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Revisiting a remedy against the chain of unkindness

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Abstract

Previous experiments observe a chain of unkindness: unkindly treated people treat an innocent third party unkindly. As a remedy, it has been proposed that the unkindly treated person engages in emotional regulation by writing a letter to the unkind person. Indeed, subjects who received little money were willing to leave more to a third person when they were writing a letter rather than waiting.

Here, we examine whether emotional regulation is indeed behind this observation. In line with emotional regulation, we find that letter writing also leads to more giving if the person is treated unkindly by being assigned to a frustrating rather than a pleasant job. Being able to write, however, does not affect self-reported happiness differently from having to wait. Even more strikingly, subjects assigned to pleasant jobs also give more when writing rather than waiting. This is not consistent with emotional regulation.

Keywords: experimental economics, chain of unkindness, emotional closure, cooling down

JEL-Codes: D91 C91 D03

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1 Introduction

Anecdotal as well as experimental evidence suggests that people pass on unkind behavior (Ben-Ner et al., 2004; Gray et al., 2014; Diekmann, 2004; Leimgruber et al., 2014; Mujcic and Leibbrandt, 2017). Consider a manager whose supervisor has just ordered her to work overtime. Perhaps, she will let out her frustration by afterwards leaving no coffee for her colleagues. This passing on of unkind behavior, also known as negative generalized reciprocity (Nowak and Sigmund, 2005; Stanca, 2009), may create a bad atmosphere in the organization and ultimately reduce productivity.

One possible reason for the passing on of unkind behavior is the lack of emotional regulation, e.g., the manager cannot ‘pull herself together’. Indeed, different emotional regulation techniques have been tried out by Strang et al. (2016) in an experiment as a remedy against chains of unkindness. Among those, only one measure was found to significantly improve self-reported happiness of a frustrated subject B who had received little money from some subject A: if B writes a letter to A. More importantly, frustrated subjects B who were given the opportunity to write a letter to A passed on more money to a third completely unrelated subject C. This suggests that writing a letter to A helps B regulate emotions and may stop chains of unkind behavior.

While the suggested remedy has been successful in the lab, it cannot be readily implemented in organizations. For example, the senders of the letters are anonymous in the experiment. Members of organizations, however, typically cannot complain about a specific bad treatment and expect to stay anonymous. Still, something may be learned from letter writing. If its success is actually due to emotional regulation, one could devise a practically more viable emotional regulation technique.

This is why we critically examine in this paper whether emotional regulation is the reason for why letter writers give more. We do so in three steps.

First, emotional regulation should work even if the decision of the unkindly treated person B does not have the same domain as that of the unkind person A. While the supervisor, for example, frustrates the manager by ordering her to work longer, the manager has to decide whether to take out her frustration on others by using up all coffee.

Here, we examine whether letter writing is also effective when decision domains differ. In our experiment, one participant (A) in each session is given the choice whether and which of the other subjects (B) have to engage in a pleasant or an unpleasant task. Then, depending on the treatment, subjects wait or have the opportunity to write a letter. Finally, we measure how kind these subjects behave toward an innocent third person (C) by letting them play a dictator game. Despite the change of decision domain, we find that letter writing increases the amount left by unkindly treated Bs to C (Result 1). The effect is similar in size to that by Strang et al. (2016), where both decisions were about sharing money.

Second, letter writing might not be effective as an emotional regulation technique but work as a signal about a social norm.¹ The unkind behavior of subject A may be taken by a waiting subject B as an indication that it is socially acceptable to behave unkindly and pass on little money in this setting. Being given the opportunity by the experimenter

¹Friebel and Schnedler (2011) or van der Weele (2012) devise formal models in which managers signal social norms and Krupka and Weber (2013) provide evidence that subjects’ behavior in experiments can be explained by what other subjects think is appropriate.

to write a letter to A, on the other hand, could be seen as an indication that A did not act properly. If this explanation is true, an intervention in practice should try to establish a different social norm rather than help emotional regulation.

Being treated unkindly presumably frustrates the subject. If letter writing helps with dealing with this frustration then reported happiness should increase more as a result of writing than waiting—just as in Strang et al. (2016). While we find that having to do an unpleasant job indeed frustrates subjects, the increase in reported happiness after writing differs neither economically nor statistically from that after waiting (Result 2). This observation is at odds with letters being used for emotional regulation but consistent with the explanation that being given the opportunity to write a letter indicates what is socially acceptable. Drawing a final conclusion from this observation, however, is difficult. Although we measure emotional regulation exactly as Strang et al. (2016), this measure may fail to capture the right emotion. Moreover, the measurement is a self-reported subjective assessment without any real consequences.

The emotional regulation explanation can be put to a more serious test by examining behavior of kindly treated subjects. These subjects have no need for emotional regulation. We hence do not expect them to give more after writing rather than waiting. If anything, they should give less. A similar argument can be made about norms. If being asked to write a letter is no news to a kindly treated subject or if it even signals that focusing on one's own benefit is 'ok', then B's who are asked to write letters should give the same or less than those who are waiting.

Interestingly, we find that kindly treated subjects who are asked to write a letter give *more* than those who wait (Result 3). This is inconsistent with both explanations of why the intervention may work. It also reveals that this intervention does not address a specific need of unkindly treated subjects and is thus no good starting point for searching for a practically applicable measure against chains of unkindness.

Like third-party punishment experiments (e.g., Fehr and Fischbacher, 2004; Rand and Nowak, 2013; Nelissen and Zeelenberg, 2009) our design involves a sequence of three players. The crucial difference is that the third 'player' *C* is neither active nor present in our experiment. More importantly, our key question is not how *C*'s possible punishment affects *A*'s behavior toward *B*, but when and why letter writing affects *B*'s behavior toward *C*.

The main aim of our paper is to add to the literature on chains of unkindness and their prevention. This literature studies whether negatively affected people behave negatively toward an independent third party—a phenomenon also referred to as downstream indirect reciprocity (Mujcic and Leibbrandt, 2017) or generalized reciprocity (Herne et al., 2013). Mujcic and Leibbrandt (2017), for example, find that people who have been let through at a car park crossing are more likely to let other people through later and Leimgruber et al. (2014) observe that children who receive a sticker from another child are more likely to give away a sticker to a third child. Others show that subjects behave more non-cooperatively after a non-cooperative experience (Rankin and Taborsky, 2009) or that subjects pass on less money in a dictator game after receiving little in a dictator game themselves (Diekmann, 2004).² These and many other studies (Ben-Ner et al., 2004; Gray

²In line with these contributions, we measure the unkind behavior of *B* using a dictator game. The wide-spread use of the dictator game (Engel, 2011) has been criticized as 'searching near the lamppost' (Oechssler, 2010). While there is some debate on whether giving in the dictator game actually measures

et al., 2014; Herne et al., 2013; Stanca, 2009) document chains of unkindness. As the only seemingly successful remedy, letter writing has been proposed by Strang et al. (2016). Here, we revisit this successful intervention, which so far has been attributed to emotional regulation. We find that it is not driven by emotional regulation and not specific to unkindly treated subjects; kindly treated subjects also act more pro-socially after writing letters.

Letter writing and pro-social behavior have been linked before. Xiao and Houser (2005) find that letting subjects *B* write a letter to *A* reduces their willingness to punish *A*. Xiao and Houser (2009) re-investigate the data from their 2005 study and find that dictators who anticipate receiving a letter are less likely to share unfairly in a dictator game. They conclude, that letter writing works as an efficient and cheaper form of sanctioning. Grosskopf and López-Vargas (2014) find that subject *B* is willing to pay for this opportunity to ‘voice’ its anger. Closer to Strang et al. (2016), Koukoumelis and Levati (2019) show that letter writing has a positive effect on contributions in a later public good game for subjects who have been treated unfairly in a dictator game. We add to this by showing that the positive effect also occurs for kindly treated subjects, which suggests that ‘voicing anger’ is not the only reason why letters lead to more cooperative behavior.

2 Experimental Design

Assignment to roles and tasks

One participant in each session is randomly selected to be the boss (*A*). When explaining the role of *A* in the instructions, we also use the term ‘boss’. This wording highlights the (dictatorial) power of the respective subject and thus her responsibility for the job assignment. All other subjects are paired. One person in each pair is randomly assigned the label ‘player *X*’ and the other ‘player *Y*’. By default, both *X* and *Y* are given a pleasant task; they are supposed to watch and rate funny video clips. The boss then decides whether to gain 10 € and re-assign one subject in each pair to an unpleasant encryption task, which comes with automatic reminders to hurry up. The decision has to be made by the boss for each pair. In case that the boss wants to impose the unpleasant task, she has to anonymously, i.e., without knowing the real people who are affected by her choice, select who in each pair should do this task, *X* or *Y*. While all participants learn that the boss receives money for assigning the unkind task, the level of the premium is only revealed to the boss during the experiment. In a pre-test, subjects were very concerned with their income relative to that of the boss. By not telling them this amount, we are hoping to shift the focus away from money to the unequal job assignments. With the loaded nature of the term ‘boss’ and the ensuing emphasis on the power relationship, we also hope to decrease happiness for those being given the unpleasant task and increase it for those being given the pleasant task.³

altruism (e.g. Bardsley, 2008; List, 2007), giving is clearly kind to the receiver. This justifies its use in the literature on chains of unkindness (e.g. Ben-Ner et al., 2004; Diekmann, 2004; Bahr and Requate, 2014). Here, we use it so that our results become directly comparable to these contributions.

³As pointed out by a referee, subjects may react more ambiguous to loaded than neutral terms. Using such terms thus risks introducing measurement error. Fortunately for us, subjects’ emotional response to the assignment turns out to be significant—see later.

Pleasant versus unpleasant tasks

The rating task is designed to give subjects a pleasant experience. They watch short, funny videos. The actual work, the rating of the video, is very simple. They are asked to check a box if they enjoyed watching the video. We supply a sufficient amount of videos to keep the raters entertained throughout the duration of this part.

This pleasant task contrasts with the following task that is purposefully constructed to be annoying. Subjects have to convert sequences of numbers into meaningless combinations of letters using an encryption key that changes after each sequence. According to Benndorf et al. (2018), encryption tasks promise minimal learning, which reduces the chance for participants to enjoy getting better at it. The changing key makes learning even more difficult. Moreover, encrypters sit in the same room as raters. They can hear raters laugh at the funny videos which makes them aware of the pleasant alternative experience that they miss due to the boss's decision. In addition, automatic reminders to hurry up appear at a time where encrypters are likely to have almost finished. These reminders have to be clicked away by encrypters in order to return to their work. After each reminder, all letters that have already been encrypted are lost and have to be re-entered.

In order to render the experience even less pleasant, encrypters are put under time pressure. The experiment only continues until (i) every encrypter has solved an unknown minimum number of encryptions between three and ten or (ii) most participants have achieved their minimum and are waiting for others to finish their task. Since the minimum number is unknown to subjects, they have an incentive to continue after three encryptions. We set the minimum to three for all but one subject. For this subject, the minimum number was set to 10. As a result, we are very likely to end up in case (ii), where most subjects wait for a few other subjects to finish their task.

The relatively high effort of encrypters is ridiculed by a rather symbolic payment of 0.10 €. The unjust treatment is highlighted by showing encrypters a table in which their situation is compared with that of the rater. The table shows that both receive the same amount of money—despite the obvious differences in effort. It also reminds encrypters on how often they have been interrupted, which is contrasted with how often the rater with whom they are partnered liked the video.

Treatments L and W

In the L treatment, participants are given three minutes to write a letter to the boss. In order to underline the real consequences of their decision in this stage, letters are printed and handed out to the boss at the end of the experiment and subjects are informed about this. Subjects need to type at least one character into the text box before being able to send the letter. Otherwise, they can freely use the letter writing option.

In the W treatments, participants cannot write a letter. Instead, they wait for three minutes. This ensures that the same time passes in all treatments between the end of the pleasant or unpleasant task and the donation decision in the dictator game. There are two variants of this treatment. First, participants just wait (W_0). Second, they are asked to report how much money they need to be offered to engage in the unpleasant rather than the unpleasant task (W_M).

Written instructions for all treatments were the same—see online appendix, which contains the German original as well as an English translation. The treatment was

‘administered’ through the screens: subjects in the L treatment see an invitation to write a letter to the boss on their screen; subjects in the W_M treatment are asked about their willingness to pay after which they see the standard waiting screen and those in the W_0 treatment only see the waiting screen.

Dictator game

In the last part of the experiment, we measure how participants behave toward a neutral third party in a dictator game. Using the dictator game is customary in the literature of chain of unkindness and allows us to compare our results to those of previous studies (Strang et al., 2016; Gray et al., 2014; Ben-Ner et al., 2004). Each dictator receives 10 € and is asked to leave between 0 € and 10 € in increments of 1 € to a person C .

We do not announce the size of the pie in the instructions so that subjects cannot decide how many euros to pass on before having been exposed to the treatments. As a result, emotions are more likely to affect behavior. In addition, not mentioning the size helps with our goal to shift the focus away from comparing one’s income with the boss to comparing task assignments.

To avoid image concerns, we make sure that subject C will never be in direct contact with the dictators. All participants are informed that person C is neither present in the laboratory nor participating in another session of the same experiment. Instead, person C is a subject in a future, unrelated experiment that takes place later in the same academic term (in September 2017).⁴

2.1 Elicitation of Happiness

Since the emotional state is key for the emotional regulation explanation, we assess emotions during the experiment using exactly the same method that was employed by Strang et al. (2016). In between the different stages of the experiments, all participants are asked to describe their current mood on a nine-point Likert scale by clicking on a respective symbol (*Self-Assessment Manikin*). Each of the extremes of the scale is associated with words that subjects memorize before the experiment. The left-hand side is described as unsatisfied, unhappy, annoyed, desperate, melancholic, the right-hand side as satisfied, happy, pleased, hopeful, balanced. Bradley and Lang (1994) designed this form of elicitation to be understood intuitively, making the elicitation as quick as possible and keeping the interruption minimal. Answers by subjects are based on their very individual and subjective reading and weighing of these terms.⁵ For simplicity, we refer to this mixture in the following as ‘happiness’ and ask the reader to keep in mind that it may span a whole range of emotions including annoyance.

The elicitation is done four times: (i) before participants learn which task they are assigned to, (ii) immediately after raters and encrypters have finished their tasks, (iii) three minutes later (after waiting or writing letters) and (iv) after the dictator game.

⁴The experiment was, of course, also run and the money left by each encrypter and rater was handed to a subject in this experiment.

⁵The subjective mixture of several categories has the disadvantage that it renders this concept rather fuzzy and introduces noise.

	boss	encrypter	rater	total
W_0	3	40	40	83
W_M	4	59	59	122
L	4	55	55	114
total	11	154	154	319

Table 1: *Numbers of participants by groups.*

3 Implementation and Descriptive Statistics

The experiment was conducted in May and June 2017 at the Business and Economics Research Laboratory (BaER-Lab) at Paderborn University in Germany. The experiment was programmed using the software z-Tree (Fischbacher, 2007) and participants were recruited with the help of ORSEE (Greiner, 2015). In total, eleven sessions were run, with each session lasting around 45 minutes and earnings averaged at 10.60 €.

As shown in Table 1, there were 319 participants in total, 114 in the letter writing treatment L and 205 in the waiting time treatments. In each session, the boss chose to assign half of the participants to the encryption task, resulting in 154 observations from encrypters and 154 from raters.

Table 2 shows that randomization seems to have worked considerably well. In particular, the happiness elicited at the very beginning of the experiment is very similar in the letter writing and W_0 treatment (p-value for Mann-Whitney U test is 0.51) as well as W_M treatment (p-value for Mann-Whitney U test is 0.31), neither does it differ between encrypters and raters (p-value for Mann-Whitney U test is 0.94). Examining more closely what seem large differences, men are not more likely to become raters (p-value for Fisher’s exact test is 0.64), economics students are not more likely to be assigned to the L rather than the W_0 treatment (p-value for Fisher’s exact test is 0.20) and those who want to become teachers not less likely to appear in the L than the W_M treatment (p-value for Fisher’s exact test is 0.36).

	W_0	W_M	L	encrypters	raters
age	23.55	23.09	22.33	22.74	23.10
male	34%	33%	36%	32%	36%
economics major	38%	41%	45%	42%	40%
engineering	8%	7%	7%	7%	8%
cultural science	9%	8%	7%	7%	9%
teaching	40%	41%	36%	40%	39%
initial happiness	5.91	5.58	5.83	5.76	5.75

Table 2: *Demographic statistics by groups.*

Before actually analyzing the data, we run several checks to find out whether our design worked. First, we want to see whether encrypters were actually engaging in their task. Figure 1 shows that while most encrypters solved at least three tasks, hardly any of them

succeeded in finishing seven or more tasks before the end of the stage.⁶ All participants did thus eventually end up waiting for the person with a minimum of 10 encryptions (and a few other, exceptionally slow participants) and we were ending their waiting time by terminated the task exactly after three minutes in all sessions. Waiting and writing treatments were thus all of the same length.

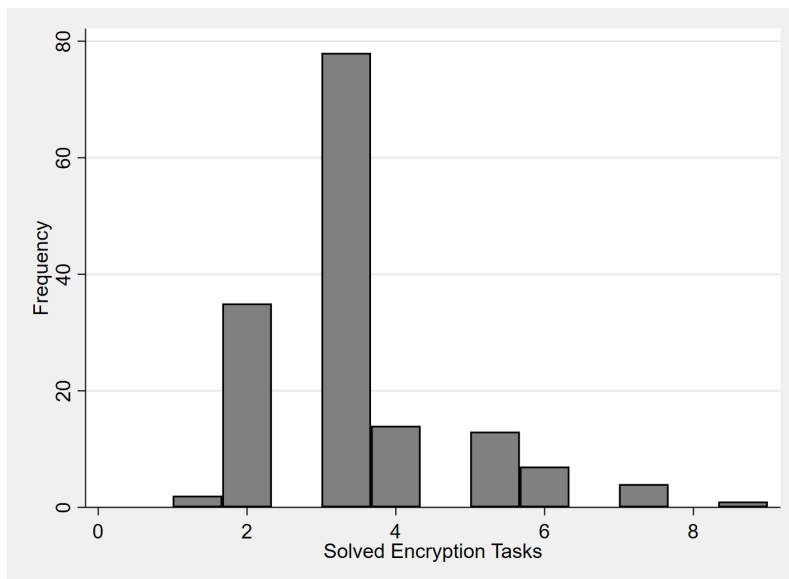


Figure 1: *Almost all encryptions solve at least three tasks correctly.*

Second, we want to check whether the manipulation was successful in the sense that rating was a more pleasant experience than encoding. Apart from four out of the 122 subjects in the W_M treatment, all express a willingness to pay for being a rater rather than an encrypter. The median willingness is 7 € and the average 6.8 €. The stated amounts have to be taken with a grain of salt because (i) they were not elicited in an incentivized manner and (ii) they differ among subjects—see Figure 6 in the appendix. Still, they indicate that subjects overwhelmingly preferred rating to encrypting. This is supported by comments from the post-experimental questionnaire suggesting that encrypting was perceived as difficult and annoying and that over time encrypters became increasingly frustrated.

For more systematic evidence on the effects of job assignment, let us turn to the self-reported ‘happiness’. Initial median ‘happiness’ was rated 6 out of 10. After the treatment, the median of self-reported ‘happiness’ is 7 among raters and 4 among encrypters. Strang et al. (2016) report a drop in the median around two points for subjects receiving an unfairly low share of money. The size of the effect of the job assignment is thus similar to theirs. The median difference of 3 points between raters and encrypter on the 9-point scale is not only economically significant it is also more likely that the ‘happiness’ of a randomly drawn rater is larger than that of an encrypter (p-value of Mann-Whitney-U test below 0.0001).

⁶The raters did also actively engage in their task. Raters watched between two and eight videos and reported at least one and at most eight likes.

Figure 2 illustrates that not just the median but the whole distribution of answers shifts to the right and this shift is highly significant, too.⁷ Raters report larger values than encrypters in the sense that the distribution of their answers first-order stochastically dominates that of encrypters.

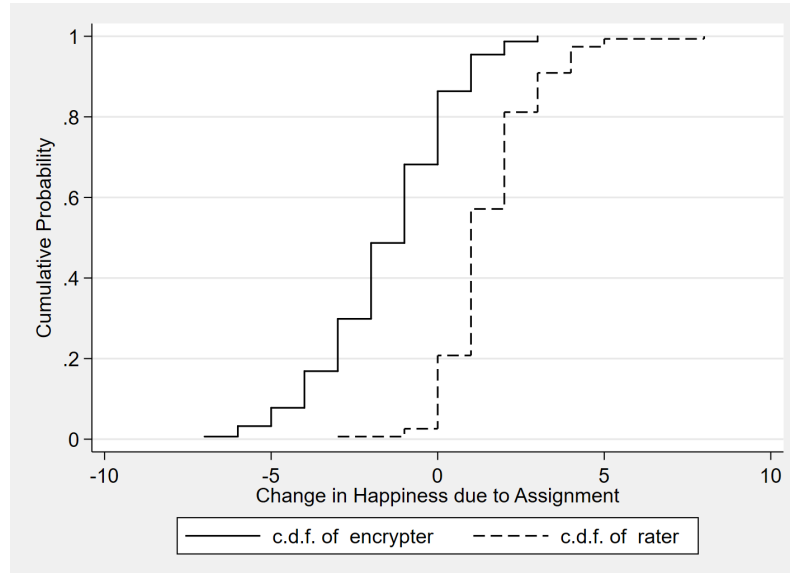


Figure 2: *Being assigned the encryption rather than the rating task results in smaller reported happiness.*

Third, we want to know whether encrypters perceived the task assignment as unkind. Evidence comes from the content of the letters written by encrypters and raters. While participants were free to design the letter as they wanted, most did choose a typical letter format, starting by addressing the recipient, in this case the boss, in formal language as if writing a letter to a stranger. Even informal letters were written as comments or messages directed to the boss. Hence, the overwhelming majority of participants took the letter writing serious.⁸ A typical letter written by an encrypter and a rater is displayed in the appendix. In the letter, encrypters normally complain about one or several issues they had while carrying out his task, such as time pressure, or being interrupted by the boss' message to hurry up. Some explicitly blame the boss for his decision and state that they would rather have done the other task. Raters tend to thank their boss, either for having been assigned to the more pleasant task, or for making the task so enjoyable. Overall, the content of the letters supports the assumption that the boss was actually held responsible for the kind or unkind task assignment.

Finally, the elicitation of monetary values of the two tasks in the W_M treatment may affect subjects differently than simply waiting W_0 . For example, attaching an amount to the task may help to 'let go'. On the other hand, mentally recalling the task may keep the memory more vivid and render it more difficult to 'close the case'. We check whether the elicitation affects our key behavioral variable, the sharing with a third party. The payment does not significantly differ across treatments, neither for raters (p-value

⁷Somer's D is 0.8 with a 95%-confidence interval of [.74,88] and thus far away from zero.

⁸Only 3 encrypters and 10 raters sent nonsensical letters containing no actual words.

for Mann-Whitney U test is .39, Somer’s D is .099 with a 95% confidence interval of [-.123,.320]) nor for encrypters (p-value for Mann-Whitney U test is .86, Somer’s D is .020 with a 95% confidence interval of [-.211,.250]). In order to increase observation numbers in the following analysis, we pool the two waiting treatments within their respective groups.

4 Results

We first examine whether letter writing still has an effect in our design, where —unlike in Strang et al. (2016)— the domain in which subjects are treated kindly or unkindly differs from that of their own decision. Emotional regulation should work regardless of the domain of the decision.

Indeed, encrypters pass on stochastically larger amounts to the third party when having had the opportunity to write a letter in the sense of first-order stochastic dominance; the cumulative distribution function shifts to larger values—see Figure 3. This shift is significant (Somer’s D is 0.18 and the respective 95% confidence interval is [.0019,.3680]). The median amount passed on in treatment *L* is 3 € and thus one euro higher than in the *W* treatments. The raw average effect amounts to 0.64 €; an arbitrary drawn subject is significantly more likely to give more in the *L* than in the *W* treatment at the 10% level (p-value of Mann-Whitney-U test 0.0525). This effect is comparable in size to that found by Strang et al. (2016). Controlling for faculty, session, gender, age and level of studies with a Tobit regression,⁹ the average effect is 0.91 € and significant at the 5% level —see result (1) in Table 3.

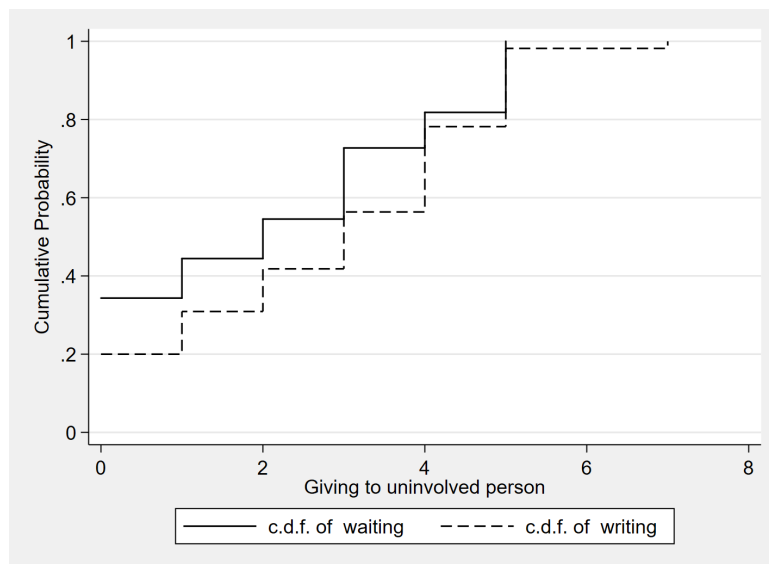


Figure 3: *The willingness to give differs between treatments. Higher amounts are more frequent when writing (L) rather than waiting (W).*

Result 1. *Unkindly treated subjects (encrypters) give more to an uninvolved third subject if they had the opportunity to first write a letter instead of waiting.*

⁹We run a Tobit regression (Tobin, 1958) with truncation from the bottom and top because ratings are between 1 and 9—see Schnedler (2005) for consistency of the respective estimator.

Letter writing thus works even if the unkind treatment is not monetary. This suggests that it helps to regulate emotion. We further inquire into this by examining how letter writing affects reported subjective happiness. Like Strang et al. (2016), we find that expressing emotions in the letter is significantly correlated with the change in happiness due to the letter ($t=2.32$, $p=0.024$) and even more so when controlling for exogenous variation like age, gender or faculty ($t=2.47$, $p=0.017$). When including indicators for other content elements (showing understanding, criticizing unfairness, questioning of motive or suggestion of usage), the indicator for expression of emotions becomes insignificant ($t=1.45$, $p=0.155$), while the expression of unfairness becomes weakly significant ($t=1.87$, $p=0.069$)—see Table 4 in the appendix. All content elements (including expression of emotions) are likely to depend on unobserved characteristics of subjects that also affect what they write. For example, subjects with a disposition for choleric behavior might have trouble to calm down and use letters to rant. This would, for example, downwardly bias the effect of expression of emotions, which is why such correlations can only be a very rough indication.

Returning to effects that we know to be causal because they have been manipulated in the experiment, we find that around 40% of the subjects do not change their self-reported happiness—irrespective of whether they wait or write letters. The cumulative distribution of answers looks very similar for subjects who wait and write—see Figure 4. Letter writing does not appear to lead to stochastically larger increases in happiness (Somers’s D is 0.0064 with a 95%-confidence-interval that centers around zero [-.1783,.1912]). The median reported happiness is identical for writers and waiters and we detect no statistically significant difference in terms of waiters or writers having larger values (p -value for Mann-Whitney U test is 0.95). Although we measure ‘happiness’ exactly at the same points of the experiment in exactly the same way as Strang et al. (2016), they find a highly significant effect—albeit using asymptotic normality assumptions and a t -test ($t = 4.95$, $p = 0.01$, 95%-confidence interval [1.21,8.68]). The choice of test, however, is not at the root of the problem. A t -test would not be significant with our data, either ($t=.20$, $p=.41$, [-.391,.484], power=73%). Neither is the effect due to our pooling of the W_0 with the W_M treatment.¹⁰ Notice that the problem is also unlikely to be caused by a lack of power. Even if our effect had been statistically significant, it would still be economically negligible given that the average difference amounts to .0465 points on the 9-point Likert-scale.

Result 2. *The opportunity to write letters does not affect self-reported ‘happiness’ of unkindly treated subjects (encrypters).*

We used the measure suggested in the psychology literature to capture emotional regulation (Bradley and Lang, 1994; Strang et al., 2016). Still, there are two caveats. It might not elicit the emotional state properly and elicitation is ‘soft’ in the sense that it does not have real consequences. Our final step deals once more with a decision that has such consequences.

If emotional regulation is the reason for higher giving in the L treatment, this effect should be limited to encrypters. Raters with their experience of kindness have no need to regulate and should thus not give more when being given the opportunity to write a letter. Similarly, if being given the opportunity to write a letter to A is a signal indicating that A behaved inappropriately by focusing too much or too little on others, raters get the signal

¹⁰All our test results remain robust when focusing only on treatments L and W_0 .

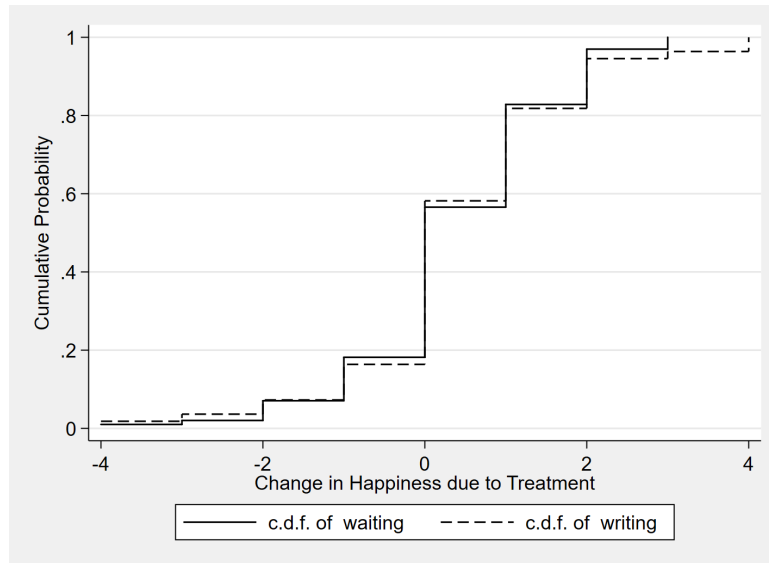


Figure 4: *Self-reported happiness is affected similarly by writing (L) and waiting (W).*

that more selfishness is not problematic and have no reason to share more. More generally, if letter writing specifically affects unkindly treated subjects, kindly treated subjects, i.e., raters, should not be affected.

Interestingly, letter writing also leads to stochastically more sharing among raters in the sense of first-order stochastic dominance; the size of the shift is very similar to that of encrypters; Somer's D is 0.177 with a 95%-confidence interval of $[-.0090, .3646]$. Raters' median amount shared is by one Euro larger for subjects who write (2 €) rather than wait (1 €) and the raw average amount is 0.69 € larger. The effect is statistically significant at the 10%-level (p-value for Mann-Whitney U test is 0.059). When using a Tobit regression to control for faculty, session, gender, age and level of studies it amounts to .95 € and is significant at the 10% level. In terms of size, we find that it is even slightly larger than the effect found for encrypters. A lack of power may thus be the reason for the weak significance.

Result 3. *Kindly treated subjects (raters) also give more to an uninvolved third person if they had the opportunity to first write a letter instead of waiting.*

The effect of letter writing on subjects who rate and encrypt is astonishingly similar. If we pool the data of both groups, we find that average difference of .69 € and a shift in median by 2 €; the likelihood for a randomly drawn subject to give more when writing a letter rather than wait is highly statistically significant (p-value for Mann-Whitney U test is below 0.01). This suggests that the weak significance in each of the groups was due to a lack of power. In addition, the effect is also economically significant because it amounts to 20% of the overall earnings.

5 Discussion & Conclusion

We took the only known successful remedy against chains of unkindness, letter writing, as a starting point. From the literature, one might conclude that this intervention works

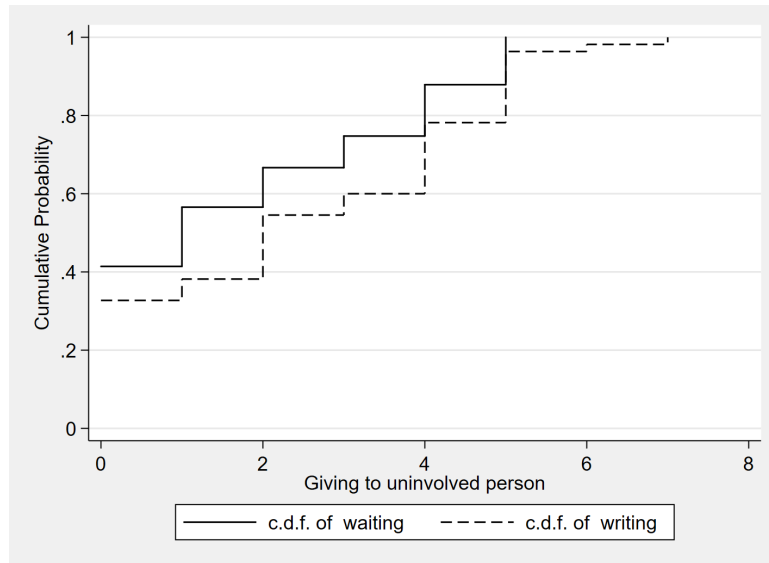


Figure 5: *Raters also give larger amounts when writing (L) rather than waiting (W).*

because of emotional regulation and that another emotional regulation technique may be a good tool to counter chains of unkindness in practice. Here, we have critically examined that letter writing works *because it regulates emotions*.

We have found that letter writing is versatile in the sense that it also increases giving toward a third party if the unkind treatment was non-monetary but the assignment of a frustrating job (Result 1). While this is in line with emotional regulation being at work, our other findings indicate the opposite. First, reported happiness is not affected differently for people who write rather than wait (Result 2). And second and more importantly, kindly treated subjects also give more when they write rather than wait (Result 3).

For the moment, we thus have no experimental evidence supporting that emotional regulation techniques offer a remedy against chains of unkindness. (Of course, it would be wrong to conclude from this that emotional regulation techniques cannot work.) One possible reason why letter writing did not unfold its effect through emotional regulation might be that the emotions created in the laboratory are simply too weak to require such regulation. Generating stronger emotions, however, may be unethical and hence not feasible in the lab. Future research exploring this might thus turn to field data.

Even though letter writing does not specifically affect unkindly treated people, our findings show that it seems a versatile and general tool to increase pro-social behavior. The exact reason why letter writing triggers pro-social behavior is beyond this paper. We can only speculate about the reasons. Perhaps effectively getting across a message to someone requires taking this person's perspective and thus activates more other-regarding modes of thinking. This would explain why the effect in Strang et al. (2016) for letters that are not sent is weaker: there is less need to take on a different perspective because there is no actual reader. If letter writing is really about perspective taking, other interventions, e.g., writing to someone else or preparing a speech, should also work to increase pro-social behavior.

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A Original letter from an encrypter (translated from German)

Dear Mr/Mrs

herewith, I would like to ask you to please give the encrypters more time to do their work, as under the current circumstances work is not efficient. Moreover, the other workers with their video-task are quite noisy, which makes the encrypting even more difficult.

With kind regards

B Original letter from a rater (translated from German)

Dear Mr. Boss,

I would like to use this opportunity to thank you in person for this pleasant work atmosphere. The videos are mostly funny.

Your satisfied employee

C Further results

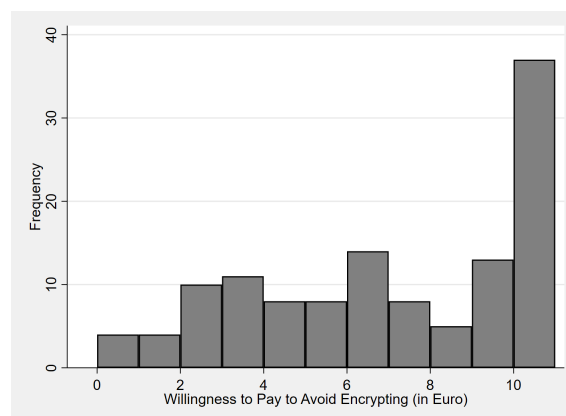


Figure 6: *Subjects overwhelmingly prefer the rating task.*

Table 3: Dependent Variable: Giving to Uninvolved Person (in Euro)

Tobit model	(1) encrypters only	(2) raters only	(3) all
L treatment	.9084** (.4488)	.9480* (.5246)	1.034** (.4850)
Encryptor			.5790 (.40180)
L treatment x Encryptor			-.1273 (.6760)
male	-.8301* (.4889)	-.5308 (.5389)	-.6585* (.3655)
master	.5736 (.5315)	.0131 (.5768)	.2869 (.3901)
age	-.0869 (.0811)	-.1428 (.1021)	-.1070* (.0636)
constant	4.6387** (1.9836)	4.04917 (2.6265)	3.8226** (1.6071)
N. obs.	154	154	308
Pseudo-R ²	.0241	.0274	.0205

* significant at p<0.10; ** significant at p<0.05; *** significant at p<0.01, controls for faculty and session were used

Table 4: Correlation Between Letter Content and Change in ‘Happiness’

OLS regression (encrypters only)	(1)	(2)	(3)
Expression of emotions	1.384** 0.597	1.527** 0.619	1.024 0.707
Expression of Understanding			-0.209 0.402
Criticism of Unfairness			1.189* 0.636
Suggestion for Usage			-0.147 0.480
Age		-0.084 0.085	-0.113 0.089
Male		0.037 0.442	-0.020 0.449
Master		-0.514 0.501	-0.660 0.518
	0.563	2.222	2.274
N. obs.	55	55	55
R ²	0.092	0.306	0.362

* significant at p<0.10; ** significant at p<0.05; *** significant at p<0.01, controls for faculty and session were used