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Does happiness spread within organizations? Evidence from a field experiment in Norway

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Abstract

We report the results from a large field experiment that tests if happiness spreads within an organizational setting, as predicted by the emotional contagion hypothesis. Although some studies have supported this hypothesis, no well-powered randomized controlled trial has ever tested it in the field. In collaboration with the Norwegian Armed Forces, we randomly assign over 1,500 recruits to rooms during eight-week boot camps. Some recruits live with relatively happier peers while others live with relatively unhappier ones. We find no evidence of happiness convergence at the room level and reject even small contagion effects. We show that this result is not because happiness is overly stable and that peer effects do emerge for some attitudes in non-emotional domains. These results call for a reconsideration of the presumed ubiquity of happiness spillovers, with subsequent implications for well-being models and policies.

Keywords: happiness, subjective well-being, emotional contagion, field experiment, RCT

JEL codes: R3, N9

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1 Main

As organizations have increasingly recognized the importance of emotional states, including happiness (Barsade & Gibson, 2007; Fleming, 2024), promoting workers' well-being has become a standard practice in many workplaces (Ashkanasy & Dorris, 2017). Research indicates that happy workers are more productive (Bellet, De Neve, & Ward, 2024; Oswald, Proto, & Sgroi, 2015) and less likely to leave their jobs (Clark, 2001; Freeman, 1978; Kaiser & Oswald, 2022). High workplace well-being is positively associated with stock market performance (De Neve, Kaats, & Ward, 2023), and job seekers tend to prefer workplaces perceived as happier (Ward, 2022).

Because workplaces function as tightly knit social clusters, understanding how happiness spreads through social interactions is essential for designing effective well-being interventions. This study tests the hypothesis that, within an organization, happiness states can be transmitted through repeated social interactions with peers.

According to the emotional contagion hypothesis (Hatfield, Cacioppo, & Rapson, 1993, 1994), humans tend to align with the emotional states they perceive during interactions. Research has shown that this form of behavioral synchrony can occur through facial expressions (Barger & Grandey, 2006; Ekman, Levenson, & Friesen, 1983; Hennig-Thurau, Groth, Paul, & Gremler, 2006; Surakka & Hietanen, 1998; Zajonc, 1985), other non-verbal behaviors (Kret, 2015), tone of voice (Neumann & Strack, 2000), physiological or neurological synchrony (Decety & Lamm, 2006; Prochazkova & Kret, 2017; Singer & Lamm, 2009), social evaluation (Parkinson & Simons, 2009), and even indirect interactions on social media (Ferrara & Yang, 2015; Kramer, Guillory, & Hancock, 2014) or through digital messages (Smith & Rose, 2020).

These findings have implications beyond the workplace. A growing number of governments — two-thirds of OECD countries — now use well-being frameworks that include self-reported happiness metrics (Mahoney, 2023). Some researchers advocate evaluating public policies based on their costs and benefits in terms of self-reported happiness metrics (Frijters, Clark, Krekel, & Layard, 2020; Frijters, Krekel, Sanchis, & Santini, 2024; Layard & Oparina, 2021). This type of cost-benefit analysis often hinges on assumptions about happiness spillovers, i.e., the extent to which psychological benefits spread from one individual to another (cf. HM Treasury, 2021). While some studies document emotional contagion on university campuses (Anderson, Keltner, & John, 2003; Courtney et al., 2024; Howes, Hokanson, & Loewenstein, 1985) and Mervin and

Frijters (2014) provides estimates of mental health spillover within a family, there is virtually no causal evidence about happiness spillovers in an organizational setting.

An influential series of studies by Fowler, Christakis and colleagues (Cacioppo, Fowler, & Christakis, 2009; Fowler & Christakis, 2008; Rosenquist, Fowler, & Christakis, 2011) documented that happiness levels tend to correlate within social networks, as well as loneliness and depression do. However, this correlation is confounded by factors such as homophily—the tendency of individuals to associate with those who share similar characteristics—raising the question of whether prevalent moods in networks result from social transmission or self-selection (Aral, Muchnik, & Sundararajan, 2009; Shalizi & Thomas, 2011). In an ideal experimental setting, a large number of individuals would be randomly assigned to social networks where they engage in frequent interactions, allowing researchers to isolate the pure effect of emotional transmission. This is precisely what we did, thanks to a collaboration with the Norwegian Armed Forces.

We conducted a field experiment to examine how peer influence affects self-reported happiness over an eight-week period. Upon arrival at a military camp, 1,508 newly enlisted recruits completed an initial survey and were then randomly assigned to dorm rooms for the duration of the boot camp. This random assignment ensured that recruits were exposed to varying levels of peer happiness over eight weeks, and allowed us to draw causal inferences about the impact of peer influence. Follow-up surveys measured self-reported happiness on a scale from 0 to 10. Our specific setting shares important characteristics with typical workplaces, such as structured routines, hierarchical roles, and repeated team interactions. Moreover, it can be particularly conducive to peer effects, as demonstrated by previous studies that documented strong peer effects on various attitudes in the boot camp of the Norwegian Armed Forces (Dahl, Kotsadam, & Rooth, 2021; Dahlum, Hanson, Johnsen, Kotsadam, & Wuttke, 2024; Finseraas, Hanson, Johnsen, Kotsadam, & Torsvik, 2019; Finseraas, Johnsen, Kotsadam, & Torsvik, 2016; Finseraas & Kotsadam, 2017).

We are not the first to test the emotional contagion hypothesis using random assignment to groups. But critical gaps remain, and we still lack a clear, generalisable understanding of the phenomenon.

Most laboratory studies capture the short-term effect of single interactions in a noiseless setting (Herrando & Constantinides, 2021; Sy, Côté, & Saavedra, 2005; Tee, Ashkanasy, & Paulsen, 2013; Van Kleef & Côté, 2022). Instead, in many real-life settings - such as a workplace,

a neighborhood or a classroom - social interactions are often repeated at different points in time and emotions are expressed in a sea of other communication elements. It thus remains unclear whether longer interactions (spanning weeks rather than minutes) can influence more enduring moods, defined as the set-point emotional state to which individuals tend to return after daily fluctuations. Beyond the horizon of a few minutes, is happiness contagious?

While lab studies shed light on momentary contagion, evidence from real-world, long-term settings remains sparse. Field experiments with random assignment outside the lab are rare, and typically limited to American college dormitories. Some classic small-scale studies have documented that college roommates are more similar to each other when it comes to depression scores (Howes et al., 1985, N=88) or emotional reactions to a clip (Anderson et al., 2003, N=70) than they were to another randomly chosen participant in the sample. Courtney et al. (2024) study Stanford students (N = 798) who were non-randomly assigned to dorms but quasi-randomly assigned to dormitory halls. They do not estimate causal spread of individual well-being, but rather construct measures of peer emotional stability based on hallmates who are *not* direct social ties. They find that hall network density and peer emotional stability are associated with higher well-being. In all, the evidence for lasting happiness contagion remains scant.

“Happiness” is a multifaceted and potentially ambiguous concept. This study focuses on happiness intended as a set-point emotional state, that is, a relatively lingering feeling about one’s own situation. This concept is akin to *life evaluations* – like general life satisfaction or Cantril ladder life evaluations, which are the focus of the World Happiness Report – but differs in that respondents tend to place greater weight on their experiences in the very recent past (e.g., the last week) when answering the happiness question compared to a standard life evaluation (Benjamin, Debnam Guzman, Fleurbaey, Heffetz, & Kimball, 2023). It should also be distinguished from happiness intended as an *emotion* - which is a more ephemeral feeling (in the order of minutes), and about something specific (Kaplan, Levine, Lench, & Safer, 2016). Our self-reported measure of happiness uses a standard wording and scale (cf. OECD 2013, Appendix B, and European Quality of Life Surveys, Q5/Q41).

Our study has some limitations that we ought to acknowledge from the onset. First and foremost, it focuses on a specific setting - a military boot camp. While military boot camps share important characteristics with typical workplaces, such as structured routines, hierarchical

roles, and repeated team interactions, they also differ significantly due to mandatory participation, limited autonomy, and strict discipline. Future research should further explore how these distinctive features affect the potential for emotional contagion in organizational contexts. Second, it involves a specific population (young Norwegian adults), and any extrapolation to other populations should be treated with caution. That said, we note that the baseline level of happiness for the recruits of 7.82 is very similar to that of the general Norwegian population (8.05) and Norwegians below age 22 (7.92) in the European Social Survey. Third, we used a common single-item self-reported measure to assess the happiness level. This approach is standard in the subjective wellbeing literature, but recent evidence suggests that this type of measure is not very sensitive to changes, and alternative measures might be used instead (Kaiser & Prati, 2025). Finally, we must underline the important distinction between the *experiential* aspect of happiness (i.e., feeling happy) and its *expressive* aspect (i.e., manifesting happiness, typically via Duchenne smiles). Our study focuses solely on the former and it therefore does not represent a rebuttal of the evidence that smiles are contagious (Barger & Grandey, 2006; Ekman et al., 1983; Hennig-Thurau et al., 2006; Surakka & Hietanen, 1998; Zajonc, 1985).

The rest of the paper is organised as follows. Section 2 gives an overview of military conscription in Norway and describes our RCT protocol conducted in the boot camp. Section 3 presents the empirical strategy and section 4 the results. Section 5 concludes.

2 Context, data and RCT protocol

Norway enforces mandatory military conscription for both men and women. The selection process consists of two stages, typically beginning in the final year of upper secondary school. Initially, all individuals in a given cohort complete a self-assessment form issued by the Armed Forces. Based on their responses, approximately 17,500 candidates proceed to the next stage, where they undergo physical, medical, and cognitive evaluations at various locations nationwide. During the final phase, recruits participate in interviews discussing their motivation and preferred areas of service. Ultimately, around half of those assessed are chosen for service based on their capabilities and motivation. A majority of the conscripts report that they serve voluntarily (Køber, 2020).

Military service generally lasts one year, beginning with an eight-week basic training period,

commonly referred to as boot camp. During this time, recruits are assigned to a specific troop and further divided into smaller groups sharing a room within the barracks. While most training activities occur at the troop level, recruits also collaborate on tasks within their rooms, such as maintaining cleanliness. Our data indicates that individuals tend to socialize more frequently with their roommates than with others in the camp (70 percent report that they "often" spend their leisure time with roommates and 27 percent report "sometimes"). Following boot camp, recruits apply for specialized roles and are subsequently stationed at different military bases across Norway.

We collected baseline data on the first day of military service, in September 2022 and January 2023. When recruits arrived at the camp, they went through various posts, such as drug checks by the military police, dental control, and a post where we administered a baseline survey in a designated room. Groups of around 45 recruits entered the room at the time. The survey is voluntary but everyone was asked to sit in the room in silence until everyone (who wants to) had completed the survey, which took between 15-25 minutes. Over 99 percent of the recruits consented and filled out the survey at baseline. The survey questions are broad and relate to attitudes and behaviors on a range of issues.

We measured baseline values of happiness with the following survey question: "On a scale from 1 to 10, where 1 is very unhappy and 10 is very happy, where would you place yourself? [1 - very unhappy; 10 - very happy]". The distribution of the variable at baseline is shown in Appendix Figure [A.1](#). We describe the coding of the other variables in Section [A.1.1](#). We collected follow-up data at the end of the recruit training periods (8 weeks after baseline). The follow-up survey included many of the same measures as the baseline survey. Most importantly, at this point we ask about happiness again.

In our field experiment, individuals are randomly assigned to rooms for the entirety of the recruit period, replacing the usual practice in which room assignments are made ad hoc by officers, without a formal assignment protocol. This setup provides a valuable opportunity to study peer effects, as recruits are suddenly placed in a social environment that is entirely distinct from their previous experiences. They must interact and cohabit with new peers, adjusting to differences in habits, expectations, and perspectives. Many are likely to adapt in an effort to integrate into their new surroundings, leading to a process of mutual influence and learning. Rooms are relatively small and host, on average, five individuals, which makes it plausible that

interactions among all members of the group occur during the whole eight-week period. Rooms are gender-mixed, but we refrain from conducting gender-based analyses, as these were not pre-registered. Research has previously identified significant peer effects in the Norwegian Armed Forces' boot camp, particularly in shaping attitudes toward gender roles (Dahl et al., 2021; Finseraas et al., 2016) perceptions of immigrants (Finseraas et al., 2019; Finseraas & Kotsadam, 2017), and attitudes toward strong leadership (Dahlum et al., 2024).

Our sample size (1508) was determined by the number of participants in the two data collections that we conducted. Given the empirical moments of happiness at endline (mean = 7.69, SD = 1.36), assuming a type-I error rate of 0.05 and a 0.05 equivalence margin, a two-tailed t-test has a 93% power to detect a minimum effect of 1/5 of a standard deviation (0.27). This represents a substantial improvement in the statistical power of the test compared to existing evidence from smaller samples. For context, we would have achieved only about 20% power with a sample size of 100 and around 56% with a sample size of 500.

3 Empirical strategy

To test for peer effects in happiness, we run the following regression:

$$(1) \text{Happiness}_{i,t1,r} = \beta \text{Happiness in the room}_{i,t0,r-i} + \gamma X_{i,t0} + \delta \text{Troop}_{i,t0,r} + \epsilon_{i,t,r},$$

where i indexes individuals, t indexes time ($t0$ is baseline and $t1$ is endline), and r indexes rooms. The main outcome variable is *Happiness*. *Happiness in the room* is measured as the baseline average happiness of the other individuals in the room, excluding individual i (i.e., $r - i$).

A noteworthy issue is the presence of a mechanical negative correlation between baseline *Happiness in the room* and an individual's own baseline value of happiness. Under complete random assignment (which we document, see section 4), and because individuals cannot be assigned to themselves, those with below-average baseline values will, by construction, be exposed to peers with higher average baseline values, and vice versa (Farrar, Green, Green, Nickerson, & Shewfelt, 2009; Feld & Zölitz, 2017; Guryan, Kroft, & Notowidigdo, 2009). To account for this mechanical relationship, it is therefore necessary to control for the individual's own baseline value. The vector $\mathbf{X}$ includes baseline values of the outcome variables and the other pre-treatment controls reported in Table A.1. Troop fixed effects reflect the randomization strata, as individuals are

randomly assigned within troops. We cluster standard errors at the room level (305 rooms).

For our main specification, we follow [Dahlum et al. \(2024\)](#) and use post-double-selection LASSO ([Belloni, Chernozhukov, & Hansen, 2014](#)) to select controls from the full set of pre-treatment covariates. The LASSO procedure selects variables that are correlated with the treatment and/or highly predictive of the outcome, which can improve precision and mitigate any chance imbalances across groups. We also report results with only the necessary controls, as well as results including the full set of controls. Because the controls in [Dahlum et al. \(2024\)](#)’s pre-registration was tailored to outcomes related to political values, we additionally report specifications that expand the list of controls with variables correlated with baseline happiness (see Section [A.1.2](#), Table [A.9](#) and Table [A.10](#)); the results are very similar to our main findings.

4 Results

Preliminary checks

Sufficient variation at the room level. The main independent variable for measuring peer effects is *Happiness in the room* which is the average score on happiness for all others in the room (excluding the person herself) as measured in the baseline survey. As we see in Figure [1](#), the randomization creates substantial variation in the room level happiness that different recruits will experience. This variation is possible thanks to the relatively small size of the rooms, while with very large groups, randomized assignment would generate mostly similar happiness levels across rooms. In our setting, by chance, some recruits will live with very happy peers while others will have less happy roommates and most will live with peers who exhibit moderate levels of happiness. This random variation in roommates’ happiness allows us to analyze the causal effects of assignment to rooms with higher or lower baseline levels of happiness.

Balance tests. Descriptive statistics for all variables are shown in Table [A.1](#). In the appendix, we report balance tests (see Table [A.7](#)) where we regress the treatment variable on the baseline control variables. Most importantly, the balance tests show that recruits did not cluster in rooms depending on their baseline levels of happiness. Only one of the 17 variables correlates significantly with treatment status at the 10 percent level, and the F-tests, testing whether all variables jointly predict treatment, are not statistically significant. In contrast, the baseline

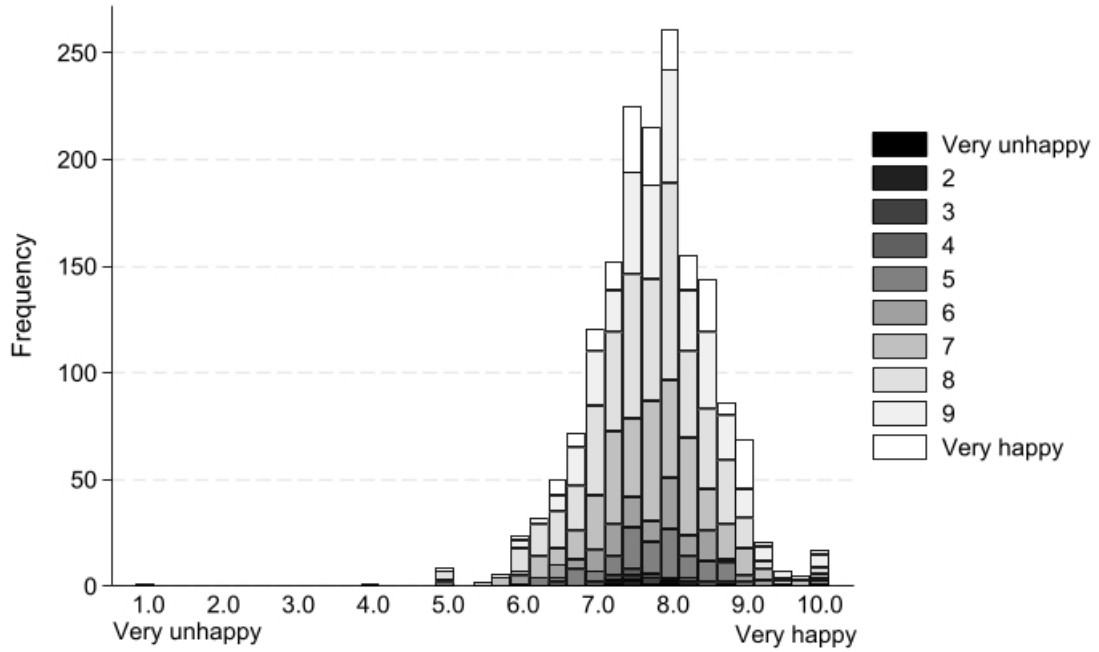


Figure 1: Distribution of the main peer treatment variable

Reading note: The x-axis shows the distribution of roommates' mean baseline happiness (*excluding* ego)—binned in 0.25 steps on the 1–10 scale. The y-axis shows the number of individuals in each bin. Stacked segments within each bar display exposed recruits' (ego) baseline happiness (1–10).

control variables strongly correlate with endline happiness, showing that they are relevant. See Appendix Section A.1.1 for a fuller discussion of the balance tests.

Selective attrition and seasonality. As expected, we did not manage to reach all respondents initially sampled since some people quit during boot camp. We show in Appendix Table A.8, however, that attrition is not correlated with baseline happiness in the room. Since data were collected at two separate moments, in September 2022 and January 2023, we show the descriptive statistics by year in Appendix Table A.2. We note that the samples are similar but the 2023 sample is slightly less happy at baseline.

Peer effects

Identifying causal peer effects presents two key challenges. First, individuals typically select their own social circles, meaning that observed similarities may stem from pre-existing characteristics rather than social influence — that is, there is a self-selection problem. To overcome

this, we randomly assigned recruits to live and work together in shared rooms. Second, when analyzing soldiers' responses at the end of boot camp, it is difficult to determine whether soldier A influenced soldier B or vice versa - commonly known as the reflection problem. To mitigate this issue, we use baseline measurements of soldiers' happiness to define their roommates' happiness levels (i.e., the "characteristics of the peers").

Another, broader challenge in research is the multitude of analytical choices that can influence results, often without being explicitly disclosed. One way to address this issue is through pre-registration, which helps ensure transparency and replicability by tying authors' hands. Although the survey included measures of happiness, analyses of happiness outcomes were not pre-registered because this component had not yet been finalized by the time of data collection. However, to minimize flexibility in our coding and empirical specifications, we adhered to the same procedures as [Dahlum et al. \(2024\)](#), who pre-registered an analysis to test the spread of authoritarian orientation before the 2022 data was collected. We followed their exact same procedure, but replaced the variable for authoritarian orientation with the one for happiness.¹ For our main estimation, we follow [Dahlum et al. \(2024\)](#) and include the optimal controls chosen from the total list of controls using a post-double LASSO selection approach of [Belloni et al. \(2014\)](#). Details of the estimation models are reported in section 3.

Our findings reject the hypothesis that emotional contagion occurs in this context: recruits exposed to relatively happier (or less happy) peers do not exhibit significant changes in their own happiness levels by the end of the camp. In the main specification, we can rule out small effects in either direction. Specifically, in our main specification (Column (1), Table A.3), we see that assignment to peers whose average level of happiness is one scale-point higher (corresponding to 1.37 standard deviations higher levels) are 0.047 less happy at endline on a scale from 1 to 10 (0.03 standard deviations). The estimate is both small in magnitude and precisely estimated. Conducting an equivalence test with two one-sided t-tests (TOST), we can reject effects more negative than -0.11 and more positive than 0.02.

Figure 2 summarizes results from eight different specifications, with the corresponding regression output reported in Appendix Table A.10. Results are essentially unchanged across specifications that differ in control variables, estimation approach, and sample composition. All outcome variables and the peer-happiness treatment variable are standardized, so coefficients

¹An anonymous version of the pre-analysis plan is found here: https://osf.io/qv3hb?view_only=0dfc46d7015d434daa6daf84b89e6ae0.

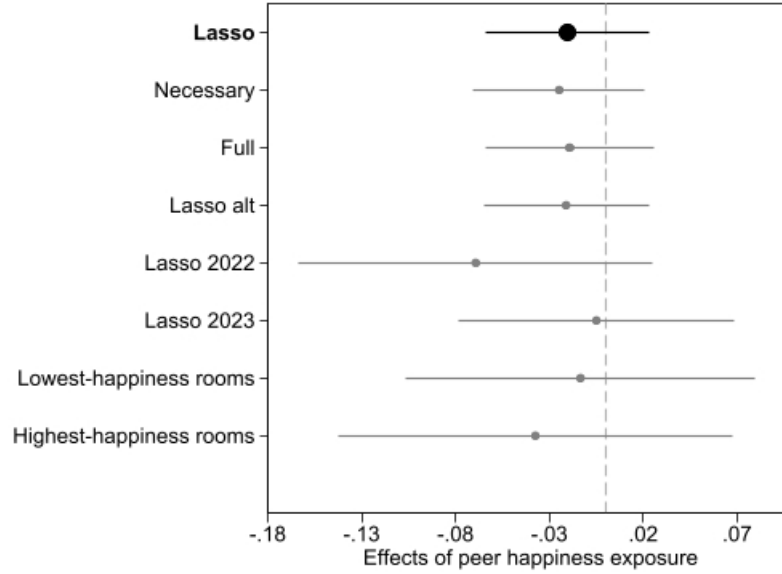


Figure 2: Effect of peers' baseline happiness on own endline happiness

Reading note: Points plot standardized coefficients. *Lasso* is the main specification using post-double-selection LASSO with baseline happiness, selected pre-treatment controls, and troop fixed effects; *Necessary* includes only baseline happiness and troop fixed effects (OLS); *Full* adds all pre-treatment covariates (OLS); and *Lasso alt* adds additional pre-treatment covariates (PDS Lasso). Separate PDS LASSO estimates are shown for the 2022 and 2023 cohorts, as well as for individuals assigned to the lowest- and highest-happiness rooms. Error bars show 95% confidence intervals with standard errors clustered at the room level.

can be interpreted as the effect of a one-standard deviation increase in peers' baseline happiness on own endline happiness, measured in standard deviation units. Our main specification (*Lasso*) uses post-double-selection LASSO to select control variables from the full set of pre-treatment covariates, while forcing baseline happiness and troop fixed effects into the model. The *Necessary* specification reports OLS estimates using a parsimonious set of controls (including baseline happiness and troop fixed effects), while *Full* reports OLS estimates including the full set of pre-treatment covariates. The *Alt* specification (PDS Lasso) additionally includes pre-treatment controls capturing respondents' motivation and orientation toward a military career—plans to continue in the Armed Forces, perceived suitability for service, motivation to contribute, and interest in military employment—as well as satisfaction with the final year of high school. The *2022* and *2023* specifications split the sample by cohort and re-estimate the model using post-double-selection LASSO; see Appendix Tables A.4 and A.5 for specifications with raw (unstandardized) variables. Finally, the *Low* and *High* specifications report estimates separately for individuals assigned to low- and high-happiness rooms (for more on this, see A.1.3).

Across all specifications, the estimated peer effects are small and statistically indistinguishable from zero, providing no evidence that happiness is contagious on average.

Sensitivity checks

We also investigate heterogeneous treatment effects by baseline levels of happiness and do not find any statistically significant differences (see Appendix Table A.6). Neither do we find any large non-linear effects of the effects of the peers. In Figure 3 we show a binned scatterplot of the effects of *Happiness in the room* on individual *Happiness* from the LASSO specification with a superimposed quadratic line based on all the data. All dots include approximately equally many observations and we superimpose a line based on the full individual level data. We see a very small negative correlation.

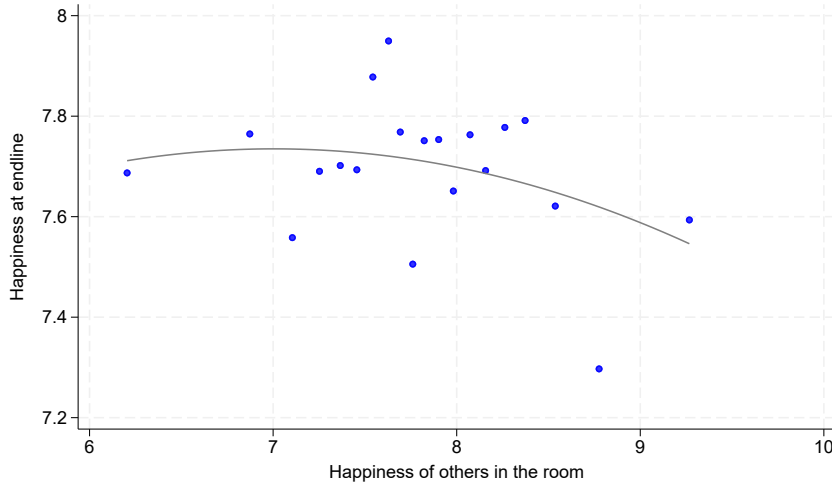


Figure 3: Endline happiness and happiness in the room

Reading note: The figure shows binned scatterplots where each dot contains equally many observations. The fitted quadratic line is estimated on all the underlying data and controls for the optimal controls chosen by the post double LASSO procedure.

In investigating who is changing their happiness score, Figure 4 presents a Sankey diagram illustrating the shifts in Happiness from baseline to endline. The width of the lines is proportional to the share of individuals transitioning from one response to another. We observe that the widest lines generally connect the same categories, indicating that many individuals provide consistent responses at both baseline and endline. However, there is also substantial movement, suggesting that changes do occur.

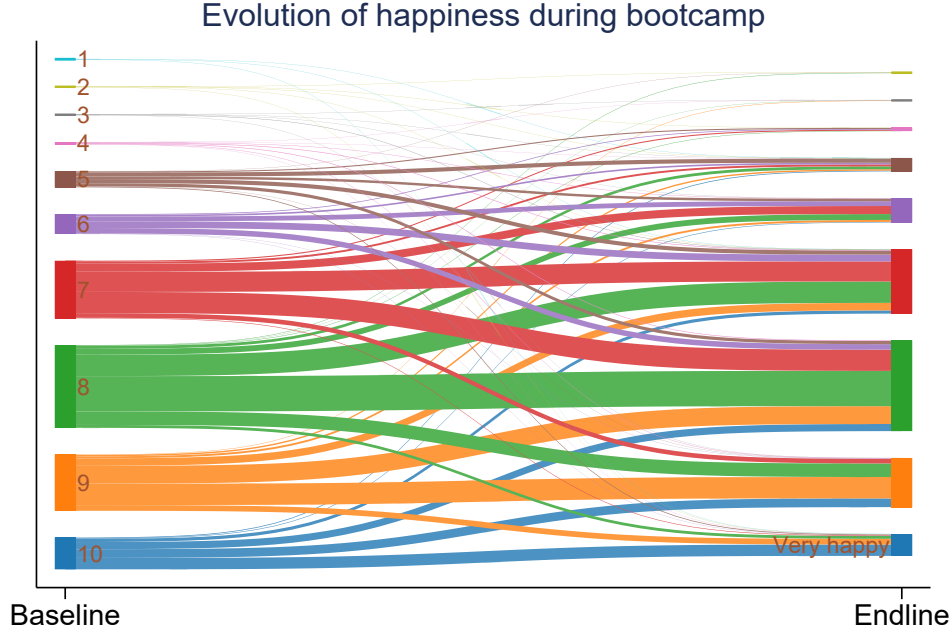


Figure 4: Movements in Happiness during boot camp

Reading note: The Sankey diagram shows the movements in Happiness during boot camp from baseline responses.

In sum, we find no peer effects in happiness. This is in sharp contrast to the strong peer effects in the boot camp of the Norwegian Armed Forces on gender attitudes (Dahl et al., 2021; Finseraas et al., 2016), attitudes towards immigrants (Finseraas et al., 2019; Finseraas & Kotsadam, 2017), and on support for authoritarian rule (Dahlum et al., 2024). More importantly, it is in contrast with all published evidence on happiness contagion among university students. Our study suggests that workplaces might not facilitate emotional contagion as effectively as more voluntary or socially intimate settings. Notably, this conclusion is *not* incompatible with the correlational findings of Fowler and Christakis (2008), who report significant happiness correlations among friends, family, and neighbors, but *not* among coworkers.

5 Discussion and conclusions

This study reports the results from the first large-scale field experiment testing whether happiness is contagious in an in-person setting outside of a university campus. The idea that emotions, especially happiness, spread via repeated interactions is widely accepted and influences public policies, workplace strategies, and even mental health interventions. Our study invites reconsid-

eration of this idea since, in our experiment, we find no evidence that happiness spreads within groups.

When interpreting the results of our field experiment, it is important to keep in mind that absence of evidence is not evidence of absence, and happiness might spread within our groups, but below a detectable level. Hence the importance of a large sample and of high statistical power, which allows us to conclude that happiness contagion, if it happens, has negligible effects in our groups. We do not claim to have demonstrated that happiness cannot spread within social networks. However, we believe we provide convincing evidence that happiness does not always spread in "real-world" environments that feature repeated in-person interactions.

Our findings have implications for well-being policies. The lack of substantial happiness spillovers challenges the idea that policy interventions increasing one person's happiness could have 'cascade effects' on others. On a positive note, our findings support the validity of widely used well-being estimation methods ([Frijters & Krekel, 2021](#); [Frijters et al., 2024](#); [HM Treasury, 2021](#); [Layard & Oparina, 2021](#)), as their disregard for happiness spillovers among non-family members does not overlook a crucial factor. Overall, our results provide a clearer understanding of which psychological characteristics are socially transmissible and under what conditions.

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Online Appendix

A.1 Variables coding

A.1.1 Coding of control variables and balance

When we have missing values on explanatory variables, we code the variables as zero and include dummy variables controlling for missing status so that we do not lose observations. To make the models fully saturated, we partition the covariate space and add control variables as indicator variables rather than using their multi-valued coding (Athey & Imbens, 2017). When cells are too small, with less than 5 percent of the observations, adjacent cells are combined. When using interaction terms and in tests of balance we retain the continuous coding of the variables. The coding choices of all variables follow the pre-specification in Dahlum et al. (2024):

Happiness, measured at baseline (same coding as for endline).

High trust: Answer to the question "Generally, would you say that most people can be trusted or that one cannot be careful enough?". 1= Cannot be careful enough to 10=Most people can be trusted. High trust are those answering high on this variable. We transform this into a binary variable by splitting it in a way that retains the ordering while it minimizes the difference in number of observations between the two categories.

Vote: Answer to the question "If there was a municipal election today, would you have voted?", where we code vote=1 if the respondents answer yes and zero otherwise.

Party choice: Dummy variables for party choice based on the answer to the question "If there was an election today, which party would you have voted for?". Parties with less than 5 percent of the answers are coded as "other".

High political interest: A dummy variable for answering high on the question: "How interested are you in politics". Answer categories range from 1=very little interest to 7=Very interested. We transform this into a binary variable by splitting it in a way that retains the ordering while it minimizes the difference in number of observations between the two categories.

Female: A dummy variable equal to one if the respondent is female.

High GPA: Self reported grades from high school at baseline, equal to 1 if grades are high and zero otherwise. We transform this into a binary variable by splitting it in a way that retains the ordering while it minimizes the difference in number of observations between the two categories.

High risk aversion: Answer to the question "In general, how willing are you to take risks?" The answer categories are from 1 to 10 where 1 is labeled "not willing to take risk at all", and 10 is labeled "very willing to take risk". We transform this into a binary variable by splitting it in a way that retains the ordering while it minimizes the difference in number of observations between the two categories. The ones with higher risk aversion are coded as one.

Mother and Father with high education (2 variables): Based on the question: "Do your parents have higher education (university/college)?" Original: 1= Yes, both have higher education, 2=My mother has higher education, my father has not, 3= My father has higher education, my mother has not, 4=No, neither of them have higher education. We recode the answers into two variables: Mother with high education (1/2=1, 3/4= 0) and Father with high education (1 and 3=1, 2 and 4=0)

Plan higher education: Based on the question: "Do you plan to take higher education?" Original: 1=Yes, 2=Don't know, 3=No. We recode the answers such that 2 and 3 equal 0.

To test for balance, we regress treatment on the main independent variables described in section A.1.1 together, while controlling for the troop fixed effects. The balance test for the *preferences in room* further controls for the average peer levels of baseline orientation in the troops to account for the artificial negative correlation between the outcome and individual baseline orientation otherwise induced (Guryan et al., 2009).

In Table A.7 we show the balance and we also show how the same control variables are correlated with *happiness* at endline. When regressing the treatment variable on the list controls

in column 1 we find that the baseline value of happiness is not statistically significant, which is comforting.

With many variables tested, some of them are likely to be different and in column 1 we see that one of the variables are statistically significantly correlated with the room treatment variable (High GPA), but only at the 10 percent level. The bias from this is likely minor since GPA is not correlated with happiness at endline (column 2).

Most importantly, the F-test of whether all the control variables jointly predict treatment is not statistically significant. In contrast, in column two we see from the F-test that the variables included are highly correlated with endline *happiness* which shows that they are relevant.

A.1.2 Additional control variables

Since we followed the pre-registration of a previous study, the control variables are not necessarily the ones we would have chosen for a study of happiness. We therefore code up a set of additional variables and include them in the regression. These variables are dummy variables capturing individuals' attitudes and intentions related to military service as well as satisfaction in high school.

In particular, these variables indicate whether the respondent plans to continue in the Armed Forces after completing initial service, feels suited to complete military service, is motivated to contribute in the Armed Forces, and is interested in working in the Armed Forces in the future. In addition, we include dummy variables for self-reported satisfaction with high school, based on a seven-point scale. Each level of satisfaction is represented by a separate dummy variable (but 1-3 is pooled together due to few responses). All variables are measured at baseline.

A.1.3 Non-linear effects

To assess whether peer effects in happiness differ across the distribution of peers' baseline happiness, we implement a median split (see Figure 2, based on regression results reported in Table A.10). Specifically, we divide the sample at the median of peers' baseline happiness, classifying observations below the median as belonging to "low-happiness rooms" and those above the median as belonging to "high-happiness rooms." This median split provides a transparent and symmetric way to assess potential non-linearities in peer effects while maintaining sufficient sample size and statistical power within each subgroup. Treatment effects are then estimated separately within each subsample. Within each group, peers' baseline happiness is re-standardized, and effects are estimated using post-double-selection LASSO, including troop fixed effects and clustering standard errors at the room level.

A.2 Tables and Figures Discussed in the Paper

Table A.1: Descriptive statistics

	(1)	
	Mean	SD
<i>Endline outcomes</i>		
Happiness	7.69	(1.36)
<i>Baseline values of outcome variables</i>		
Happiness	7.82	(1.47)
<i>Treatment variables</i>		
Happiness in the room	7.80	(0.73)
<i>Other control variables</i>		
Female	0.42	(0.49)
High political interest	0.52	(0.50)
High GPA	0.48	(0.50)
Mother with high education	0.74	(0.44)
Father with high education	0.63	(0.48)
Plan higher education	0.73	(0.44)
High risk	0.52	(0.50)
High trust	0.61	(0.49)
Vote	0.86	(0.34)
Party: Labour	0.14	(0.34)
Party: Conservative	0.32	(0.47)
Party: Progress	0.09	(0.28)
Party: Liberal	0.07	(0.26)
Party: Socialist Left	0.06	(0.24)
Party: Other	0.07	(0.26)
Party: Unknown	0.25	(0.43)
<i>N</i>	1508	

Notes: The sample includes everyone answering at baseline where we also have information about rooms.

Table A.2: Descriptive statistics by year

	2022		2023	
	Mean	SD	Mean	SD
<i>Baseline values of outcome variables</i>				
Happiness	7.92	(1.51)	7.73	(1.42)**
<i>Other control variables</i>				
Female	0.40	(0.49)	0.43	(0.49)
High political interest	0.52	(0.50)	0.52	(0.50)
High GPA	0.48	(0.50)	0.48	(0.50)
Mother with high education	0.72	(0.45)	0.75	(0.43)
Father with high education	0.62	(0.49)	0.63	(0.48)
Plan higher education	0.72	(0.45)	0.75	(0.44)
High risk	0.51	(0.50)	0.53	(0.50)
High trust	0.61	(0.49)	0.60	(0.49)
Vote	0.86	(0.35)	0.87	(0.34)
Party: Labour	0.14	(0.35)	0.13	(0.34)
Party: Conservative	0.28	(0.45)	0.35	(0.48)***
Party: Progress	0.10	(0.29)	0.08	(0.27)
Party: Liberal	0.06	(0.24)	0.08	(0.27)
Party: Socialist Left	0.07	(0.26)	0.05	(0.22)
Party: Other	0.08	(0.27)	0.06	(0.24)
Party: Unknown	0.27	(0.44)	0.24	(0.43)
<i>Endline outcomes</i>				
Happiness	7.63	(1.44)	7.75	(1.29)
N	676		832	

Notes: The sample includes everyone answering at baseline where we also have information about rooms. P-values for t-tests of differences are ≤ 0.01 ***, ≤ 0.05 **, and ≤ 0.1 *.

Table A.3: Peer effects on happiness (Pooled sample)

	(1)	(2)	(3)
Happiness in the room	-0.047 (0.041)	-0.053 (0.043)	-0.046 (0.043)
Mean in sample	7.69	7.69	7.69
N	1508	1508	1508
Controls	Lasso	Necessary	Full

Notes: The necessary controls include troop fixed effects and baseline happiness. Standard errors, clustered at the room level, are in parentheses. P-values are ≤ 0.01 ***, ≤ 0.05 **, and ≤ 0.1 *.

Table A.4: Peer effects on happiness (2022 sample)

	(1)	(2)	(3)
Happiness in the room	-0.10 (0.067)	-0.10 (0.070)	-0.083 (0.068)
Mean in sample	7.63	7.63	7.63
N	676	676	676
Controls	Lasso	Necessary	Full

Notes: The necessary controls include troop fixed effects and baseline happiness. Standard errors, clustered at the room level, are in parentheses. P-values are ≤ 0.01 ***, ≤ 0.05 **, and ≤ 0.1 *.

Table A.5: Peer effects on happiness (2023 sample)

	(1)	(2)	(3)
Happiness in the room	-0.011 (0.054)	-0.021 (0.054)	-0.025 (0.055)
Mean in sample	7.75	7.75	7.75
N	832	832	832
Controls	Lasso	Necessary	Full

Notes: The necessary controls include troop fixed effects and baseline happiness. Standard errors, clustered at the room level, are in parentheses. P-values are $\leq 0.01^{***}$, $\leq 0.05^{**}$, and $\leq 0.1^*$.

Table A.6: Heterogeneity in Peer effects on happiness (Pooled sample)

	(1)
Happiness in the room	-0.041 (0.043)
Baseline happiness (standardized)	0.74* (0.43)
Baseline happiness*Happiness in the room	-0.018 (0.056)
Mean in sample	7.69
N	1508
Controls	Lasso

Notes: Baseline happiness is standardized to have mean 0 and standard deviation 1. Standard errors, clustered at the room level, are in parentheses. P-values are $\leq 0.01^{***}$, $\leq 0.05^{**}$, and $\leq 0.1^*$.

Table A.7: Balance tests

	(1) Happiness in room	(2) Endline happiness
Happiness	-0.047 (0.074)	0.44*** (0.025)
Female	0.0081 (0.037)	0.14** (0.066)
High political interest	-0.034 (0.043)	-0.048 (0.064)
High GPA	0.076* (0.042)	0.023 (0.064)
Mother with high education	0.052 (0.039)	-0.082 (0.078)
Father with high education	0.016 (0.041)	0.053 (0.072)
Plan higher education	-0.063 (0.047)	0.065 (0.080)
High risk	-0.046 (0.051)	0.16** (0.082)
High trust	-0.026 (0.054)	0.24*** (0.082)
Vote	-0.0074 (0.061)	0.27*** (0.10)
Party: Labour	0.11 (0.072)	0.030 (0.16)
Party: Conservative	0.091 (0.071)	0.097 (0.14)
Party: Progress	0.026 (0.081)	0.038 (0.20)
Party: Liberal	0.024 (0.086)	-0.20 (0.16)
Party: Socialist Left	0 (.)	0 (.)
Party: Other	0.11 (0.088)	-0.10 (0.18)
Party: Unknown	0.077 (0.071)	-0.23 (0.15)
N	1508	1508
P-value of F-test	0.68	0.000

Notes: All independent variables are measured at baseline. The sample includes everyone answering at baseline where we also have information about rooms. Standard errors, clustered at the room level, are in parentheses. P-values are $\leq 0.01^{***}$, $\leq 0.05^{**}$, and $\leq 0.1^*$. The F-tests are tests of all variables jointly being zero.

Table A.8: Attrition

	(1)	(2)	(3)
Happiness in the room	-0.0023 (0.012)	-0.0041 (0.012)	-0.0043 (0.012)
Mean in sample	0.10	0.10	0.10
N	1668	1668	1668
Controls	Lasso	Necessary	Full

Notes: The sample includes all individuals answering at baseline. Attrition is defined as the individuals that answered at baseline but not at endline. The necessary controls include troop fixed effects and baseline attitudes. Standard errors, clustered at the room level, are in parentheses. P-values are $\leq 0.01^{***}$, $\leq 0.05^{**}$, and $\leq 0.1^*$.

Table A.9: Peer effects on happiness with extra control variables

	(1)
Happiness in the room	-0.049 (0.041)
Mean in sample	7.69
N	1508
Controls	Lasso

Notes: The necessary controls include troop fixed effects and baseline happiness. Standard errors, clustered at the room level, are in parentheses. P-values are $\leq 0.01^{***}$, $\leq 0.05^{**}$, and $\leq 0.1^*$.

Table A.10: Effect of peers' baseline happiness on own endline happiness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Lasso	Necessary	Full	Alt	2022	2023	Low	High
Peers' baseline happiness	-0.020 (0.022)	-0.025 (0.023)	-0.019 (0.023)	-0.021 (0.022)	-0.051 (0.036)	-0.004 (0.028)	-0.010 (0.035)	-0.028 (0.040)
Observations	1508	1508	1508	1508	676	832	755	753

Notes: The table reports the regression results underlying the coefficient plot in Figure 2. Outcome variables and the peer-effects measure are standardized to have mean zero and standard deviation one. "Lasso" is the main specification using post-double selection Lasso with baseline happiness and troop fixed effects; "Necessary" is an ordinary least squares regression (OLS) including only baseline happiness and troop fixed effects; "Full" (OLS) adds all pre-treatment covariates from the pre-plan, baseline happiness, and troop fixed effects; "Lasso alt" adds covariates beyond the pre-plan (pre-treatment measures of military motivation and career orientation, and satisfaction with the final year of high school). Separate Lasso estimates are shown for the 2022 and 2023 cohorts. Additional estimates are reported separately for the lowest- and highest-happiness rooms (also post-double selection Lasso). All specifications include troop fixed effects and baseline happiness as controls. Standard errors are clustered at the room level and reported in parentheses. P-values are $\leq 0.01^{***}$, $\leq 0.05^{**}$, and $\leq 0.1^*$.

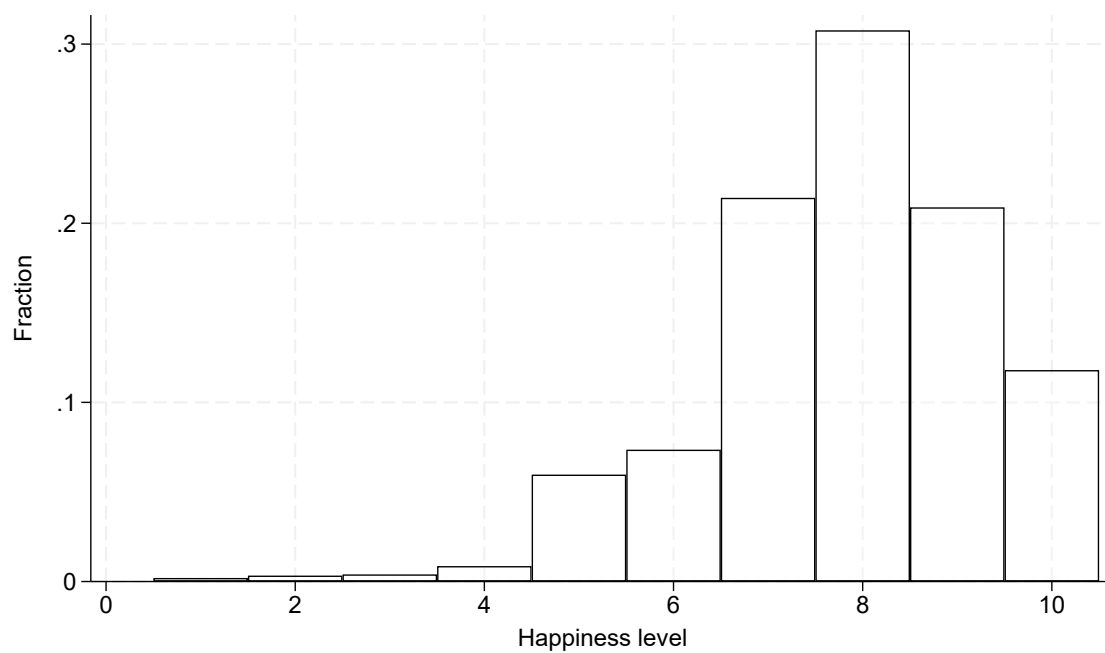


Figure A.1: Happiness at baseline.

Notes: The sample consists of all recruits that answered the survey at baseline.