentangle the effort component from the ability component. To this end, we allowed for two control variables that we hoped would capture underlying exogenous but heterogeneous ability as opposed to effort -- although we were also open to the possibility that changes in underlying happiness might induce shifts in ability or change the nature of the interaction between ability and effort to alter overall productivity. Our control variables came from (i) requiring our subjects to do a brief GMAT MATH-style test (5 multiple choice questions) along similar lines to that of Gneezy and Rustichini (2000), and (ii) obtaining information in a final questionnaire to allow us to construct a measure of subjects' prior exposure to mathematics. The aim was to control for heterogeneous ability.[10]

Some means has to be found of inducing an exogenous rise in happiness. The psychology literature offers evidence that movie clips (through their joint operation as a form of audio and visual stimulus) are a means of doing so. They exogenously alter people's feelings. Westermann et al. (1996) provides a meta-analysis of methods.

We used a 10-minute clip based on composite sketches taken from various comedy routines enacted by a well-known British comedian. In order to ensure that the clip and subjects were well-matched, we restricted the laboratory pool to subjects of an English background, who would likely have been exposed to similar humor before. Whether subjects were exposed to the clip or not is our key dependent variable. As is explained later, whether subjects enjoyed the clip turned out to be important to the effects on productivity.

[10] 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.

While the key treatment involved the use of the clip as compared with a control treatment identical but for the lack of a clip, we also wanted to address the possibility that the time spent watching the movie clip might be an important factor. Hence we also ran a second control treatment using a "placebo" film clip designed to be neutral with regard to mood but to take up the same amount of time as the comedy clip. The placebo film essentially consisted of the appearance of colored lines placed randomly on a screen. Usefully, the data revealed that this placebo clip was not significantly different from showing no clip whatsoever. These results are reported in part 2 of the appendix.

Experiment 1 generates variation in happiness across our laboratory subjects. At the broadest level, however, we are also interested in whether natural real-world variation in happiness, in response to emotional shocks, might create productivity effects. Moreover, Experiment 1 is intrinsically short-run: we would not expect the impact of the comedy clip to last. Therefore, in a separate set of supplementary experimental sessions, we asked subjects to report significant real-world shocks in the previous few years -- including family health issues and deaths. Section 9 reports our findings -- this is Experiment 2 -- in this longer-run context.

In summary, the data collected included the number of successful and unsuccessful numerical additions, performance in a brief GMAT MATH-style test, and (for a subset of laboratory subjects) responses to a questionnaire that included questions relating to happiness, personal characteristics and intellectual ability.

5. Design in detail

In Experiment 1, we randomly assigned people into two groups:

Treatment 0: the control group who were not exposed to a comedy film clip.

Treatment 1: the treated group who were exposed to the comedy clip.

The control-group individuals were never present in the same room with the treated subjects (hence they never overheard laughter, or had any other interaction). The experiment was carried out on six days, with deliberate alteration of the early and late afternoon slots, so as to avoid underlying time-of-day effects, as follows.

Our main experiment took place over 4 days and 8 sessions; we then added 4 more sessions to check for the robustness of our central result to both the introduction of an explicit payment and a placebo film (shown to the otherwise untreated group).

Accordingly, the experiment consists of

- Day 1: session 1 (treatment 0 only), session 2 (treatment 1 only).

- Day 2: session 1 (treatment 0 only), session 2 (treatment 1 only).
- Day 3: session 1 (treatment 1 only), session 2 (treatment 0 only).
- Day 4: session 1 (treatment 1 only), session 2 (treatment 0 only).

plus

- Day 5: session 1 (treatment 1 and explicit payment), session 2 (treatment 0 and placebo clip)
- Day 6: session 1 (treatment 0 and explicit payment), session 2 (treatment 1 and explicit payment)

Subjects were allowed to take part on only one day and in a single session.

On arrival in the lab, individuals were randomly allocated an ID, and made aware that the tasks at hand would be completed anonymously. They were asked to refrain from communication with each other. Those in treatment 1 (the Happiness Treatment subjects) were asked to watch a 10 minute comedy clip designed to raise happiness or 'positive affect'.[11] Those in the control group came separately from the other group, and were neither shown a clip nor asked to wait for 10 minutes. In a different setting, Isen et al. (1987) found that a control clip without positive affect gives the same general outcomes as no clip, which we later confirmed in our own "placebo" treatment (day 5, session 2).

For days 1-4, the subjects in both the movie-clip group (treatment 1) and the not-exposed-to-the-clip control group (treatment 0) were given identical basic instructions about the experiment. These included a clear explanation that their final payment would be a combination of a show-up fee (£5) and a performance-related fee to be determined by the number of correct answers in the tasks ahead. At the recruitment stage it was stated that subjects would make "… a guaranteed £5, and from £0 to a feasible maximum of around £20 based purely on performance". In practice, subjects received £0.25 per correct answer on the arithmetic task and £0.50 on each correct GMAT MATH answer, and this was rounded up to avoid the need to give them large numbers of coins as payment.

[11] The questionnaire results indicate that the clip was generally found to be entertaining and had a direct impact on reported happiness levels. More on this is in the results section.

We used two different forms of wording related to payment (to try to be as thorough as possible):

- 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 MATH-style questions (£0.50 per correct answer).

In this way, by using two different forms of wording, we can do several things. First, in the latter case we cover the standard revealed-payment setup, which is a good proxy for traditional piece-rate contracts (days 5-6), and we can also mimic many real-life situations where workers usually do not know the precise return from each productive action they take (days 1-4). Second, this provides us with the opportunity to check that the wording of a payment method does not have a significant effect on our results -- essentially making one set of days a good robustness check on the other. The later findings reveal that the role of happiness is not different in days 5-6 compared to days 1-4.

Following the economist's tradition, one reason to pay subjects more for every correct answer was to emphasize that they would be benefit from higher performance. We could also then avoid the idea that they might be paying back effort -- as in a kind of 'reciprocity' effect -- to the investigators for the show-up fee.

The subjects' first task was to answer correctly as many different additions of five 2-digit numbers as possible. The time allowed for this, which was explained to them beforehand, was 10 minutes. Each subject had a randomly designed sequence of these arithmetical questions. Numerical additions were undertaken directly through a protected Excel spreadsheet, with a typical example as in Legend 1. The spreadsheet necessarily contained more such rows that any subject could hope to add in the ten minutes allowed. The subjects were not allowed to use calculators, and it was explained that any attempt to use a calculator or any outside assistance was deemed to be a disqualification offence, resulting in only the show-up fee being paid, though they were allowed to use pen and paper and these were provided for their use. This did not prove to be a problem across the 4 experimental days. The numerical additions were designed to be reasonably simple, if repetitive, and earlier literature has deemed this a good measure of intellectual effort (Niederle and Vesterlund, 2007).

31	56	14	44	87	

Legend 1: Adding 2-digit Numbers

The second task for subjects was to complete a simple 5-question GMAT MATH-style test. These questions were provided on paper, and the answers were inputted into a prepared protected Excel spreadsheet. The exact questions are given in an appendix. This test was designed as a brief check on ability, as used before in the research literature (Gneezy and Rustichini, 2000).

The final task, which was not subject to a performance-related payment (and subjects were made aware of this), was to complete a questionnaire. A copy of this is provided in the appendix. The questionnaire inquired into both the happiness level of subjects (before and after the clip for treatment 1), and their level of mathematical expertise[12]. The wording was designed to be straightforward to answer; anonymity was once again stressed before it was undertaken; the scale used was a conventional 7-point metric, following the well-being literature.

Moreover, in day 5 and day 6, we added extra questions (as detailed in the appendix). These were designed to inquire into subjects' motivations and their own perceptions of what was happening to them. The purpose was to try to shed light on the psychological mechanism that made our treated subjects work harder.

To summarize the timeline for Experiment 1:[13]

1. Subjects enter and are given basic instructions on experimental etiquette.

2. Subjects in treatment 1 are exposed to a comedy clip for 10 minutes, otherwise not.

3. Subjects are given additional instructions, including a statement that their final payment relates to the number of correct answers, and instructed against the use of calculators or similar devices.

[12] 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. As reported below, we can argue from the answers given to the questionnaires that we have been successful in this aim.

[13] The full instructions provided in the appendix provide a description of the timing.

4. Subjects move to their networked consoles and undertake the numerical additions for 10 minutes.

5. Results are saved and a new task is initiated, with subjects undertaking the GMAT MATH-style test for 5 minutes.

6. Results are again saved, and subjects then complete the final questionnaire.

7. After the questionnaire has been completed, subjects receive payment as calculated by the central computer.

6. Principal results from Experiment 1

A group of 276 subjects drawn from the University of Warwick participated in the experiment. Of these, 182 took part in the main experiment, while the others participated in the sessions of day 5 and 6. Each was part of only one session. A breakdown of the numbers per day and per session is contained in Table 1.

The subject pool in the main version of Experiment 1 was made up of 110 males and 72 females. Tables 2 and 3 summarize the means and standard deviations of the main variables (respectively for the treated and untreated subjects). Our key dependent variable is whether a subject observed the clip or not. The first independent variable, the key one in our analysis, is the number of correct additions in the allotted ten minutes. 'Happiness before' is the self-reported level of happiness for all subjects (before the clip for the treated group) on a seven point scale. The variable 'happiness after' is the level of happiness after the clip for the treated group; 'GMAT MATH' is the number of correct problems solved in that section; 'high-school-grades' is an index calculated from the questionnaire. Enjoyment-of-clip is a measure in a range between 1 and 7 of how much they said they liked the movie clip.

According to the data, the clip is successful in increasing the happiness levels of subjects. As shown in Figure 1, they report an average rise of almost one point (0.98) 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).

Figure 2 displays the mean levels of productivity. The treated group's mean performance is higher by 1.71 additions than the average performance of the untreated group. This productivity difference is considerable; it is approximately ten percent. It is also statistically significantly different from zero (*p*=0.04).

A sub-group was noticeable in the data. Interestingly, and encouragingly, the performance of those 16 subjects in the treated group who did not report an increase in happiness is statistically non-different from the performance of the untreated group (*p*=0.67). Therefore, the increase in the performance seems to be linked to the increase in happiness rather than merely to the fact of watching a movie clip. The clip did not hamper the performance of subjects who did not declare themselves happier.[14] For them, the effect is zero. We return to this below.

In Figure 3 we show the performances of male and female subjects. Both groups feature a similar increase in their arithmetical productivity (1.90 additions for male, 1.78 for female). The effects of the movie clip on productivity do not operate perfectly symmetrically. From the cumulative distributions on the number of correct answers for the treated and untreated groups, shown in Figure 4, we see that the treatment increases the performances of low and medium performers, while the high performers are apparently less affected.

So far, these findings are based on elementary t-tests. We also performed OLS-based regressions to analyze the determinants of performance. Table 3 presents equations for the log of the number of correct additions. The variable called 'Change-in-Happiness' is the difference in reported happiness before and after the clip; 'GMAT MATH' is a test score. 'High School Grades' measures school performance. 'Day 2', 'Day 3' and 'Day 4' are day-of-the-week dummies.

Consistent with the result seen in the previous session, the subjects' performance is markedly better in the treated sessions. As clear in Table 7's regression (1), in the first column, this result holds when we control for the subjects' characteristics and for periods. The coefficient of 0.118 implies that the happiness treatment increases people's productivity by approximately 12%. In regression (2) of Table 7, the performances are increasing in the rise in elicited happiness (for the case of untreated subjects, by definition, Change-in-Happiness=0). This result is still true when we restrict the analysis to the treated subjects alone, as in regression (3). The size of the effect is only slightly smaller (than in column 2 of Table 7) at

[14] The 17 subjects who did not declare an increase in happiness apparently enjoyed the clip. In a range of values between 1 and 7, the average is 5.41, with a minimum of 5 and a maximum of 7.

approximately eight and a half percentage points.

Because of the known skewness in human-performance data, it is natural to use a logged variable. Nevertheless, as a rough check, Table 8 (columns (1) and (3)) re-runs the first two regressions of Table 7 with a dependent variable defined on absolute values rather than log values. The variable 'Treatment' in column (1) remains large and positive. It remains statistically significant when, in column (2) of Table 8, we exclude the performance outliers (here we drop the two extreme laboratory subjects, with respectively 2 and 43 correct additions). Similarly, the coefficient on the variable Change-in-Happiness is statistically significantly different from zero irrespective of whether or not in Table 8 the two outliers are retained: see regressions 3 and 4.

Might the pattern in the data be in part a kind of reciprocity effect? Are these laboratory subjects 'repaying', or somehow trying to please, the investigators? Such difficulties are not uncommon in economics experiments. However, the issue is not applicable in the current setting. In this experiment, people are paid more for every addition they solve. That money goes to them, so that, if anything, they know that extra productivity <u>hurts</u> rather than aids the investigators.[15]

Alternatively, laboratory subjects might wish to reciprocate the expected payments made by experimenters by doing as the experimenter wishes. To partially address this, we added direct questions to the questionnaires in days 5 and 6, asking subjects: "Did you try your best when asked to add numbers?"; "If so, why? If not, why not?"; "Did you feel that first observing the video clip made you better or worse at adding up numbers?"; and "Can you say why you believe that?" Among the treated subjects, out of 48 answers only 31% thought the clip had a good effect, for 23% this effect was bad, while 42% felt it was irrelevant. The number of subjects who declared that the clip had beneficial consequences was not statistically larger than the number of subjects who felt the effect likely to be bad (p=0.22). Furthermore, among the 25 subjects who were shown a placebo film -- discussed later -- the answers were similar (bad 44%, good 24%, indifferent 32%). The difference between subjects who thought that the placebo film had a positive influence is not statistically different from

[15] Notwithstanding this point, even if the mood-induction procedure did enhance productivity in the experiment through some feeling of reciprocity on the part of the subjects, this would not be in contrast with our hypothesis. Subjects became happier after the clip, assuming against our previous argument that they wanted to reciprocate by working harder. This feeling would always result through a positive increase of $u(.)$, the utility from working. This mechanism would result in a positive transfer of e from worrying to working. The difference would be that h would act to increase $u(.)$ through reciprocity rather than directly.

the number of subjects thinking that the real treatment had a positive effect (p=0.26). All this appears to point towards subjects not being able to assess the impact of the clip, and not being entirely sure whether we as experimenters were using the clip to aid or hinder them.

Accordingly, since subjects' own perceptions on the impact of the clip on productivity are incorrect, it is hard to argue that they first worked out what the experimenter wanted and then went about trying to ensure that the experiment was a success.

Another potential concern arises. It is that subjects may convince themselves that the fact of watching a clip per se might enhance performance. In section 7, we discuss this, and find that it is unlikely to be true. It is shown that individuals who are treated with a placebo clip do not perform significantly differently from individuals who are untreated (if anything, they do fractionally worse on the additions task). It appears, therefore, that positive emotion invigorates human beings. Yet the mechanism remains unclear. Does happiness have its effect on labor productivity through greater numbers answered or through greater accuracy of the average answer? This distinction is of interest. It might even be viewed as one between industry and talent -- between the consequences of happiness for pure effort compared to effective skill.

To inquire into this, we estimate a different kind of equation. Table 9 takes *attempted additions* (in log terms) as the dependent variable. The results are similar to the ones in Table 7, where we considered the number of correct additions. Attempted additions rise by slightly more than 9%. Then, in Table 10, we run exactly the same regression as in Table 9 but with a different dependent variable. This is an estimated equation for ‘precision’, namely, the *ratio of correct-answers to attempted-answers*. Interestingly, in Table 10 neither the dummy treatment nor Change-in-Happiness is statistically significantly different from zero. Therefore the treatment acts as an upward intercept shifter in the attempts equation; the treatment itself does not provide extra precision. It is perhaps also worth noting that subjects’ precision levels are influenced by their underlying mathematical skill, as measured by the mini GMAT MATH score, and to a lesser extent by scholastic grades.

7. Empirical checks

We performed a large set of tests of robustness. The detailed results from these, with extra tables, are laid out in the appendix.

First, we examined sub-samples of data. Importantly, the positive effect on productivity was visible in a remarkably robust way in the data. This can be seen in the first part of the appendix. For example, in the raw data of Table A2, the productivity boost from the happiness treatment is observable in seven of the eight sub-trials. See the 4th and 5th columns of Table A2. The single exception is for males in Session 2, and that result is driven by one outlier -- the individual in the sample who scored an extraordinary 43 correct answers.

Tables A3 and A4 give regression results on further experimental breakdowns. The continued robustness of the main finding is evident.

Second, an extra trial was done in which a 'placebo' film -- a moderately interesting but not intrinsically happy clip -- was shown to a control group. This was to ensure that our productivity findings were not an outcome that any film might be produce. The film clip was "Computer Graphic" on James Gross's resources site: http://www-psych.stanford.edu/~psyphy/movs/computer_graphic.mov.

This movie clip depicts patterns of colored sticks. These appear and disappear randomly on screen. The film is considered "neutral" by social psychologists. By setting the process to repeat, it was possible to play this clip for the appropriate length of time. Importantly, the productivity of individuals was not increased by showing them this placebo film. In Table 11, regression 1, we see that the number of correct additions declined marginally (not in a statistically significant way) when compared to the control setting used earlier in our paper, namely, where individuals straight away begin work on the additions tasks. Third, a trial was done in which individuals were told an explicit monetary amount -- £0.25 -- for each correct answer. The purpose here was to check that having a specified payment did not alter the tenor of the findings. From regression 1 of Table 11 we note that this increased productivity in a non-significant way (p value 0.21). More importantly, we can see from Table A5 that the previous pattern holds in this case as well: treated subjects perform on average better than untreated ones. Table 11 establishes this claim. In regression 1, where this new scheme is interacted with treatment, the variable, although negative, is not significant (p value 0.69) and it is actually positive (but again not significant (p value 0.81) in regression 3 where we consider the attempts as the dependent variable. Interestingly, from regression 5 we can see that the dummy payment is positive (although non significant with p value 0.21) with respect to the precision; this seems to suggest that -- if anything-- an explicit payment scheme increases productivity via precision and not via attempts. The above considerations suggest that the impact of happiness on productivity will not change if the payment is specified. These consistency tests are encouraging. Much remains, nevertheless, to be understood. One puzzle generated by the data is about the nature of the transmission channel from human happiness to people's labor productivity. The paper's earlier theoretical framework describes a set of cases in which, as a structural or mathematical matter, the correct empirical prediction emerges. However, further experiments will have to be designed to try to probe the precise transmission mechanism.

Another consideration which may be relevant -- we thank Greg Jones for this suggestion -- is that happiness could act to increase cognitive flexibility. In some recent work, this has been proposed in a narrow context, of the perception of local versus global aspects of a visual scene (Baumann and Kuhl, 2005; Tan, Jones and Watson, 2009). The argument is fairly simple. If someone is focusing on local aspects, then positive affect improves processing of global aspects; and if focusing on global aspects, then it encourages local processing. Jones and colleagues have called this "encouraging the perceptual underdog", and it is distinct from previous suggestions about, say, positive affect simply promoting global processing. It seems plausible to hypothesize that happiness could have a similar effect on a broader canvas, where labor productivity benefits from the individual worker being encouraged to try out hitherto neglected strategies. Finally, (see Tables 2 and 3) the result of the GMAT-style test are not significantly different between treated and untreated, (3,5 for the first 3.37 for the second). This suggests that the GMAT-style test (see note 12 for more details) is a good control variable; it is unaffected by the treatment.

8. Subjects' self-perceptions

Towards the end of our early experimental trials, it became clear that the main result was occurring again and again -- appearing even in sessions with the fewest subjects. We therefore decided to attempt to probe, in a qualitative way, what might be happening. In the light of 73 questionnaires completed by the subjects on days 5 and 6, we can ask which of the ideas discussed in section 3 are consistent with the subjects' own perceptions. In general, no laboratory subject declared that the treatment induced greater focus, while 10 percent of the treated subjects found the comedy clip distracting (this is significantly different from 0, with p=0.01). Moreover, it seems that subjects disagree on the effect of the treatment on performance: out of 48 answers, for 31% the effect was positive, for 23% the effect was negative, while 42% felt it to be irrelevant. This seems to reflect the ambiguity of the effect of happiness on productivity implicit in our theoretical model. As shown in section 3, this effect is positive only if condition (6) is satisfied, which might be the cause of the variation in subject responses, though again a lack of self-perception about the ultimate effect of the clip is also equally plausible. While 88% of subjects who think that the effect is positive find the clip relaxing, 45% of subjects who think that the effect is negative find it distracting, and 12% still use the word "relaxing" albeit this time to describe a negative impact. If we interpret a pronouncement of "relaxing" by subjects for which the effect was positive as an indication of some relief from outside worries, and the pronouncement of "distracting" by subjects for which the effect was negative as an indication of an inability to focus on the task in hand -- perhaps even an increased preoccupation with outside worries -- this answer might be again consistent with the theoretical model.

We need to add a note of caution, because the ambiguity in subjects' responses might be indicative of a general inability to perceive the true impact of the clip on their own performance. This is not implausible, because no subject was allowed to take part in more than one session, so there was no frame of reference for the subjects to consider. To consider how good the subjects were at correctly identifying the direction of the effect on their performance, we tried another approach. The 15 subjects who declared that the treatment had a positive effect made on average 21.33 correct additions, against the 18.54 of the remaining 33 subjects. This difference is insignificantly different from zero (p=0.15), although the sample here is small. If we consider only subjects who felt relaxed *and* thought the effect of the clip was positive, the p value is 0.10. A positive side-effect of subjects' inability to perceive the impact of the clip on their own performance is to lessen any concerns about the so-called 'demand effect' through which laboratory subjects might wish to reciprocate the expected payments made by experimenters by doing as the experimenter wishes. If subjects' own perceptions are incorrect, it is difficult to argue that they first worked out what the experimenter wanted and then went about trying to ensure the experiment succeeded.

9. Real-life happiness shocks and productivity in a second experiment

We also performed Experiment 2. For this, a (deliberately) different group of laboratory subjects were chosen. Once again, the individuals had to do additions task. They were asked at the end of their session to complete a questionnaire (reported in the appendix) with supplementary questions designed to check whether they had experienced at least one of the following bad life events: close family bereavement, extended family bereavement, serious life-threatening illness in the close family, and/or parental divorce. We asked subjects to report their level of happiness right at the start of the session prior to any of these questions about life events and prior to any explanation of the tasks to be performed. Since, here, we did not expose the subjects to a comedy film clip, reported happiness is a key dependent variable for this experiment. Otherwise, the design was kept identical to the main experiment under explicit payment. As before, subjects were paid 25 pence per correct addition performed, and 50 pence for correct GMAT question answers, and they knew the payment rates. To summarize the timeline for Experiment 2:[16]

1. *Subjects enter and are given basic instructions on experimental etiquette.*
2. *Subjects move to their networked consoles and are asked to report their level of happiness.*
3. *We announce the payment method (25 pence per correct addition), instruct subjects against the use of calculators or similar devices and then subjects undertake the numerical additions for 10 minutes.*
4. *Results are saved, and the GMAT MATH-style test is initiated for 5 minutes, with the explicit payment rate again announced in advance (50 pence per correct answer).*
5. *Results are again saved, and subjects are asked to complete the final questionnaire in privacy and without time pressure.*
6. *After the questionnaire is completed, subjects receive payment as calculated by the central computer.*

Throughout Experiment 2, anonymity and privacy were stressed.

This experiment took place over 2 days and 8 sessions:

- Day 1 (October 2009), 4 sessions lasting approximately 45 minutes each.
- Day 2: (November 2009), 4 sessions lasting approximately 45 minutes each.

Only subjects who had not taken part in the main experiment were permitted to take

[16] The full instructions provided in the appendix provide a description of the timing.

part in the real-life happiness shock experiment, and they were allowed to participate on only one day and in a single session.

A group of 179 subjects drawn from the University of Warwick participated in Experiment 2. A detailed breakdown of the numbers per day and per session is contained in Table 12.

We define the bad life event (BLE) to be either bereavement or illness in the family.[17] Given the small numbers, the data suggested that it was appropriate to aggregate these happiness-shock events by using a single variable BLE, equal to 1 if a subject reported at least one of the three BLE considered. Table 13 summarizes the means and standard deviations of the main variables.

Here we are, in effect, thinking of Nature as having dealt randomized shocks to some, but not all, of the laboratory subjects. We exploit that variation.

It may be worth emphasizing the similarity of the laboratory subjects who took part in these additions experiments. They are young men and women who attend one of the elite English universities with required entry grades amongst the highest three or four universities in the country. Compared to any random slice of an adult population, they are extraordinarily homogenous individuals. Those who have experienced family illness or bereavement are, to the outside observer, almost indistinguishable from the others.

The first column of Table 14 reports the statistical impact of BLE in each year from 0 to 5 (as declared at the end of the experiment) upon the individuals' levels of happiness (as declared at the beginning of the experiment). Interestingly, the pattern is consistent with *hedonic adaptation* -- an effect that is positive, significant, and declining through time. It is strong if it happened less than a year ago, and becomes insignificantly different from zero after approximately 2 years.

This intriguing 'diminishing' effect of a Bad Life Event appears also in the second column of Table 14, which shows the effect of the Years-Since-Bad-Life-Event variable YBLE, set to Log(year of the Bad Life Event+0.01) if the bad life event occurred, and Log(N+0.01) otherwise, where in that case we set the number of years to N=10.[18] The coefficient of YBLE is positive and statistically significantly

[17] In the questionnaire, we also asked about parental divorce, but it turned out to have an ambiguous and opposite effect on the subject according to the gender of the subject.

[18] This is not a crucial issue but literally implies that we think even the luckiest subjects will get a bad life event once every decade. Empirically, we asked subjects to report the events that happened in the last 5 years. Even setting N equal to 6, for the case of no bad life event, does not change the results substantively.

different from zero.[19]

These experimental results are consistent with a range of hedonic adaptation findings in the survey-based statistical research within the literature on the economics and psychology of human well-being (e.g. Clark et al. 2008).

In column 3 of Table 14, we aggregated the bad life events into two years; as expected, the sign is negative and significant. Finally, column 4 performs a robustness check. As can be observed from the data description in Table 14, 25 subjects that declared at least one BLE did not report the exact year of it.[20] In order to explore whether these missing data points could somehow bias our results, we (conservatively) assumed that the bad life event for which the time was not reported was older than 2 years. Therefore, we set BLE1=0 either when subjects did not report any bad life event or they did not report the year. The coefficient on BLE1 is slightly smaller than the coefficient of BLE, but it remains statistically significant at the five percent level. The idea to look in an experimental setting at life events was inspired somewhat by the large-scale econometric findings in papers such as Oswald and Powdthavee (2008), but we had not anticipated, in such a homogenous and relatively small sample, to find powerful results.

In what follows, we test the effect of a bad life event on performance. In particular, we use the years-since-bad-life-event variable YBLE as defined before -- defined by the time point when the bad life event happened -- that we showed to be positively related to the happiness. We also use BLE -- a simpler variable for a bad life event having happened in the last 2 years -- that we showed in Table 14 to have a negative impact on the subjects' happiness.

Happiness, to reiterate, was measured at the beginning of the experiment, and thus well before any discussion of life events.

The first regression in Table 15 presents the impact of BLE on productivity. The result is striking. 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 measured to be more than 10%, and is statistically significant at better than the five per cent level.

Furthermore, as can be seen from regression 2 in Table 15, the effect of the

[19] Results of regression 1 to 3 provide a form of indirect evidence about the genuine information content in well-being survey responses.

[20] We did not want to monitor the writing of the questionnaire in any way, since a concern was to guarantee the privacy of the subjects.

elapsed-years variable YBLE is the expected one; it is positive and statistically significantly different from zero. The size of the deleterious effect of a Bad Life Event upon subjects' productivity is a declining function of the elapsed time since the event.[21]

This experimental finding is strongly redolent of Experiment 1's outcome. In the present case, however, the shock is negative rather than positive, and of course it is of a much more important kind. We here study genuinely black events in people's lives. What is found in these data is that there are long-lasting (though time-declining) consequences. Such a fact could not be checked easily in the earlier, and intrinsically short-term, lab experiment with randomized happiness allocations.

To help the comparison with earlier Experiment 1, we also report in Table 15 a regression with a log specification on the correct additions (regression 3); here BLE becomes borderline insignificant around the 5 percent level. We also report the effect for attempts and precision (regression 4 and 5). Like in the previous experiment, these regression results suggest that the negative effect of a BLE passes through the subject by them making fewer attempts rather than being less precise.

We also report as a robustness check (regression 6 of Table 15) the effect of BLE1 (where, as said before, the Bad Life Events whose year was not reported were considered older than 2 years). The coefficients on BLE and BLE1 are the same.

Finally, Table 16 depicts the effect of happiness on performance in two econometric specifications. We draw in part upon IV estimation rather than a direct approach as above, and we use bad-life-event variable BLE (regression 2) and the log of years-since-event YBLE (regression 3) as instruments. The coefficient in an OLS productivity equation is positive, although small and not statistically significant. However, it becomes large -- unsurprisingly, given the sample, significance is only at the 10 percent level -- when the level of happiness is instrumented with BLE and YBLE. The large coefficients on the instrumented happiness variables in these additions equations correspond to 5 fewer correct answers. These results are complementary to, and consistent with, the ideas of Experiment 1.

10. Conclusions

The contribution of this paper is to show that human happiness has powerful causal effects on labor productivity. Two kinds of randomized trials (denoted

[21] Interestingly, if we use the non-logged YBLE as regressor (not reported), the coefficient generally becomes non-significant, which implies a concave, declining effect with year of the BLE.

Experiments 1 and 2) have been described. The first, with approximately 270 subjects, examines the consequences of randomly-assigned happiness in the laboratory. The second, with a sample of approximately 180 different individuals, estimates the consequences of major life-shocks assigned by Nature.

In Experiment 1, some laboratory subjects have their happiness or 'positive affect' levels increased via a mood-induction procedure. Others, in a randomly selected control group, do not. The procedure leads to greater productivity in a paid piece-rate task whether we use as the independent variable either exposure to the treatment or (the induced) happiness. The productivity effect is marked; it appears in each session and can be replicated even with fairly small numbers of subjects; the effect is found equally in male and female subsamples; it is present whether we specify the exact payment procedure or simply explain to subjects that payment is performance-related. Interestingly, the effect operates though a change in output rather than the quality of the laboratory subjects' work. Happier workers' effort levels go up while their precision is unaltered. In Experiment 2, laboratory subjects fill in a questionnaire about their lives and family events. Here the aim is to detect who has, and who has not, received major happiness shocks from Nature. The study's questionnaire particularly elicits information on those who have recently suffered a close bereavement or illness in their family. Such events play the role of a real-world equivalent to the kind of happiness movements studied in Experiment 1 (plainly, for ethical reasons, we cannot in a laboratory randomly assign to some of our subjects large unhappiness shocks). As in the first experiment, a strong statistical link is found between well-being and productivity.

Various implications for economics emerge. First, research may need to pay more attention to the influence of emotions. In so far as such forces play a role in economics, they have usually been viewed, as in the literature on well-being, as a form of dependent variable. Second, more effective bridges will have to be built between scholars in applied psychology and in applied economics. Third, if happiness in a workplace carries with it a substantial return in productivity, the paper's findings have consequences for firms' promotion policies[22] and how they structure internal labor markets, and thus may be of interest to management scholars and human resources specialists. Fourth, if well-being boosts human productivity,

[22] Over and above the neoclassical pay-effort mechanisms discussed in sources such as Oswald (1984).

this raises the possibility of self-reinforcing spirals -- ones that might operate even at a macroeconomic level.

Figure 1: Reported happiness in Experiment 1

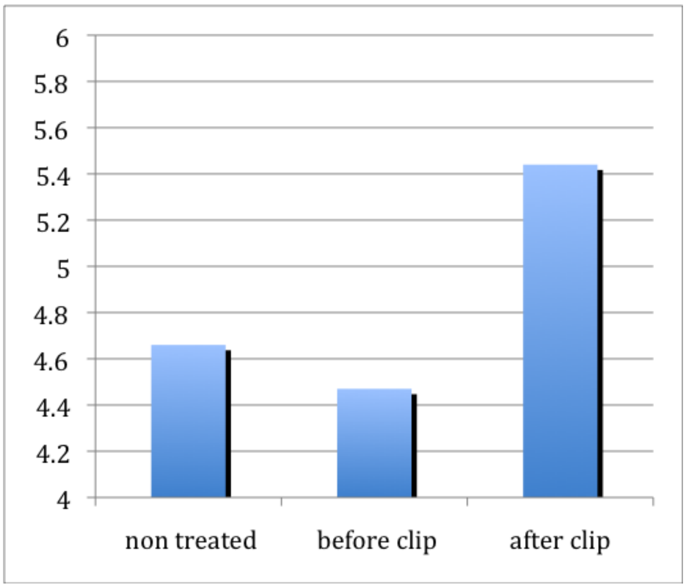

Figure 2: Number of correct additions in Experiment 1

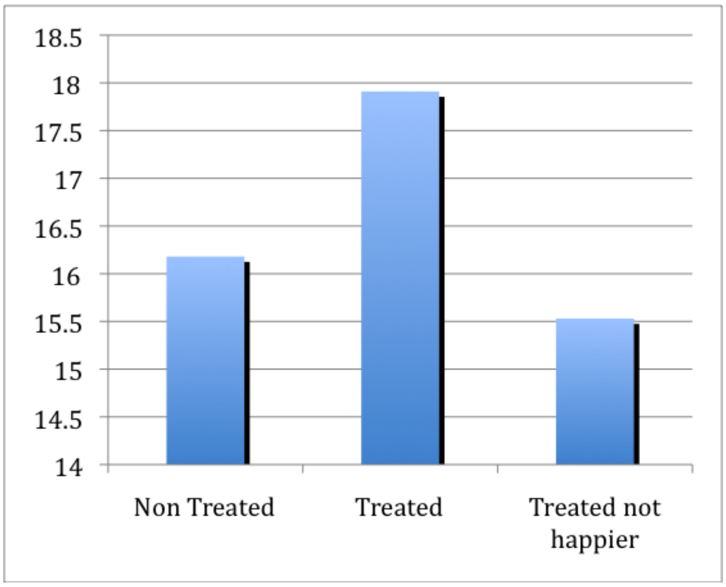

Figure 3: Performance difference between males and females (Exp. 1)

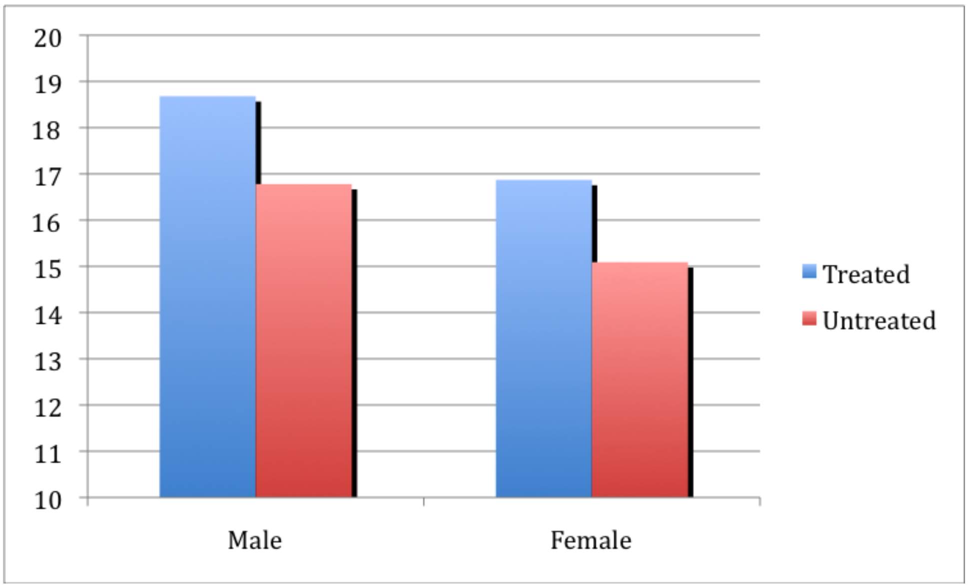

Figure 4: CDF of subjects' performances (Exp. 1)

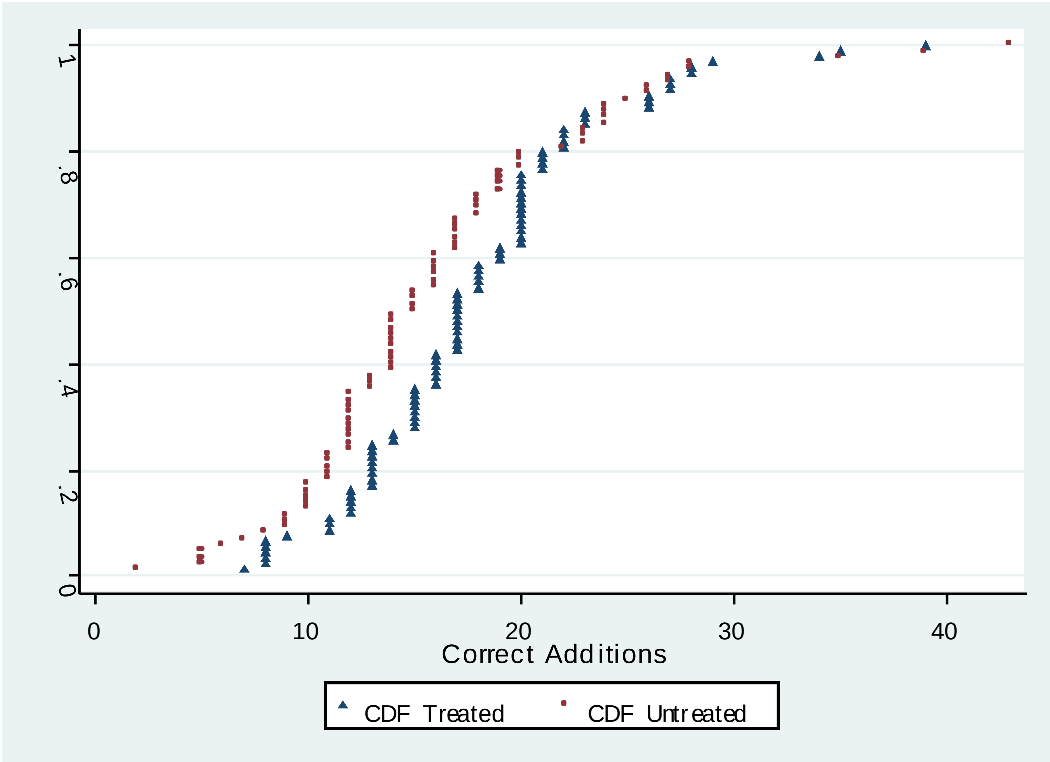

Table 1: Subject numbers for each session and day (Experiment 1)

	Day	Session with Treatment	Session without treatment
Main Sessions			
	1	24	24
	2	23	20
	3	23	24
	4	24	25
Extra Sessions	5	25	25
	6	23	21

Table 2: Data description: treated individuals (Experiment 1)

Variable	#Observations	Mean	Std Error	Min	Max
#Correct Additions	94	17.91	5.99	7	39
Happiness Before	94	4.46	0.82	3	7
Happiness After	94	5.45	0.74	3	7
GMAT MATH	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

Table 3: Data description: non-treated individuals (Experiment 1)

Variable	#Observations	Mean	Std Error	Min	Max
#Correct Additions	88	16.20	7.16	2	43
Happiness Before	88	4.64	1.20	1	7
GMAT MATH	88	3.36	1.37	1	5
High School Grades	85	0.48	0.24	0	1

Table 4: Data description: individuals treated with placebo clip[23] in Exp. 1

Variable	#Observations	Mean	Std Error	Min	Max
#Correct Additions	25	14.84	6.43	5	34
Happiness Before	24	4.37	1.05	2	6
Happiness After	24	4.29	0.99	2	7
GMAT MATH	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

Table 5: Data description: treated individuals (precise-payment case) in Exp. 1

Variable	#Observations	Mean	Std Error	Min	Max
#Correct Additions	48	19.41	8.88	0	42
Happiness Before	48	4.35	1.02	1	6
Happiness After	48	5.39	0.94	3	7
GMAT MATH	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

Table 6: Data description: non-treated individuals (precise-payment) in Exp. 1

Variable	#Observations	Mean	Std Error	Min	Max
#Correct Additions	21	18.52	7.08	7	34
Happiness Before	21	4.47	1.29	0	5
GMAT MATH	21	3.38	1.60	0	5
High School Grades	20	0.58	0.25	0.14	1

[23] 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. 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).

Table 7: Determinants of subjects' performance[24] in Experiment 1

	(1) log(Additions)	(2) log(Additions)	(3) log(Additions) *Treated only*
Treatment	0.118**		
	(0.0548)		
Change-in-Happiness		0.101**	0.0847*
		(0.0405)	(0.0495)
GMAT MATH score	0.104***	0.100***	0.0739***
	(0.0226)	(0.0226)	(0.0273)
High School Grades	0.471***	0.477***	0.428***
	(0.114)	(0.114)	(0.124)
Male	-0.0257	-0.0267	0.00675
	(0.0609)	(0.0606)	(0.0774)
Day 2	-0.0169	0.000901	-0.0170
	(0.0790)	(0.0787)	(0.0905)
Day 3	0.0975	0.106	0.131
	(0.0779)	(0.0776)	(0.0885)
Day 4	0.0118	0.00724	-0.00752
	(0.0762)	(0.0758)	(0.0895)
Constant	2.106***	2.120***	2.244***
	(0.105)	(0.102)	(0.126)
Observations	178	178	93
R-squared	0.273	0.280	0.307

Std errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

[24] Within the table, the notation *** indicates p<0.01, ** p<0.05, * p<0.1, and standard errors are given in parentheses.

Table 8: Determinants of subjects' performance [Non-logged] in Exp. 1

	(1) Additions	(2) Additions (no outliers)	(3) Additions	(4) Additions (no outliers)
Treatment	1.336	1.572**		
	(0.889)	(0.825)		
Change-in-Happiness			1.316**	1.407**
			(0.657)	(0.608)
GMAT MATH score	1.286***	1.291***	1.243***	1.244***
	(0.367)	(0.343)	(0.366)	(0.342)
High School Grades	8.284***	8.349***	8.355***	8.429***
	(1.854)	(1.710)	(1.844)	(1.701)
Male	0.824	0.606	0.828	0.607
	(0.988)	(0.919)	(0.982)	(0.914)
Day 2	0.472	-0.325	0.693	-0.0707
	(1.281)	(1.193)	(1.276)	(1.187)
Day 3	2.105*	2.330**	2.212*	2.455**
	(1.264)	(1.173)	(1.258)	(1.167)
Day 4	0.868	0.809	0.814	0.749
	(1.236)	(1.140)	(1.230)	(1.134)
Constant	6.603***	6.602***	6.680***	6.763***
	(1.697)	(1.575)	(1.657)	(1.535)
Observations	178	176	178	176
R-squared	0.245	0.283	0.253	0.290

*** p<0.01, ** p<0.05, * p<0.1 Std errors in parentheses

Table 9: Determinants of attempts in Experiment 1

	(1) Log(Attempts)	(2) Log(Attempts)
Treatment	0.0911**	
	(0.0417)	
Change-in-Happiness		0.0812***
		(0.0308)
GMAT MATH score	0.0758***	0.0733***
	(0.0172)	(0.0171)
High School Grades	0.372***	0.377***
	(0.0869)	(0.0863)
Male	-0.0165	-0.0170
	(0.0463)	(0.0460)
Day 2	0.0198	0.0340
	(0.0600)	(0.0597)
Day 3	0.133**	0.140**
	(0.0592)	(0.0589)
Day 4	0.0767	0.0732
	(0.0579)	(0.0576)
Constant	2.432***	2.441***
	(0.0795)	(0.0776)
Observations	178	178
R-squared	0.279	0.288

*** p<0.01, ** p<0.05, * p<0.1 Std errors in parentheses

Table 10: Determinants of subjects' precision

(i.e. ratio of correct answers) in Experiment 1

	(1) Correct/ Attempt	(2) Correct/ Attempt
Treatment	0.0128	
	(0.0185)	
Change-in-Happiness		0.0102
		(0.0138)
GMAT MATH score	0.0165**	0.0162**
	(0.00765)	(0.00767)
High School Grades	0.0656*	0.0663*
	(0.0386)	(0.0386)
Male	0.00152	0.00134
	(0.0206)	(0.0206)
Day 2	-0.0268	-0.0249
	(0.0267)	(0.0267)
Day 3	-0.0201	-0.0192
	(0.0263)	(0.0263)
Day 4	-0.0507*	-0.0512**
	(0.0258)	(0.0257)
Constant	0.753***	0.755***
	(0.0354)	(0.0347)
Observations	178	178
R-squared	0.095	0.096

Std. errors in parentheses *** p<0.01,

Table 11: The effects of a placebo film and explicit payment in Experiment 1

VARIABLES	(1) Log Additions	(2) Log Additions	(3) Log Attempts	(4) Log Attempts	(5) Correct/ Attempts
Treatment	0.123**	0.112**	0.0935**	0.0986**	-0.000874
	(0.0569)	(0.0496)	(0.0458)	(0.0399)	(0.0170)
Explicit Payment	0.105	0.0747	0.0119	0.0262	0.0237
	(0.0952)	(0.0557)	(0.0767)	(0.0446)	(0.0191)
Treatment*ExPa	-0.0461		0.0216		
	(0.117)		(0.0939)		
Placebo film	-0.0553	-0.0612	-0.0761	-0.0733	0.000595
	(0.0879)	(0.0865)	(0.0707)	(0.0696)	(0.0297)
GMAT Math	0.0897***	0.0894***	0.0751***	0.0752***	0.0142**
	(0.0186)	(0.0185)	(0.0148)	(0.0148)	(0.00632)
High School	0.455***	0.458***	0.377***	0.375***	0.0664**
	(0.0977)	(0.0971)	(0.0786)	(0.0780)	(0.0333)
Male	0.0308	0.0299	0.0206	0.0210	0.0129
	(0.0501)	(0.0500)	(0.0404)	(0.0402)	(0.0172)
Constant	2.147***	2.153***	2.464***	2.462***	0.736***
	(0.0782)	(0.0769)	(0.0624)	(0.0614)	(0.0262)
Observations	268	268	269	269	269
R-squared	0.247	0.247	0.259	0.259	0.062

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

Table 12: Subject numbers for each session and day: Experiment 2

Day / Session	1	2
1	19	19
2	25	19
3	25	26
4	23	24

Table 13: Data description in Experiment 2

(where Bad Life Event is defined as 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	179	0.08	0.25	0	1
Years From Bad Life Event	93	2.30	1.70	0	5
Male	170	0.5	0.5	0	1
Age	169	19.49	1.48	18	30

Table 14: Happiness equations in Experiment 2 with a variable for a Bad Life Event (where Bad Life Event =BLE is defined as family illness or bereavement)

	(1) Happiness	(2) Happiness	(3) Happiness	(4) Happiness
BLE less than 1 year ago	-0.88**			
	(0.35)			
BLE 1 year ago	-0.30			
	(0.21)			
BLE 2 year ago	-0.86**			
	(0.34)			
BLE 3 year ago	0.18			
	(0.36)			
BLE 4 year ago	-0.40			
	(0.31)			
BLE 5 year ago	-0.45			
	(0.35)			
YBLE[25]		0.13**		
		(0.049)		
BLE in the last 2 years			-0.43**	
			(0.18)	
BLE1[26]				-0.39**
				(0.17)
Male	0.11	0.069	0.081	0.20
	(0.17)	(0.17)	(0.17)	(0.16)
Age	-0.094	-0.088	-0.092	-0.064
	(0.069)	(0.069)	(0.069)	(0.052)
High School Gr.	-0.23	-0.17	-0.16	-0.14
	(0.31)	(0.31)	(0.31)	(0.30)
Session 1.2	-0.19	-0.15	-0.13	-0.15
	(0.31)	(0.31)	(0.31)	(0.29)
Session 1.3	0.089	0.11	0.080	-0.058
	(0.31)	(0.31)	(0.31)	(0.29)
Session 1.4	0.38	0.37	0.36	0.41
	(0.33)	(0.32)	(0.33)	(0.30)
Session 2.1	0.15	-0.0019	-0.045	0.11
	(0.40)	(0.39)	(0.39)	(0.36)
Session 2.2	0.45	0.48	0.48	0.29
	(0.35)	(0.34)	(0.35)	(0.31)
Session 2.3	0.0013	0.077	0.11	-0.0081
	(0.33)	(0.32)	(0.33)	(0.30)
Session 1.2	-0.059	0.028	-0.088	-0.090
	(0.32)	(0.33)	(0.32)	(0.31)
Constant	6.85***	6.33***	6.71***	6.11***
	(1.42)	(1.43)	(1.42)	(1.12)
Observations	142	142	142	164
R-squared	0.163	0.113	0.107	0.099

*** p<0.01, ** p<0.05, * p<0.1 Standard errors in parentheses

[25] The variable YBLE is equal to Log(year of the Bad Life Event+0.01) if the bad life event occurred, and Log(N+0.01) otherwise, where we set N=10. Very similar results are obtained by setting N=6.

[26] BLE1 is set equal to 1 when a bad life event happened in the last two years and it is set equal to 0 when no bad life event happened or the year is missing.

Table 15: Determinants of subjects' performance in Experiment 2
(where Bad Life Event =BLE is defined as family illness or bereavement)

VARIABLES	(1) Additions	(2) Additions	(3) Additions (log)	(4) Attempt	(5) Correct/ Attempts	(6) Additions
BLE in the last 2 years	-2.31**		-0.12*	-1.92*	-0.030	
	(1.12)		(0.067)	(1.13)	(0.022)	
BLE1[27]						-2.05*
						(1.04)
YBLE[28]		0.65**				
		(0.31)				
Male	-0.77	-0.83	-0.048	-0.88	0.0040	-0.84
	(1.15)	(1.15)	(0.068)	(1.16)	(0.023)	(1.03)
Age	0.30	0.32	-0.0023	0.41	-0.0027	-0.14
	(0.44)	(0.44)	(0.026)	(0.44)	(0.0087)	(0.33)
High School Gr.	3.75*	3.68*	0.24*	5.42***	-0.018	3.21*
	(2.04)	(2.04)	(0.12)	(2.06)	(0.041)	(1.92)
GMAT MATH	1.22***	1.23***		1.23***	0.0079	0.98***
	(0.38)	(0.38)	(0.023)	(0.38)	(0.0076)	(0.35)
Session 1.2	-1.44	-1.54	-0.098	-0.41	-0.054	-1.31
	(1.96)	(1.95)	(0.12)	(1.97)	(0.039)	(1.83)
Session 1.3	1.44	1.57	-0.030	2.17	-0.046	1.22
	(2.00)	(2.00)	(0.12)	(2.01)	(0.040)	(1.86)
Session 1.4	-0.39	-0.42	-0.018	0.45	-0.030	-0.032
	(2.05)	(2.05)	(0.12)	(2.07)	(0.041)	(1.89)
Session 2.1	2.89	3.12	0.14	4.45*	-0.042	2.87
	(2.49)	(2.49)	(0.15)	(2.51)	(0.050)	(2.28)
Session 2.2	2.85	2.80	0.14	2.82	0.014	2.11
	(2.19)	(2.19)	(0.13)	(2.21)	(0.044)	(1.97)
Session 2.3	1.45	1.25	0.044	2.23	-0.029	3.01
	(2.07)	(2.05)	(0.12)	(2.08)	(0.041)	(1.86)
Session 2.4	4.08**	4.62**	0.19	4.38**	0.020	4.30**
	(2.02)	(2.08)	(0.12)	(2.04)	(0.040)	(1.93)
Constant	5.79	3.85	2.60***	5.35	0.90***	15.2**
	(9.04)	(9.10)	(0.54)	(9.12)	(0.18)	(7.11)
Observations	142	142	142	142	142	164
R-squared	0.218	0.218	0.188	0.228	0.078	0.184

*** p<0.01, ** p<0.05, * p<0.1 Standard errors in parentheses

[27] BLE1 is set equal to 1 when a bad life event happened in the last two years and it is set equal to 0 when no bad life event happened or the year is missing.

[28] The variable YBLE is equal to Log(year of the Bad Life Event+0.01) if the bad life event occurred, and Log(N+0.01) otherwise, where we set N=10. Very similar results are obtained by setting N=6.

Table 16: Performance equations and instrumented happiness in Experiment 2

	(1) Additions OLS	(2) Additions IV (BLE)	(3) Additions IV (YBLE)
Happiness	0.47	5.27*	4.98*
	(0.51)	(3.11)	(2.86)
Male	-0.90	-1.61	-1.56
	(1.05)	(1.51)	(1.46)
Age	-0.12	0.79	0.76
	(0.33)	(0.62)	(0.60)
High School	3.14	4.10	4.07*
	(1.94)	(2.51)	(2.45)
GMAT MATH	1.09***	1.62***	1.60***
	(0.36)	(0.51)	(0.49)
Session 1.2	-1.49	-0.77	-0.82
	(1.85)	(2.45)	(2.39)
Session 1.3	1.16	1.23	1.23
	(1.87)	(2.44)	(2.39)
Session 1.4	-0.86	-2.22	-2.17
	(1.87)	(2.51)	(2.44)
Session 2.1	2.91	3.21	3.19
	(2.30)	(3.05)	(2.98)
Session 2.2	1.81	0.37	0.50
	(1.99)	(3.02)	(2.91)
Session 2.3	2.56	0.97	0.97
	(1.86)	(2.51)	(2.45)
Session 2.4	3.71*	4.52*	4.45*
	(1.92)	(2.53)	(2.47)
Constant	11.9	-30.7	-28.7
	(7.88)	(24.4)	(22.7)
Observations	164	142	142
R-squared	0.168	.	.

*** p<0.01, ** p<0.05, * p<0.1 Standard errors in parentheses

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APPENDIX: PART 1

Replication of the findings on subsamples

Table A1: Treatment Dates

Experiment 1 was at first carried out on four separate days, as follows:

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

Treatment 0 is the control treatment without a video clip and treatment 1 includes a video clip. Sessions 1 and 2 were undertaken in term 3 of the University of Warwick academic year 2007-8, while sessions 3 and 4 were undertaken in term 1 of the 2008-9 academic year. Since they are separated by a gap of approximately 4 months, we might wish to check for significant changes across the time between sessions 1-2 and sessions 3-4. The aggregate variables results broken down by session are as follows:

Table A2: Summary Statistics by Treatment

Session	Additions	Log Additions	Additions Male	Additions Female	Happy before	Happy after	Enjoy clip
1 Treatment 0	15.38**	1.17	14.88**	16.83	4.54	na	na
1 Treatment 1	18.21**	1.23	18.26**	18.00	4.54	5.63	5.96
2 Treatment 0	16.85	1.18	19.41	13.00*	4.45	na	na
2 Treatment 1	16.48	1.19	16.36	16.58*	4.43	5.22	5.74
3 Treatment 0	16.26*	1.16	15.75*	17.14	4.79	na	na
3 Treatment 1	19.52*	1.27	20.42*	18.11	4.48	5.39	5.83
4 Treatment 0	16.04	1.15	18.07	14.36	4.92	na	na
4 Treatment 1	17.72	1.22	19.6	15.92	4.36	5.44	6.21

The key column is perhaps *log additions* (the log of the number of correct additions) which somewhat smoothes outliers in the number of correctly answered numerical additions. The data for sessions 1-2 are very similar to those from sessions 3-4. Importantly, the pattern of results seems strongly consistent across sessions. The only exception comes in session 2 where the raw number of additions does not rise moving from control treatment 0 to happiness treatment 1. As explained, this is down to one outlier. Using logs brings the results into line with those from the other sessions.

We put an asterisk when the difference between treated and untreated groups is statistically significant. In particular, we have that for session 1 (21 May 2008) and session 3

(10 October 2008) the difference for the entire pool is already statistically significant at *p*-values 0.047 and 0.052 respectively. When we split the group into males and females, we note that, even in these small subsamples of raw data, there is a statistically significant finding individually in 3 out of 8 sub-cases.

Alternatively, we also regressed the key variables for all four sessions individually:

Table A3: Session Regressions (Log Additions)

	(1) Log Add.	(2) Log Add.	(3) Log Add.	(4) Log Add.	(5) Log Add.	(6) Log Add.	(7) Log Add.	(8) Log Add.
Treatment	0.129		0.0931		0.184		0.0979	
	(0.0889)		(0.124)		(0.127)		(0.118)	
GMAT score	0.0799*	0.0859*	0.115**	0.110**	0.139***	0.135***	0.0739	0.0722
	(0.0472)	(0.0453)	(0.0507)	(0.0510)	(0.0434)	(0.0448)	(0.0473)	(0.0469)
High Sc. Gr.	0.482**	0.486**	0.398	0.386	0.277	0.332	0.657***	0.652***
	(0.198)	(0.192)	(0.261)	(0.266)	(0.262)	(0.262)	(0.239)	(0.236)
Male	-0.0729	-0.0373	0.113	0.0985	-0.153	-0.150	-0.0258	-0.0350
	(0.111)	(0.110)	(0.127)	(0.126)	(0.134)	(0.136)	(0.136)	(0.133)
Ch.-in-happ.		0.126**		0.0256		0.0993		0.0980
		(0.0585)		(0.112)		(0.102)		(0.0792)
Constant	2.220***	2.165***	2.022***	2.093***	2.219***	2.256***	2.122***	2.128***
	(0.187)	(0.185)	(0.218)	(0.198)	(0.184)	(0.184)	(0.170)	(0.163)
Observations	48	48	40	40	41	41	49	49
R-squared	0.286	0.323	0.288	0.278	0.336	0.315	0.264	0.278

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

Regression (1) considers *log additions* from session 1 regressed on treatment, with (2) instead using change-in-happiness. This is in general a better measure of the hedonic impact since it allows for those subjects who did not gain in happiness from watching the clip. Columns (3) and (4) are the respective regressions for session 2, (5) and (6) for session 3, and (7) and (8) for session 4. We might also consider merging sessions 1 and 2, and merging sessions 3 and 4:

Table A4: Grouped Session Regressions (Log Additions)

VARIABLES	(1) Log Additions	(2) Log Additions	(3) Log Additions	(4) Log Additions
Treatment	0.0989		0.139	
	(0.0712)		(0.0848)	
GMAT MATH score	0.100***	0.0987***	0.111***	0.108***
	(0.0333)	(0.0330)	(0.0316)	(0.0318)
High School Grades	0.458***	0.462***	0.468***	0.479***
	(0.157)	(0.155)	(0.169)	(0.169)
Male	0.0299	0.0309	-0.0658	-0.0720
	(0.0797)	(0.0789)	(0.0918)	(0.0916)
Change-in-happiness		0.0990*		0.0982
		(0.0535)		(0.0617)
Constant	2.091***	2.096***	2.147***	2.174***
	(0.135)	(0.130)	(0.122)	(0.118)
Observations	88	88	90	90
R-squared	0.268	0.281	0.274	0.273

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

In Table A4, regressions (1) and (2) group together sessions 1 and 2. Similarly, regressions (3) and (4) group together sessions 3 and 4. As in Table A3, the first regression in each pair considers *Treatment*.

APPENDIX: PART 2

Checking the effects of a placebo film and of exact numerical payment

This describes Day 5 (3/12/08): session 1 (placebo, 25 subjects), session 2 (explicit payment and treatment, 25 subjects) and Day 6 (14/01/09), session 1 (explicit payment and no treatment), session 2 (explicit payment and treatment). Table A5 provides the results in terms of average correct additions.

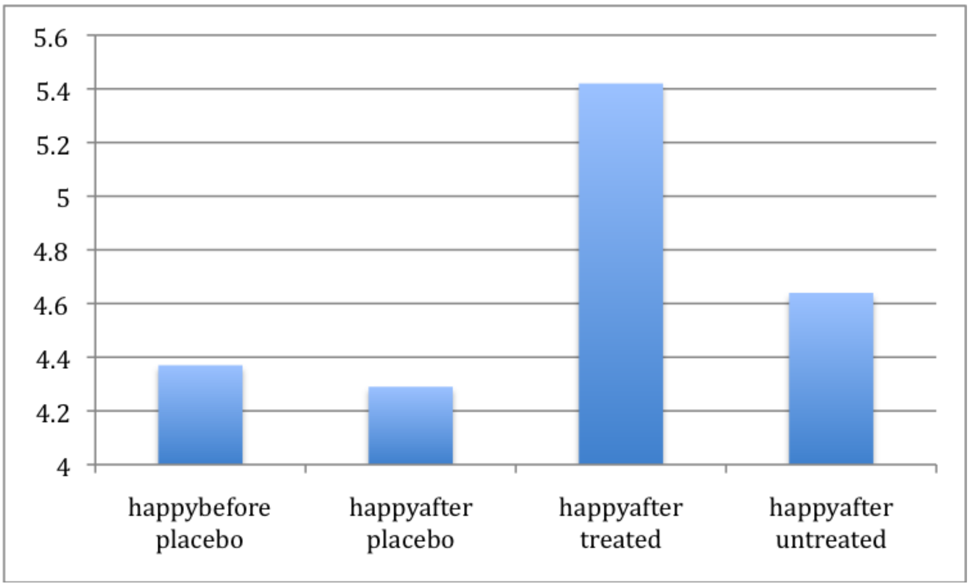
Table A5: Summary Statistics by Treatment for Explicit Payment and Placebo

Description	Day /Session	Additions	Log Additions
Explicit payment Treatment 0	6 / 1	18.52	2.84
Explicit payment Treatment 1	5/2 and 6/2	19.41	2.89
Treatment Placebo	5/1	14.84	2.61

Placebo film

In the figure below we present the level of reported happiness after and before the placebo. The placebo film had the effect of very slightly reducing subjects' happiness but the two levels are not statistically different (p=0.39). The level of reported happiness after the placebo is slightly lower than the one of the non treated group (p=0.093), and statistically lower than the one in the treated group (p<0.001). All in all, the placebo film does not have a statistically significant impact on the level of self-reported happiness when compared against the non treated group.

The placebo film has, if anything, a negative impact on performances, although this difference is statistically non-significant (p=0.19). This finding is shown in table A5, where we can see that placebo treatment has no effect on additions, attempts or precision.



Explicit payment

Here we announced that each subject would be rewarded with £0.25 for each correct addition. In table A5 we introduce the dummy Explicit Payment, this is equal to 1 where the payment is specified, 0 otherwise. We interact this term with the variable Treatment to check whether this could threaten our previous finding. If the interacted term is negative and significant, this would imply that happiness has less or no impact when the payment is specified. From the first regression on table A5, we note that the interacted term Treatment*ExPayment is not statistically different from 0 (p=0.60), while the payment when introduced alone (2nd regression in table A5) is positive, although not significant at the 10 percent level. Similar results are obtained when we consider (log) attempts as a dependent variable (3rd and 4th regressions). Finally we also note that explicit payment has a positive but insignificant effect on precision (5th regression).

APPENDIX: PART 3

Description of Procedures

This part of the appendix includes a full set of subject instructions, a copy of the GMAT MATH-style test, and the questionnaire.

Instructions

[**bold** = only for the comedy clip/placebo treatment; *italics* = explicit payment variant; X directly addresses; Y, Z, etc. are assistants. Parts in square brackets are descriptive.]

[X invites subjects to enter room **while Y sets up the video clip]**

Welcome to the session. My name is X, and working with me today are Y, Z, etc. Many thanks for attending today. You will be asked to perform a small number of very minor tasks and will be paid both a show-up fee and an amount based on how you perform, **but first we would like to ask you to watch a video clip.** Please do not talk to each other at any stage in the session. If you have any questions please raise your hands, but avoid distracting the others in the room.

Z will now guide you to the seats at the front of the room directly in front of the projector, while Y prepares the video clip. Please make yourselves comfortable: the clip will last about 10 minutes and I will have more instructions for you afterwards.

[10 minutes: video clip]

Thanks for watching. Z will now distribute ID cards to you and you are asked to sit at the computer corresponding to the ID number. Everything is done anonymously – your performance will simply be recorded based on the ID card, and not your names. You will find some paper and a pen next to your computer – use them if you wish, and raise your hand if you wish to request additional paper. Please do not use calculators or attempt to do anything other than answer the questions through mental arithmetic. If we observe any form of cheating it will invalidate your answers and you will be disqualified, and therefore receive only the show-up fee.

For the first task you will have 10 minutes to add a sequence of numbers together and enter your answers in the column labelled “answer”. To remind you, you will be paid based on the number of correct answers that you produce. *at the rate of £0.25 per correct answer* When the ten minutes are over I will ask you to stop what you are doing and your results will be saved.

Next look at your screens: you will find that a file called “Numberadditions.xls” is open but minimized on your screen. Please now maximize the file by clicking on the tab. You have ten minutes starting now.

[10 minutes: number additions]

Please stop what you are doing, your answers will now be saved. Y and Z will now visit your computers and place a sheet faced down next to your keyboards. Please do not turn over the sheet until I ask.

[Y and Z move to terminals, placing question sheets faced down]

For the second task we would like you answer a small number of questions. You can maximize the file on your computer labelled “GMAT MATH.xls” and you will once again see a column labelled answers. In this column you will have to enter a letter from (a) to (e), corresponding to a multiple-choice answer to the sheet which is faced-down in front of you. Once again, I remind you that you will be paid based on the number of correct answers *at the rate of £0.50 per correct answer*. You have 5 minutes to attempt these questions, please turn over the sheets and begin.

[5 minutes: GMAT MATH-style test]

Please stop what you are doing, your answers will now be saved. You should next open the final document: a questionnaire that you are asked to complete. You will be given 10 minutes to complete this, though if you need additional time we can extend this deadline indefinitely. Please answer as truthfully as you can and feel free to raise your hands if anything is unclear. To stress, where you are asked to input a number from 1 to 7, “7” is the high number and “1” is the low one.

[10 minutes: questionnaire]

Hopefully you have all had a chance to complete the questionnaire. If you need more time, then please raise your hand. Otherwise we will save your questionnaire replies.

The central computer has calculated your payments. Please remain at your computer for the time being. I will ask you to approach the front in order of your ID numbers and you will need to sign a receipt for your payments and to hand in both your ID cards and the test document before receiving payment. Many thanks for taking part in today’s session.

[Test documents destroyed, ID cards collected, receipts signed and payments handed out]

FOR REFEREES ONLY

NOT FOR PUBLICATION

GMAT MATH-style Test

Questions

Please answer these by inserting the multiple choice answer a, b, c, d or e into the GMAT MATH spreadsheet on your computer.

1. Harriet wants to put up fencing around three sides of her rectangular yard and leave a side of 20 feet unfenced. If the yard has an area of 680 square feet, how many feet of fencing does she need?

a) 34
b) 40
c) 68
d) 88
e) 102

2. If $x + 5y = 16$ and $x = -3y$, then $y =$

a) -24
b) -8
c) -2
d) 2
e) 8

3. If "basis points" are defined so that 1 percent is equal to 100 basis points, then 82.5 percent is how many basis points greater than 62.5 percent?

a) .02
b) .2
c) 20
d) 200
e) 2,000

4. Which of the following best completes the passage below?

In a survey of job applicants, two-fifths admitted to being at least a little dishonest. However, the survey may underestimate the proportion of job applicants who are dishonest,

because——.

a) some dishonest people taking the survey might have claimed on the survey to be honest.

b) some generally honest people taking the survey might have claimed on the survey to be dishonest.

c) some people who claimed on the survey to be at least a little dishonest may be very dishonest.

d) some people who claimed on the survey to be dishonest may have been answering honestly.

e) some people who are not job applicants are probably at least a little dishonest.

5.People buy prestige when they buy a premium product. They want to be associated with something special. Mass-marketing techniques and price-reduction strategies should not be used because ——.

a) affluent purchasers currently represent a shrinking portion of the population of all purchasers.

b) continued sales depend directly on the maintenance of an aura of exclusivity.

c) purchasers of premium products are concerned with the quality as well as with the price of the products.

d) expansion of the market niche to include a broader spectrum of consumers will increase profits.

e) manufacturing a premium brand is not necessarily more costly than manufacturing a standard brand of the same product.

Questionnaire in Experiment 1: The Randomized-Happiness Laboratory Experiment

Questionnaire for Treatment 1.

Questionnaire
Please insert your answers into the shaded boxes to the right

Details
What is your age?
Are you a 1st year, 2nd year, 3rd year, graduate student, or other? (1/2/3/G/O)
What is your gender? (M/F)

The Clip
How much did you enjoy the clip shown at the beginning? (1-7)
Note: 1 is completely disliked, 2 very disliked, 3 is fairly disliked, 4 is neither enjoyed nor disliked, 5 is fairly enjoyed, 6 is very enjoyed, 7 is completely enjoyed

Happiness
How would you rate your happiness before seeing the clip? (1-7)
Note: 1 is completely sad, 2 is very sad, 3 is fairly sad, 4 is neither happy nor sad, 5 is fairly happy, 6 is very happy, 7 is completely happy

Did the clip shown at the beginning make you feel happier? (yes/no)
IF SO:
How would you rate your happiness after seeing the clip (1-7)?
Note: 1 is completely sad, 2 is very sad, 3 is fairly sad, 4 is neither happy nor sad, 5 is fairly happy, 6 is very happy, 7 is completely happy

School Record
Have you taken GSCE or equivalent in maths? (yes/no)
IF SO:
What was the highest grade possible for this course? (A/A*/etc.)
What was your grade?
Give a percentage if you know it

Have you taken A-level or equivalent in maths? (yes/no)
IF SO:
What was the highest grade possible for this course?
What was your grade?
Give a percentage if you know it

How many school level qualifications have you taken (including GCSEs, A-levels and equivalent)?
How many of these qualifications were at the best grade possible? (e.g. A* in GCSE, A is A-level, etc.)

University Record
Are you currently or have you ever been a student (yes/no)
If yes, which degree course(s)?
If you are a second or third year student what class best describes your overall performance to date? (1/2.1/2.2/3/Fail)

Note: For days 5-6 we also added the following questions to the end of the questionnaire for the treated group (including the placebo treatment): (1) Did you try your best when asked to add numbers? (2) If so, why? If not, why not? (3) Did you feel that first observing the video clip made you better or worse at adding up numbers? (4) Can you say why you believe that?

Questionnaire for Treatment 0 (untreated group).

Questionnaire
Please insert your answers into the shaded boxes to the right

Details

What is your age?	
Are you a 1st year, 2nd year, 3rd year, graduate student, or other? (1/2/3/G/O)	
What is your gender? (M/F)	

Happiness

How would you rate your happiness at the moment? (1-7)	

Note: 1 is completely sad, 2 is very sad, 3 is fairly sad, 4 is neither happy nor sad, 5 is fairly happy, 6 is very happy, 7 is completely happy

School Record

Have you taken GSCE or equivalent in maths? (yes/no)	
IF SO:	
What was the highest grade possible for this course? (A/A*/etc.)	
What was your grade?	
Give a percentage if you know it	
Have you taken A-level or equivalent in maths? (yes/no)	
IF SO:	
What was the highest grade possible for this course?	
What was your grade?	
Give a percentage if you know it	
How many school level qualifications have you taken (including GCSEs, A-levels and equivalent)?	
How many of these qualifications were at the best grade possible? (e.g. A* in GCSE, A is A-level, etc.)	

University Record

Are you currently or have you ever been a student (yes/no)	
If yes, which degree course(s)?	
If you are a second or third year student what class best describes your overall performance to date? (1/2.1/2.2/3/Fail)	

Questionnaire in Experiment 2: The Real-life Happiness Shock Experiment

Asked at the start of the session:

Happiness

How would you rate your happiness at the moment? (1-7)

Note: 1 is completely sad, 2 is very sad, 3 is fairly sad, 4 is neither happy nor sad, 5 is fairly happy, 6 is very happy, 7 is completely happy

Asked at the end of the session:

Questionnaire

Please insert your answers into the shaded boxes to the right

Details

What is your age?

Are you a 1st year, 2nd year, 3rd year, graduate student, or other? (1/2/3/G/O)

What is your gender? (M/F)

School Record

Have you taken GSCE or equivalent in maths? (yes/no)

IF SO:

What was the highest grade possible for this course? (A/A*/etc.)

What was your grade?

Give a percentage if you know it

Have you taken A-level or equivalent in maths? (yes/no)

IF SO:

What was the highest grade possible for this course?

What was your grade?

Give a percentage if you know it

How many school level qualifications have you taken (including GCSEs, A-levels and equivalent)?

How many of these qualifications were at the best grade possible? (e.g. A* in GCSE, A is A-level, etc.)

University Record

Are you currently or have you ever been a student (yes/no)

If yes, which degree course(s)?

If you are a second or third year student what class best describes your overall performance to date? (1/2.1/2.2/3/Fail)

General Questions

Life has its ups and downs. During the last 5 years, have you experienced any of the following events (yes/no*).*

If yes, please could you indicate how many years ago in the second column to the right. For example, if this happened this year enter 0, for a year ago enter 1, etc. up to 5 years ago.

	yes/no	number of years ago
A bereavement in your close family? (e.g. parent/guardian, sibling)		
A bereavement in your extended family? (e.g. close grandparent, close aunt/uncle, close cousin, close friend)		
A parental divorce?		
A serious (potentially life-changing or life-threatening) illness in your close family?		

	yes/just averagely good/no	number of years ago
Has anyone close to you had anything really good happen to them within the last 5 years? (yes/just averagely good/no)		