

# Converging to the Lowest Common Denominator in Physical Health

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**Objective:** This research examines how access to information on peer health behaviors affects one's own health behavior. **Methods:** We report the results of a randomized field experiment in a large corporation in which we introduced walkstations (treadmills attached to desks that enable employees to walk while working), provided employees with feedback on their own and their coworkers' usage, and assessed usage over 6 months. We report how we determined our sample size, and all data exclusions, manipulations, and measures in the study. **Results:** Walkstation usage declined most when participants were given information on coworkers' usage levels, due to a tendency to converge to the lowest common denominator—their least-active coworkers. **Conclusion:** This research demonstrates the impact of the lowest common denominator in physical activity: people's activity levels tend to converge to the lowest-performing members of their groups. This research adds to our understanding of the factors that determine when the behavior of others impacts our own behavior for the better—and the worse.

**Keywords:** social norms, sedentary lifestyle, physical activity, behavior change, field experiment

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Up to 40% of premature deaths in the United States are attributable to modifiable behaviors such as physical inactivity and overeating (Schroeder, 2007). Despite being aware of the negative health consequences (Bartlett, Miller, Rice, & Max, 1994; Hughes, 2003; Viscusi, 1992), individuals continue to engage in these behaviors, even when such behaviors are modifiable—such as choosing to exercise more and eat less. Moreover, some of these behaviors have been on the rise, as evidenced by the increase in obesity and physical inactivity in recent decades. For example, whereas half of U.S. occupations required at least moderate physical exercise in the 1960s, just one fifth did so in 2010 (Church et al., 2011). As a result, average daily occupation-related energy expenditure has decreased by more than 100 calories over the same time period, a reduction that alone can account for a significant portion of the increase in average body weight (Church et al., 2011).

Many of these adverse—but modifiable—health behaviors can be both social and visible. For example, exercise is sometimes a public or team activity, and the weight (and weight gain) of peers is readily observable. Might these commonalities provide a clue as to why poor health spreads through populations? A large body of previous research has shown that observing others' behaviors can have a profound effect on one's own behavior, with people aligning their behavior with norms set by others (Cialdini, Reno, & Kallgren, 1990; Schultz, Nolan, Cialdini, Goldstein, & Griskevi-

cius, 2007). And for behaviors that require effort to perform—as is the case with many positive health behaviors—knowledge of a peer's failure to engage in a positive behavior may prove especially influential. In one study for example, many participants who had agreed to volunteer their time stopped volunteering as soon as they observed another person stopping (Linardi & McConnell, 2011).

Recent research suggests that peers may exert more downward than upward pressure on health behaviors. One study assessed the likelihood of failing the U.S. Air Force Academy's fitness requirements as a function of the high school fitness level of one's squadron mates (Carrell, Hoekstra, & West, 2011). Students randomly assigned to squadrons with peers who had been less fit in high school (i.e., received poor grades in physical education classes) were more likely to fail the academy's fitness requirements. Moreover, analysis of a large longitudinal data set suggests that weight gain tends to "spread" though social networks, whereas weight loss does not (Christakis & Fowler, 2007; Hill, Rand, Nowak, & Christakis, 2010). To explain this result, Hill et al. (2010) introduce a model that incorporates *indirect contact* into traditional models of contagion. Like traditional infectious diseases, behavioral phenomena can "spread" through direct social interaction; however, unlike traditional infectious diseases, behavioral phenomena can also spread in the absence of such contact—by merely receiving information on others' behavior.

Although this previous research suggests that such indirect contact can have a negative effect on one's health behavior (Carrell et al., 2011; Hill et al., 2010; Linardi & McConnell, 2011), the causal impact of such contact has yet to be assessed. In these studies, access to such information was not manipulated: *all* participants were exposed to information on their peers' health behaviors. In the present study, we test the causal impact of indirect contact—in the form of peer health information—on one's own health behavior, by randomizing participants to receive different

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levels of information about their peers' physical activity levels. We therefore experimentally test a fundamental implication of the Hill et al. (2010) model of contagion of behavioral phenomena, namely that indirect contact in the form of mere exposure to information on others' health behaviors can have a harmful effect on one's own behavior.

## Overview

We conducted a randomized field experiment in a large corporation in which we examined the causal impact of observing the health behaviors of others on one's own health behaviors, by manipulating individuals' exposure to information on the behavior of their peers. Participants were given the opportunity to reduce their sedentary workplace behavior by using walkstations—slow-moving treadmills attached to elevated desks enabling them to walk while working. We varied whether participants had access to information on their coworkers' walkstation usage. Some participants viewed only their own usage, some also viewed the usage of one coworker, and others also viewed the usage of four coworkers. We found usage to be lowest for participants assigned to receive feedback on their coworkers' usage because of a tendency to converge to the worst performer in their group.

## Method

Employees ( $N = 225$ ; 84.8% women; 76.2% Caucasian, 9.4% African American, 5.8% Hispanic; 6.7% Asian) at a company in the northeastern United States participated in a 6-month between-subjects randomized field experiment.

Approximately half of participants were randomly assigned to one of three experimental conditions ( $N = 122$ ), in which participants were asked to use the walkstations and received a personalized weekly e-mail. Participants in the solo condition ( $n = 33$ ) were e-mailed information on their own walkstation usage for the previous week. In the duo condition ( $n = 40$ , to create 20 duos)—in which each participant was randomly paired with another participant—the e-mail listed the names of both members of the given duo, along with each person's usage for the previous week. In the quintet condition ( $n = 50$ , to create 10 quintets)—in which each participant was randomly grouped with four other participants—the e-mail indicated each participant's name and corresponding walkstation usage for the previous week. The other participants ( $N = 103$ ) were randomly assigned to a hold-out control group that could not use the walkstations until the study was completed, enabling us to examine the effect of walkstations on secondary measures, reported in the Supplemental Online Material (SOM).

The study was conducted with employees from three different work sites in the same city. Participants were randomized at the individual level, and randomization was stratified by site (i.e., participants were assigned to the treatment conditions in equal proportions across work sites). Two walkstations were installed at each work site and participants signed up for 1-hr time slots. Participants were encouraged to sign up for three sessions per week.

We obtained the largest sample size possible, given the constraint that there were only two walkstations per work site. We determined our sample size by estimating that on average, two thirds of participants would use the walkstation the recommended

three times per week. We further estimated that to allow sufficient flexibility in time-slot scheduling, we could have a maximum of 40 participants per site, producing a target sample size of approximately 120 participants in the experimental condition. To use the walkstations, employees entered their unique employee ID. Our primary outcome measure was walkstation usage operationalized by login data, which allowed us to assess how long each employee used the machine.

In our primary data analysis, we tested for differences between experimental conditions in usage (in minutes) using a mixed model with a random intercept for participant and fixed effects for experimental condition, week number, and their interaction. We also included a fixed effect for work site. For ease of explication we have reported the results of the mixed model here; however, we obtained similar results when we ran an alternative, two-part model.

One participant assigned to a duo withdrew before participants had been informed of their random assignment and therefore before data collection began. The results are similar if we include or exclude this duo in our analyses. In the experimental conditions, 21 participants did not use the walkstations at any point in time (*NS* between conditions). Not all participants used the walkstation every month, and the proportion of participants failing to use the walkstations increased over time. For example, in Month 1, 82% of participants used the walkstations at least once (*NS* between conditions). By Month 6, that percentage had fallen to 44% of participants (*NS* between conditions). For any given week on which any given participant did not use the walkstation, he or she contributed a value of zero weekly minutes to the data set.

## Results

Figure 1 depicts the mean number of weekly minutes spent on the walkstations over time, by condition. Walkstation usage declined over time ( $p < .01$ ). We observed significant differences between conditions in walkstation usage ( $p < .05$ ). Follow-up testing revealed that participants in both the duo and quintet conditions spent less time on the walkstations relative to those in the solo condition ( $p < .02$ ). These main effects were qualified by

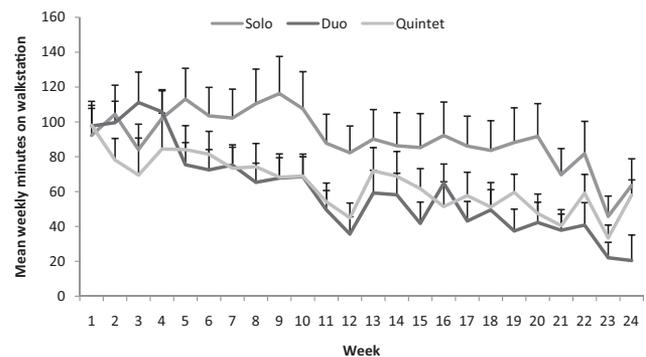


Figure 1. Mean weekly minutes spent on the walkstations, by experimental condition. Error bars represent  $\pm 1$  standard error from the mean.

an interaction between time and feedback condition ( $p = .01$ ).<sup>1</sup> Below, we report analyses broken out by quintets and duos. These analyses suggest that the pattern of usage decline seen in the duos and quintets is driven by convergence to the usage of the least frequent users, who we refer to as the “lowest common denominators.”

### Quintet

We identified each quintet’s best performer (the quintet member with the highest usage in Month 1), worst performer (the quintet member with the lowest usage in Month 1—the lowest common denominator), and the three “middling” performers in Month 1 and then tracked their behavior in Months 2 through 6. Below, we group the three middling performers for ease of explication. The results are similar if each performer type is reported separately; see SOM for more information.

Middling performers’ usage converged over time to that of the lowest common denominator, rather than being pulled upward by the best performer (Figure 2A). Walkstation usage was significantly different between all types in Month 1, such that top performers’ usage was higher than both middling and worst, and middling was higher than worst (all  $ps < .01$ ). By Month 6 however, middling performers’ usage was statistically indistinguishable from that of the worst performers ( $p = .78$ ), suggesting that they had converged to these lowest common denominators. Top performers, in contrast, remained significantly different from both middling and worst performers at 6 months (all  $ps < .01$ ).

By contrast, when we grouped solo participants into “synthetic” quintets—by pulling random sets of five participants from the solo condition (cf. Ariely & Levav, 2000; Baron, Kerr, & Miller, 1992; Diehl & Stroebe, 1991; Laughlin, VanderStoep, & Hollingshead, 1991); we saw no evidence of similar convergence (Figure 2B).<sup>2</sup> Usage was significantly different between all types in Month 1 (all  $ps < .01$ ) and generally remained so throughout Month 6—including between the middling and worst performers ( $p < .05$ ), best and middling ( $p = .08$ ), and best and worst ( $p < .01$ ). Thus, in the absence of social feedback, we do not observe convergence to the lowest common denominator.

### Duos

For duos, the same patterns are apparent: over time the better performer is pulled more toward the bottom performer than the reverse (Figure 3A), and we do not see a similar pattern when we grouped participants in the solo condition into post hoc “duos” (Figure 3B). In real duos, usage was significantly different between types in Month 1 ( $p < .01$ ), but not at Month 6 ( $p = .10$ ). In synthetic duos, usage was significantly different at both time points (both  $ps \leq .05$ ).

### “Good” and “Bad” Groups

Quintets and duos differed in the aggregate number of minutes their members spent on the walkstations, with some groups generally using the walkstations more than others. To test whether the lowest common denominator effect was robust to performance level, we summed the number of minutes each member of each quintet spent on the walkstation in Month 1. Quintets with above

median usage in Month 1 were classified as “good” quintets; those falling at or below the median were “bad” quintets. The lowest common denominator effect emerges in both good and bad quintets. In both quintet types, walkstation usage was significantly different between all performer types in Month 1 (all  $ps \leq .05$ ). By Month 6, however, there was no difference between the middling and worse performers in either (both  $ps > .21$ ). Figure 4 depicts the lowest common denominator effect separately for good quintets (A) and bad quintets (B).

A parallel analysis of duos reveals a similar pattern. In both the good and bad duos, the better and worse performers’ usage is significantly different in Month 1 (both  $ps < .01$ ) but not Month 6 (both  $ps > .14$ ). Figure S2 in the SOM depicts the lowest common denominator effect separately for good duos and bad duos.

These analyses suggest that even in groups that start out with a relatively positive norm for physical exercise, members continue to converge to the worst performer. Thus the same individual who exerts a negative effect on the behavior of his peers when the least active member of a good group is unlikely to exert a positive influence on others’ activity levels when the most active member of a bad group.

## General Discussion

We examined how access to information on peer health behaviors affects one’s own behavior by experimentally manipulating access to information on peers’ behaviors. Specifically, we conducted a longitudinal randomized field experiment in which we introduced walkstations (treadmill desks), provided employees with information on their own and their coworkers’ usage, and assessed usage over 6 months. Given the difficulty of changing health behavior, one prediction would simply be that we would observe a general decline in usage across all employees. And indeed, we did observe such an overall decline. Importantly, however, this effect was moderated by the accessibility of peer information—walkstation usage declined more for people who received information about one or several peers than those who received information only on their own usage, because of a tendency for people to converge to the lowest common denominator in their group. In addition, the fact that the lowest common denominator effect does not emerge in the synthetic groups suggests results are driven by our manipulation (peer information) rather than mere overall regression to zero minutes.

The subtlety of our manipulation—varying the content of weekly e-mails—likely made cross-contamination *across* conditions minimal. It is possible, however, that greater communication occurred *within* the duo and quintet conditions, which could have reinforced the downward slope in usage in these conditions. In

<sup>1</sup> We obtained similar results when we ran an alternative, two-part model: (1) A logistic regression revealed that relative to the duo and quintet conditions, participants in the solo condition were more likely to use (i.e., spend positive minutes on) the walkstation ( $p < .01$ ). (2) Among participants who used the walkstation on any given week, there was a significant interaction between time and feedback condition ( $p < .01$ ).

<sup>2</sup> There were 33 subjects in the solo condition; therefore, in creating the six synthetic quintets, three randomly selected solo participants were excluded. Similarly, one randomly selected solo participant was excluded in creating the 16 synthetic duos for the parallel analysis reported below.

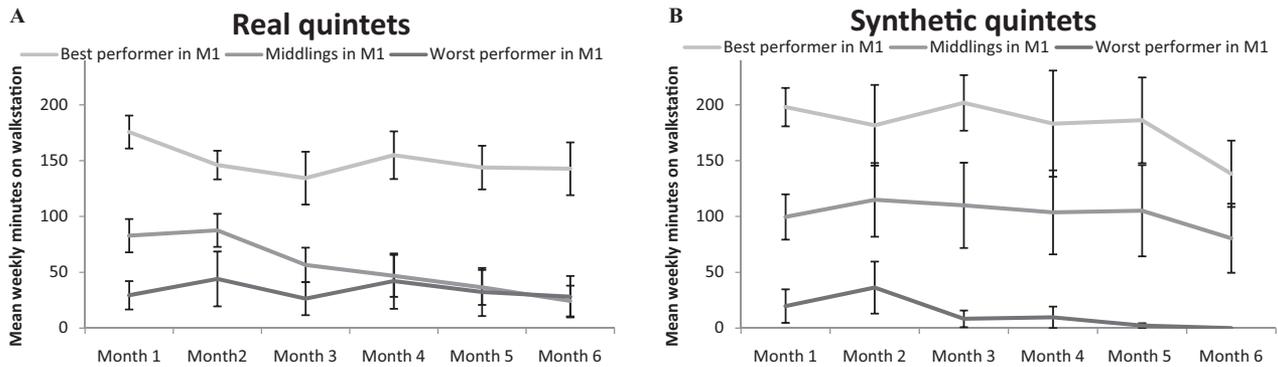


Figure 2. Mean weekly minutes spent on the walkstations, by month and performer type. Middling behavior converges to the worst performer in real quintets (A) but not in synthetic quintets (B). Error bars represent  $\pm 1$  standard error from the mean.

addition, although the members of duos and quintets were assigned randomly—thus eliminating friend selection and providing a cleaner estimate of the influence of indirect peer contact—it is possible that some members of some groups were in fact friends and had direct contact, given that all participants were coworkers in the same organization. Future research (perhaps outside of a field setting where contamination is difficult to control) should systematically vary the frequency with which participants can (or cannot) communicate about their physical health and the membership of those groups (friends vs. strangers).

Although this research was restricted to three sites of a large corporate workplace, it nonetheless has a variety of important implications and raises avenues for future research. The situation we explore—people merely receiving information on the health behaviors of their peers in the absence of direct communication (e.g., face-to-face interactions or phone conversations; Hill et al., 2010)—is becoming increasingly common because of the rise of social media; the introduction of lightweight, inexpensive electronic sensors used to monitor health behavior; and a general societal shift toward openness with personal information (John, Acquisti, & Loewenstein, 2011; Wolf, 2010). In turn, these devel-

opments have spawned new ways of disseminating and receiving health information on others; for example, Nike's Fuelband enables users to track and share fitness goals with their social networks. Underlying many such applications are the assumptions that disseminating and receiving information on peer health behavior positively affects one's own behavior, an assumption that the present research calls into question.

The present research examined the joint effect of disseminating and receiving walkstation usage history on employees' propensity to reduce their sedentary workplace behavior: employees in the duo and quintet conditions both observed and were observed by their peers. Future research should separate the unique effects of dissemination versus reception of health information on health behavior change. For example, it is possible that publicly posting one's gym attendance records—or perhaps precommitting to do so—is motivating. At the same time, this positive effect of dissemination could be overridden when paired with receiving information that peers are failing to attain their gym attendance goals. Parsing out the effect of dissemination versus reception of health information has important practical implications for the optimal design of health interventions.

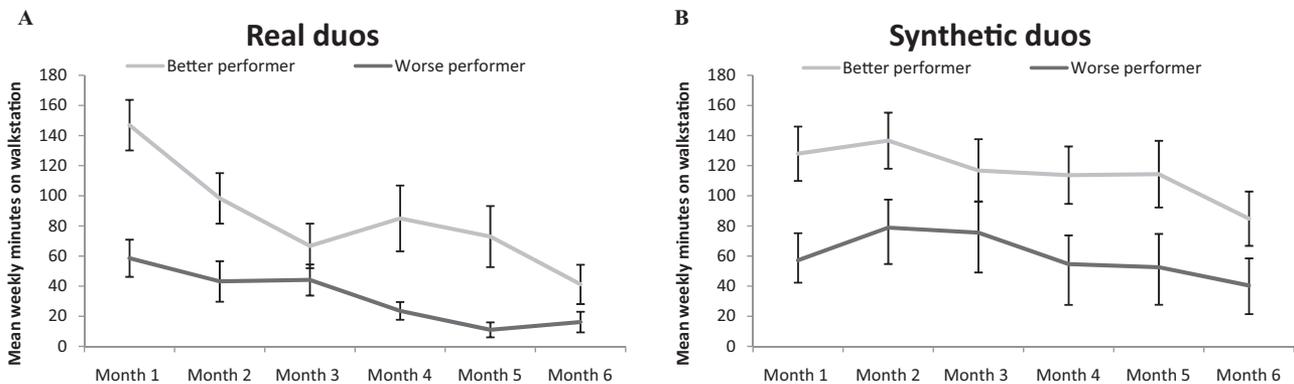


Figure 3. Mean weekly minutes spent on the walkstations, by month and performer type. Dyad behavior converges to the worst performer in real duos (A) but not in synthetic duos (B). Error bars represent  $\pm 1$  standard error from the mean.

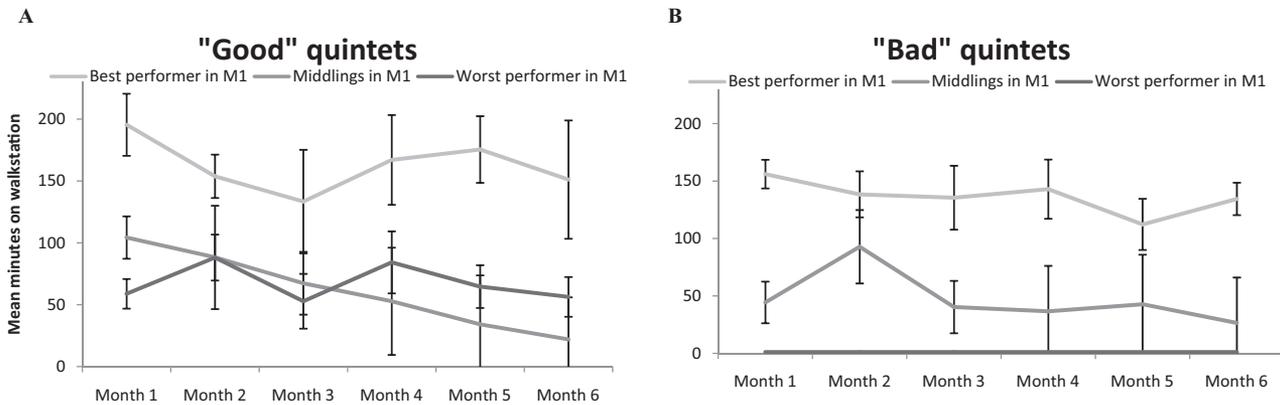


Figure 4. Good quintets (A) and bad quintets (B). Error bars represent  $\pm 1$  standard error from the mean.

Why does peer information appear to exert more downward than upward pressure on walkstation usage and on health more generally (e.g., exercise and weight gain, see Carrell et al., 2011; Christakis & Fowler, 2007; Hill et al., 2010)? For example, we could easily have observed a “convergence to the highest common denominator” whereby the very best performer led laggards to exercise more. Although speculative, one explanation lies in the general and well-documented tendency for negative stimuli to exert more influence on behavior than positive stimuli (Baumeister, Bratslavsky, Finkenauer, & Vohs, 2001; Fiske, 1980). Because—all else being equal—“bad is stronger than good,” it is possible that information about negative peer health behavior exerts more weight on our own behavior than positive peer behavior. At the same time, however, peers clearly do not have an invariably negative impact on one’s own health behaviors; indeed, active social support from peers can be beneficial in improving people’s health (Berkman, 1986; Cohen & Syme, 1985). Our results suggest that exposure to peers’ health information in the absence of social support may be more likely to reinforce bad behavior than to encourage good behavior.

As a result, and consistent with Hill et al. (2010), our results in theory provide a case for “quarantining” low performers. However, doing so would limit the ability for high performers to positively impact the behavior of low performers. How might the potential for high performers to improve the standing of low performers be harnessed? One possibility is to pair information provision with social support. In our study, the lowest common denominator effect could be mitigated by encouraging groups to interact with each other, perhaps even delegating the top performers to be “team leaders.” In a recent study, diabetics with poor blood glucose control were assigned peer mentors: diabetics who had recently gotten high blood glucose under control (Long, Jahnle, Richardson, Loewenstein, & Volpp, 2012). Relative to a control condition in which participants were not paired up, peer mentoring resulted in better blood glucose control. Pairing peer information with peer support may therefore reverse the lowest common denominator effect. As noted above, studies that systematically vary access to both peer information and peer contact are essential in unpacking the positive and negative effects of social information.

Interventions that pair the provision of peer health information with social support, however, can be resource-intensive. Could

high performers affect low performers through the mere provision of information? One possibility is to pair peer information with a normative standard. For example, Schultz et al. (2007) gave residents information on their own electricity consumption, plus that of their neighbors. In the absence of a normative standard, receiving peer consumption information caused the high performers (in this case, low consumers of electricity) to increase their energy usage toward the low performers; this perverse effect was mitigated when a clear normative standard was provided. A second possibility is to present peer health information selectively, increasing the likelihood that the high performers will positively affect the low performers. For example, had we shown participants in quintets *only* data from the top performer, thereby decreasing the salience of the lowest common denominator, we may have changed behavior for the better.

In summary, our results suggest that the mere provision of information on peer health behaviors can have perverse effects on one’s health behavior. This research therefore adds to our understanding of the factors that determine when the behavior of others impacts our own behavior for the better—and the worse.

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