

RESEARCH REPORT

Getting Unstuck: The Effects of Growth Mindsets About the Self and Job on Happiness at Work

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Past research on growth mindsets has focused on the benefits of viewing the self as flexible rather than fixed. We propose that employees can make more substantial agentic changes to their work experiences if they also hold growth mindsets about their job designs. We introduce the concept of dual-growth mindset—viewing both the self and job as malleable—and examine its impact on employee happiness over time. We hypothesize that fostering a dual-growth mindset yields relatively durable gains in happiness, while fostering a growth mindset about either the self or job is insufficient for sustainable increases in happiness. We tested these predictions using two experimental studies: a field quasi-experiment in a Fortune 500 technology company and a controlled experiment with employees in a variety of organizations and occupations. Across the two experiments, fostering dual-growth mindset yielded gains in self-reported and observer-rated happiness that lasted at least 6 months. Fostering growth mindsets about either the self or job alone did not generate lasting increases in happiness. Supplementary mediation analyses suggest dual-growth mindsets boosted happiness by enabling employees to plan more substantial job crafting. Our research suggests that durable gains in happiness at work depend on holding flexible mindsets about the job, not only the self.

Keywords: growth mindset, happiness, agency, job crafting, experiments

At work, a surprising number of the limits on our happiness may be of our own making. Scholars have long recognized how psychological experiences depend on agentic change—exercising the capacity to purposefully shape and alter one’s actions and circumstances (Bandura, 2000, 2018; Hirst et al., 2020). Indeed, a growing body of research suggests that employees can and do make agentic changes to their experiences in organizations, thereby improving their own happiness on the job (e.g., Cangiano et al., 2019; Plomp et al., 2016; Taris & Wielenga-Meijer, 2010; Tims et al., 2013).

Yet, many employees feel more like sculptures in undesirable circumstances than sculptors of their own happiness (Bell & Staw, 1989). All too often, employees “seize and freeze” (Kruglanski & Webster, 1996) on a fixed view of human qualities, missing opportunities for agentic change (Dweck, 1999; Heslin et al., 2005, 2006; Seitz & Owens, 2020). Psychologists have long demonstrated that

mindsets about the self vary along a spectrum from fixed to growth (Dweck, 1986, 2006). Individuals who hold a fixed mindset believe people are largely unchangeable, while those with growth mindsets believe people can change their abilities and traits (Madan et al., 2019). Broadly, this literature suggests that when people hold growth mindsets, they are more inclined to exercise agency to change key aspects of themselves in desirable ways (Dweck, 2006; Sue-Chan et al., 2012).

However, following the view that both the person and situation must be considered for a full understanding of human agency (Bandura, 2006), making changes to oneself represent only the person side of agentic change, overlooking the situation side. In organizations, employees’ situations are shaped by their job designs, or the bundle of tasks and relationships for which they are responsible (Ilgen & Hollenbeck, 1991). Whereas prior research has focused on mindsets

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Some aspects of the intervention materials in the two studies are based on a tool called the Job Crafting Exercise, the copyright for which is owned by the University of Michigan. Justin M. Berg and Amy Wrzesniewski are cocreators of this tool and have a financial interest in it.

Online Materials: <https://osf.io/ka7m3>

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about the self, we propose that employees can also hold fixed or growth mindsets about their jobs. Just as people may mistakenly view abilities as fixed that are in fact malleable (Chiu et al., 1997; Heslin et al., 2005, 2006), employees may view their job designs as more fixed than they are in reality (Berg et al., 2010; Gordon et al., 2018; Kooij et al., 2017). According to Ilgen and Hollenbeck (1991, p. 197), managers often design jobs to be “bureaucratic” and “quasistatic,” so that the “elements comprising the jobs exist independently of job incumbents,” and “do not change on a day-to-day basis, but rather are relatively constant over time.” During socialization and training processes, employees learn their jobs as a set of routines (Nelson & Winter, 1982; Van Maanen & Schein, 1979). Research has long shown that repeated enactment of routines makes them automatic, to the point that employees often do not reflect on whether these routines can be changed (Ashforth & Fried, 1988; Bargh & Chartrand, 1999; Dane, 2011; Langer, 1989; Parker et al., 1997, 2019). Thus, even employees in high-ranking, autonomous positions may come to view their jobs as relatively fixed (Berg et al., 2010).

To capture the idea that employees may hold mindsets about the flexibility of their job designs, we introduce the concept of job-growth mindset, or beliefs about the malleability of the bundle of tasks and relationships that comprise one’s job. Job-growth mindset complements the notion of self-growth mindset from past research. In particular, we propose that fostering both mindsets at the same time, which we call dual-growth mindset, may be most conducive to agentic change at work. Importantly, fostering dual-growth mindset may enable employees to make agentic changes that boost their own happiness, a valued outcome that is notoriously difficult to durably change (Fisher, 2010). According to research on the hedonic treadmill (Brickman & Campbell, 1971; Diener & Oishi, 2005; Kahneman, 1999), positive events may spark temporary increases in happiness, but individuals often quickly return to their baseline level of happiness. At the same time, evidence suggests that individuals can sustainably boost their happiness through intentional, agentic change (Lyubomirsky et al., 2005). We propose that fostering dual-growth mindset may enable such agentic change, leading to relatively durable gains in happiness. In contrast, fostering either self- or job-growth mindset alone may fail to produce agentic changes that are substantial enough to sustainably enhance happiness.

Dual-Growth Mindset and Happiness

When employees develop a dual-growth mindset, they come to see both the self and job—that is, the person and situation—as relatively flexible. This should increase employees’ capacity to make agentic changes in their work (Bandura, 2006, 2018). However, not all agentic changes are likely to durably heighten happiness. According to Lyubomirsky et al. (2005), changes are more likely to increase happiness when they produce experiences that are volitional, self-concordant (matching one’s interests and motivations), effortful, and novel (substantially different from one’s prior routines). Fostering dual-growth mindset should encourage agentic changes that meet these four criteria, while the agentic changes from fostering either self- or job-growth mindset may only meet the first two criteria (volitional and self-concordant). The latter two criteria—effortful and novel—are important because they help keep the experiences “fresh,” mitigating the adaptation and habituation processes that drive the hedonic treadmill. Small changes are unlikely to be effortful or novel enough to durably boost happiness. To have a meaningful

impact on happiness, agentic changes need to be relatively substantial.

Fostering dual-growth mindset should enable more substantial agentic changes than fostering either self- or job-growth mindset alone. Desirable changes to the job may require complementary changes to the self, and vice versa. For example, telephone salespeople who want to take on the new task of pitching at trade shows may need to improve their public speaking ability to make this change happen—and those who want to improve their public speaking ability may need to take on the task of pitching at trade shows in order to gain practice. Developing dual-growth mindset may encourage employees to pursue relatively substantial changes of this sort, in which both the self and job are changed in complementary ways. Without the self and job at play simultaneously, employees may struggle to see opportunities for substantial agentic change (Bandura, 2006, 2018).

More specifically, when employees increase in self-growth mindset but not job-growth mindset, they may be motivated to change their abilities and traits but see limited opportunity to leverage these changes within their (fixed) job designs. Continuing with the earlier example, salespeople may want to improve their public speaking skills but fail to increase the amount of public speaking in their job, limiting the change they ultimately experience in their work. Conversely, when employees increase in job-growth mindset but not self-growth mindset, they may see relatively little opportunity for change given their (fixed) abilities and traits. For instance, salespeople who are skilled at pitching one-on-one but not on stage may add more individual meetings at trade shows but not stretch to the task of pitching publicly. Employees may quickly habituate to the small changes they are able to make from adjusting only the self or job, resulting in little impact on enduring happiness. Moreover, gains in self- or job-growth mindset may only persist as long as employees can achieve corresponding changes on the job. As employees exhaust the small changes that can be made by fostering either self- or job-growth mindset alone, their mindsets may become more fixed, as they no longer see fresh opportunities for change. Increases in dual-growth mindset should be more sustainable, as employees should have more long-term opportunities to change their work lives in relatively substantial ways.

In sum, whereas fostering either self- or job-growth mindset alone may lead to smaller changes that are soon vanquished by the hedonic treadmill, fostering dual-growth mindset may spur relatively substantial agentic changes that are less susceptible to the hedonic treadmill. However, these relatively substantial changes may take time to translate into gains in happiness, as employees work their way through the learning curve to build skills and feel competent in their new challenges (Campion & McClelland, 1993; Reis et al., 2000). As such, we hypothesize that increases in dual-growth mindset yield gains in happiness that take time to develop, while increases in either self- or job-growth mindset alone do not yield lasting gains in happiness.

To test this hypothesis, we conducted two experimental studies: a quasi-experiment in a Fortune 500 technology company (Study 1), and a more controlled experiment with employees in a variety of organizations and jobs (Study 2). In both experiments, we assigned participants to one of three mindset interventions (self, job, or dual) and measured happiness before the intervention and at short- and long-term posttests. In Study 1, we measured happiness using ratings from participants’ managers and peers, testing whether

the mindset interventions produced gains in happiness that were observable by others. Although this helped to mitigate demand characteristics and social desirability biases, the subjective experience of happiness is also important. Thus, in Study 2, we measured self-reported happiness, as well as self- and job-growth mindset directly. In both experiments, we collected supplementary data on participants' job crafting intentions (Wrzesniewski & Dutton, 2001), which we used to test the plausibility of our theorizing that agentic change mediates the impact of dual-growth mindset on happiness.

Study 1

Method

We conducted a field quasi-experiment at a Fortune 500 technology firm headquartered in the western U.S. with several global offices. We used a quasi-experiment to facilitate causal inferences while affording fidelity to the context and phenomena of interest (Cook & Campbell, 1979). Employees participated in one of three mindset workshops: self, job, or dual. We collected observer ratings of happiness from participants' managers and peers at three time points: pretest, 6 weeks post, and 6 months post. We based the timing of the two posttests on prior research. Past studies have shown that interventions can influence happiness within 6 weeks (Lyubomirsky et al., 2005), and researchers have often used 6 months as a meaningfully long period of time for examining the effects of interventions, including in studies on mindset (e.g., Yeager et al., 2016), happiness (e.g., Kushlev et al., 2017; Stewart et al., 1997), and job redesign (e.g., Lawler et al., 1973; Orpen, 1979).

Transparency and Openness

We describe our sampling plan, all data exclusions (see Footnote 1), all manipulations, and all measures used in the study, and we adhered to the JAP methodological checklist. Analysis code is in the online supplement at <https://osf.io/ka7m3>. Data are unavailable given our privacy agreement with the participating firm. The analyses use the *mixed* command in Stata 15.1. This study was not preregistered, as it was run before the advent of preregistration. This study received Institutional Review Board (IRB) approval from the University of Pennsylvania (Protocol #812017 "Job Crafting") and Yale University (Protocol #1006006974 "Job Crafting").

Participants and Procedure

The sample consisted of 149 employees (65.10% female, age 23–51, $M_{\text{age}} = 30.32$, $SD_{\text{age}} = 4.30$), including 84 employees from the sales function and 65 from the general administrative function (human resources, financial, and legal services), who worked in the West Coast headquarters ($n = 67$) or one of two Western European offices ($n = 41$ and $n = 41$).¹ The employees' tenure at the firm ranged from 1 month to 7.5 years when their participation in the study began ($M_{\text{tenure}} = 2.63$ years, $SD_{\text{tenure}} = 1.55$ years).

To minimize validity threats (Cook & Campbell, 1979), we implemented several key design features. First, across conditions, all workshops were described as "career development" workshops. Participation was voluntary—each participant was offered multiple time slots and signed up based on availability. We randomly allocated each workshop to a condition on the day we conducted it after

employees arrived. This made it unlikely that selection threats would explain differences between conditions, as employees could not exercise choice about which condition to join. We conducted a total of 12 separate workshops across the three conditions: three for self ($n = 48$), five for job ($n = 62$), and four for dual ($n = 39$).²

Second, we used controlled treatments that were delivered by the same three authors, had the same duration (2 hr), and involved identical instructions and experiences except for the type of growth mindset fostered (self, job, or dual). We did not reveal that we were conducting multiple versions of the workshop. This reduced the likelihood that employees would become aware of the treatments in other conditions, mitigating implementation threats (Cook & Campbell, 1979). Third, by collecting three waves of data—one prior to the intervention and two afterward—our measures meet the criteria for a true longitudinal study (Ployhart & Vandenberg, 2010). This strengthens our ability to rule out statistical regression and maturation threats to internal validity (Shadish et al., 2002). Fourth, to mitigate the threat of demand characteristics, we collected independent ratings of happiness from managers and peers who were blind to the goals of the study and the treatment conditions.³

We designed the workshops based on mindset interventions used in past research (Heslin et al., 2005, 2006; Yeager et al., 2013). To maintain consistency across conditions, we created an instruction booklet for each condition to guide employees through the workshop. The three booklets (and any verbal instructions given) were identical except for the elements that participants were encouraged to see as flexible—the self, job, or both (dual). In the dual condition, the self and job were always mentioned together to maintain consistency with the self and job conditions. Participants completed steps to graphically represent their self and/or job as a flexible set of building blocks (see Berg et al., 2013). They first created a "before" diagram, and then an "after" diagram that depicted a more ideal (but still realistic) image of their self and/or job. Prior to completing their after diagrams, participants wrote responses to reflection questions about how their self and/or job has changed over time, were presented with compelling examples of employees changing themselves and/or their jobs, and completed a self-persuasion exercise (Aronson, 1999) in which they read a summary of scientific evidence that the focal elements (self, job, or both) were more flexible than people tend to think. In all conditions, participants' after diagrams included graphical elements to capture their self (abilities and traits) and job (tasks and relationships). What differed between conditions is the elements that were treated as flexible versus fixed. Participants were either encouraged to change their abilities/traits to better suit their tasks/relationships (self condition), change their tasks/relationships to better suit their abilities/traits (job condition), or change both their abilities/traits and tasks/relationships to better suit one another (dual condition). After finishing their diagrams, participants responded to open-ended questions regarding how they could take steps to enact their intended changes.

¹ Three additional employees completed a mindset workshop but were omitted from the study because we did not receive any observer ratings of happiness for them.

² Our goal was to have at least 35 participants in each condition. Power analysis using G*Power 3.1 suggests that 35 participants would provide 82% power to detect a medium within-participant effect (Cohen's $d = .50$).

³ Ten percent of raters were also participants in the study, meaning they were familiar with their own intervention but were still blind to the condition of the participants they rated. In our analyses, we controlled for whether raters were also participants.

Measures

Happiness. We measured happiness using observer ratings. This approach helped avoid demand characteristics and social desirability pressures on participants to report greater happiness following the workshop and relied upon the high level of self-other agreement found in ratings of happiness (Dobewall et al., 2013). We measured happiness using ratings from participants' managers and peers, who are privy to the happiness (or lack thereof) that participants express at work (Van Kleef, 2009; Weiss & Cropanzano, 1996). The firm identified each participant's manager, and participants nominated a peer or coworker with whom they interact regularly. At each time point, managers and peers rated participants' happiness with two items: "At work recently, to what extent has this person been [happy, satisfied]?" using a 7-point Likert-type scale (1 = *not at all* to 7 = *extremely*). These two items ("happy" and "satisfied") capture the common definition of happiness in the literature (Lyubomirsky et al., 2005, p. 115): "frequent positive affect, high life satisfaction, and infrequent negative affect," which "are highly correlated and typically yield a single factor after negative affect has been recoded." Although happiness is often measured with a single item focusing on how happy or satisfied individuals are overall (Abdel-Khalek, 2006; Kahneman, 1999), we used two items so that we could measure internal consistency while keeping the surveys as brief as possible, which was important given that raters had little incentive to complete the surveys. As displayed in Table 1, at all three time points, the two items showed strong internal consistency, and interrater reliability and agreement met conventional standards (LeBreton & Senter, 2008). We averaged the manager and peer ratings for our analyses—the mean from one rater was used when ratings were missing from the other rater. The majority of participants were evaluated by at least one rater at each of the time points: 97% at pretest ($n = 144$), 85% at 6 weeks ($n = 126$), and 72% at 6 months ($n = 108$). Participants were rated by both their manager and peer 52% of the time, only their manager 34% of the time, and only their peer 14% of the time (we control for this variation in our analyses).

Controls. At each time point, we created dummy variables for whether participants were rated by a rater who was a study participant (10% of raters), only a peer, or only a manager. We also created dummy variables that were static over time: job function, location, and workshop.

Job Crafting Intentions (Supplementary). We used the intervention materials to create a measure of job crafting intentions to use in supplementary mediation analyses. Our theorizing suggests that the effect of dual-growth mindset on happiness is mediated by agentic change that is volitional, self-concordant, effortful, and novel. Job crafting, wherein employees actively reshape their set of tasks and relationships to better suit themselves (Wrzesniewski & Dutton, 2001), captures agentic changes that are likely to fit these criteria. Indeed, job crafting has been linked to happiness/well-being (Slemp & Vella-Brodrick, 2014; Tims et al., 2013). Based on evidence that planned behaviors are robust predictors of actual behaviors (Ajzen, 1991; Armitage & Conner, 2001), including job crafting behaviors (Costantini et al., 2020), we measured the extent to which participants planned to exercise agency through job crafting after the workshop.

We collected the booklets that participants used to complete the intervention, scanned the relevant pages, and returned them to

participants so they could reference them after the workshop. In the job and dual conditions, participants created before and after diagrams comprised of "task blocks" to symbolize the activities and relationships associated with each task in their job. Participants could place tasks in three different block sizes (small, medium, large)—the larger the block, the more time and energy involved in the task and associated relationships. Participants could plan to craft more enriched jobs by allocating more time and energy to relatively desirable tasks and associated relationships. They could do so in two ways: increasing the size of an existing task block (that was already in their before diagram) or adding a new task block (that was not in their before diagram). We summed the intended job crafting moves that each participant planned, weighted by the physical size of the blocks: Small blocks were assigned the value of one, medium blocks two, and large blocks three. For example, if a participant moved a task from a small-to-large block, this would be an increase of two (from size one to three). If they added a new large block to their after diagram for a task that was not in their before diagram, this would be an increase of three. In the self condition, by design, participants could only plan changes to their abilities/traits, not their job tasks, and thus the self condition was excluded from the analyses on job crafting intentions. To facilitate interpretation, job crafting scores were standardized in our analyses. See the online supplement for further detail on this measure in both studies, including raw scores by condition.

Results

To account for nesting of observations within participant across the three time points, we used random-intercept models (Table 2; Raudenbush & Bryk, 2002). The models include a random intercept for participant and all other variables are fixed factors (except for job crafting intentions, which is continuous and is thus a fixed covariate). This approach allowed tests of within-participant changes in happiness from pretest between and within conditions. Figure 1 shows average within-participant change from pretest to 6 weeks and 6 months by condition.

Happiness

Consistent with our predictions, participants in the dual condition increased significantly more in happiness from pretest to 6 months than the self ($b = -.61, p = .021, d = -.56$) and job ($b = -.54, p = .047, d = -.50$) conditions (Model 1 in Table 2). These results remained significant with all controls included (Model 2). Next, to test change from pretest within each condition, we conducted planned contrasts using Model 1 in Table 2 (see means in Table 3). As expected, dual was the only condition to significantly increase in happiness from pretest to 6 months ($b = .63, p = .003, d = .58$). The job condition increased significantly in happiness from pretest to 6 weeks ($b = .32, p = .042, d = .29$), but this increase disappeared by 6 months ($p = .58$). No other changes from pretest were significant within each condition at 6 weeks or 6 months.⁴

⁴ As an exploratory test of whether mindsets affected performance, we also had managers and peers rate two items—how "effective" and "successful" participants had been at work recently ($\alpha = .94$, ICC[1, 2] range: .67–.79, AD_M range: .48–.54). Performance did not change significantly between time points in any condition, except that performance significantly increased from pretest to 6 weeks in the job condition ($p = .049$).

Table 1
Study 1: Correlations and Descriptive Statistics

Variable	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	
1. Observer-rated happiness (pretest)	(.91)																					
2. Observer-rated happiness (6 weeks)	.53	(.92)																				
3. Observer-rated happiness (6 months)	.36	.35	(.92)																			
4. Dual condition (vs. job/self)	.01	-.11	.21	—																		
5. Self condition (vs. dual/job)	.04	-.03	-.06	-.41	—																	
6. Job condition (vs. dual/self)	-.05	.12	-.13	-.50	-.58	—																
7. Job crafting intentions	.07	.19	.44	.31	n/a	-.31	—															
8. Rater also participant (yes vs. no)	-.03	-.06	.07	.00	.09	-.08	.01	—														
9. Rated by peer only (pretest)	-.02	-.11	.03	-.02	.01	.01	.00	.03	—													
10. Rated by peer only (6 weeks)	.17	.25	.09	.01	-.11	.09	.18	-.18	-.17	—												
11. Rated by peer only (6 months)	-.16	-.14	-.10	-.02	.09	-.06	-.22	.19	-.30	-.82	—											
12. Rated by manager only (pretest)	.03	-.15	-.03	.03	.07	-.09	-.10	-.03	.23	-.23	.13	—										
13. Rated by manager only (6 weeks)	.12	.17	.16	.05	-.04	-.01	.36	-.11	-.16	.44	-.35	-.25	—									
14. Rated by manager only (6 months)	-.11	-.05	-.12	-.10	.03	.06	-.17	.24	-.04	-.41	.44	-.33	-.55	—								
15. Rated by peer and manager (pretest)	-.08	-.10	-.15	-.06	.04	.02	.11	.02	.04	-.31	.27	.24	-.21	.06	—							
16. Rated by peer and manager (6 weeks)	-.15	.06	-.18	-.06	.04	.02	.02	.07	-.15	.27	-.21	-.14	.31	-.26	-.26	—						
17. Rated by peer and manager (6 months)	.12	.09	.29	.10	-.12	.03	-.14	.14	.07	-.27	.24	-.01	-.37	.50	-.30	-.38	—					
18. Job function (sales vs. other)	-.04	.00	.05	.40	-.32	-.05	.05	-.15	-.17	.00	.11	-.13	.07	-.05	.07	-.09	-.06	—				
19. Location A (vs. B/C)	-.17	-.15	-.01	.11	-.04	-.06	.11	-.13	-.16	-.10	.18	-.15	-.04	.12	.05	-.11	-.02	.54	—			
20. Location B (vs. A/C)	.07	.09	.04	.01	.06	-.06	.00	-.02	.10	.19	-.18	.02	.10	-.18	-.04	.17	-.09	.03	-.38	—		
21. Location C (vs. A/B)	.09	.05	-.03	-.11	-.02	.11	-.08	.14	.05	-.07	.00	.12	-.05	.06	-.01	-.05	.10	-.51	-.56	-.56	—	
Mean	4.58	4.67	4.77	2.6	.32	.42	7.26	.18	.06	.32	.58	.13	.30	.42	.17	.25	.31	.56	.28	.28	.45	—
Standard deviation	1.06	1.05	1.23	.44	.47	.49	4.60	.39	.24	.47	.49	.34	.46	.49	.37	.43	.46	.50	.45	.45	.50	—
Minimum	1.75	2.00	1.00	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Maximum	6.50	7.00	7.00	1	1	1	20	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Spearman-Brown reliability	.91	.91	.92																			
Intraclass correlation—ICC(1, 2)	.84	.82	.87																			
Average deviation from mean—AD _M	.54	.55	.38																			

Note. Correlations greater than .16 or less than -.16 were significant at $p < .05$ (except for correlations with job crafting intentions, which were significant at $p < .05$ when greater than .22 or less than -.22; the job crafting intentions measure excludes the self condition). See the online supplement for how variables were dummy coded in the analyses. Cronbach's alphas appear along the diagonal in parentheses. Cronbach's alphas and Spearman-Brown reliability capture internal consistency of the two-item scale. ICC(1, 2) captures both interrater reliability and agreement (LeBreton & Senter, 2008; Shrout & Fleiss, 1979). AD_M provides an additional measure of interrater agreement. For 7-point scales, AD_M values below 1.20 are considered acceptable interrater agreement (Burke & Dunlap, 2002). Given our privacy agreement with the firm, demographics (gender, age, and tenure) are excluded from this table. For brevity, the 12 workshop dummies are also excluded. Sample sizes by condition were as follows (includes all participants with happiness ratings from at least one rater at the time point): At pretest, dual ($n = 37$), self ($n = 46$), and job ($n = 61$). At the 6-week posttest, dual ($n = 32$), self ($n = 42$), and job ($n = 52$). At the 6-month posttest, dual ($n = 28$), self ($n = 33$), and job ($n = 47$). See the online supplement for means and standard deviations by condition at each time point for happiness, self-growth mindset, and job-growth mindset.

Table 2
Study 1: Random-Intercept Models

Dependent variable	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
	Happiness (observer-rated)	Happiness (observer-rated)	Job crafting intentions	Job crafting intentions	Happiness (observer-rated)	Happiness (observer-rated)
Condition						
Self (vs. dual)	.04 (.24)	.43 (.40)				
Job (vs. dual)	-.07 (.23)	.07 (.39)				
Time						
6 weeks (vs. pretest)	-.17 (.21)	-.10 (.21)				
6 months (vs. pretest)	.63** (.21)	.68** (.21)				
Condition x time						
Self (vs. dual), 6 weeks (vs. pretest)	.18 (.27)	.15 (.27)				
Self (vs. dual), 6 months (vs. pretest)	-.61* (.29)	-.59* (.29)				
Job (vs. dual), 6 weeks (vs. pretest)	.49 [†] (.26)	.43 [†] (.26)				
Job (vs. dual), 6 months (vs. pretest)	-.54* (.27)	-.54* (.27)				
Job crafting intentions						
Job crafting intentions x time						
Job crafting intentions, 6 weeks (vs. pretest)						
Job crafting intentions, 6 months (vs. pretest) ^a						
Controls						
Rater also participant (yes vs. no)						
Rated by peer only (vs. peer and manager)						
Rated by manager only (vs. peer and manager)						
Job function (sales vs. other)						
Location dummies						
Workshop dummies						
Fixed intercept	4.59*** (.18)	4.13*** (.46)	.46* (.20)	1.18* (.50)	4.82*** (.21)	4.04*** (.43)
Random intercept (participant)	.49*** (.09)	.42*** (.09)	n/a n/a	n/a n/a	.30*** (.10)	.22*** (.09)
Log-likelihood	-542.74	-533.92	-99.74	-98.50	-254.28	-249.89

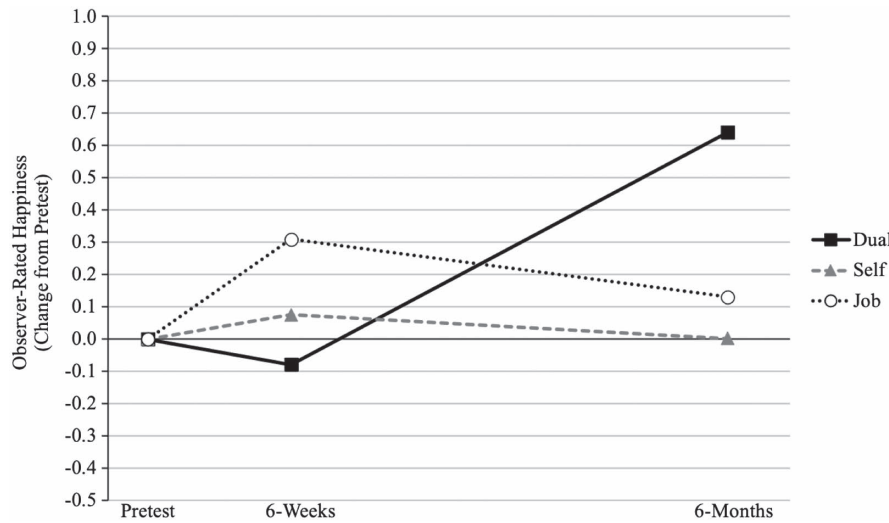
Note. Standard errors are in parentheses. Models 3 and 4 have one observation per participant and thus do not include a random intercept. Job crafting intentions is standardized (Models 3-6).

^a These coefficients were used for the supplementary mediation analyses. These coefficients test whether job crafting intentions predicted within-participant changes in happiness from pretest to 6 months.

^b The workshop dummies were excluded from Models 4 and 6 because they were largely redundant with the location dummies, given the focus on two of the three conditions in these models.

[†] $p < .10$. * $p < .05$. ** $p < .01$. *** $p < .001$.

Figure 1
Study 1: Change in Happiness by Condition (Estimates From Model 1 in Table 2)



Job Crafting Intentions (Supplementary Mediation Analyses)

We used Models 3–6 in Table 2 to test whether job crafting intentions mediated the effect of the dual condition on gains in happiness from pretest to 6 months. Only the dual and job conditions were included in these analyses, as the self condition by design did not vary in job crafting intentions. To construct 95% confidence intervals testing the indirect effects, we used the Monte Carlo method recommended for multilevel mediation (Bauer et al., 2006). The confidence interval was significant (excluded zero) for job crafting intentions mediating the effect of the dual condition on 6-month gains in happiness as compared to the job condition without $[-.56, -.01]$ and with controls $[-.62, -.02]$. These results support the plausibility of agentic change—job crafting in particular—mediating the impact of dual-growth mindset on happiness.

Study 2

Method

We designed Study 2 to build on these results in six ways. First, whereas Study 1 was a quasi-experiment with arbitrary assignment, Study 2 is a more controlled experiment with true random assignment. Second, to complement the Study 1 results on observer-rated happiness, Study 2 focuses on self-reports of happiness. Third, whereas Study 1 participants worked in the same organization (many in the same job), Study 2 participants are from a variety of organizations and jobs. Fourth, to mitigate the threat of biases that may have arisen in Study 1 from the researchers delivering the workshops, participants in Study 2 completed web-based versions of the mindset workshops—instructions were delivered entirely via standardized text online. Fifth, to enable manipulation checks, Study 2 includes measures of self- and job-growth mindsets over time. Sixth, it is plausible that the self or job condition could have

Table 3

Study 1: Estimated Marginal Means and Planned Contrasts From Pretest (Based on Model 1 in Table 2)

Condition	Time		
	Pretest	6 weeks post	6 months post ^a
Dependent variable = happiness (observer-rated)			
Dual	4.59 (.18)	4.42 (.19), $d = -.16$	5.22** (.20), $d = .58$
Self	4.63 (.16)	4.64 (.17), $d = .01$	4.64 (.18), $d = .01$
Job	4.52 (.14)	4.84* (.15), $d = .29$	4.61 (.15), $d = .08$

Note. Standard errors are in parentheses. For posttests, Cohen's d effect sizes reflect changes from pretest and significant changes are flagged. After Bonferroni corrections for multiple comparisons, the 6-week result for job became marginal ($p = .085$) while the 6-month result for dual remained significant ($p = .007$).

^a Comparing the three conditions at 6 months (in absolute terms, not changes from pretest), dual finished significantly higher than self ($p = .032$, $d = .53$) and job ($p = .016$, $d = .56$), while self and job did not significantly differ.

[†] $p < .10$. * $p < .05$. ** $p < .01$. *** $p < .001$.

boosted happiness faster than 6 weeks and the effects were already declining by 6 weeks (which could explain why job had a significant short-term boost but self did not). To address this possibility while maintaining comparability with the timeframe in Study 1, we moved the short-term posttest from 6 to 4 weeks. Thus, participants completed measures of happiness and the two mindsets (self and job) at three time points: pretest, 4 weeks post, and 6 months post.

Transparency and Openness

We describe our sampling plan, all data exclusions (see Footnote 5), all manipulations, and all measures used in the study, and we adhered to the *JAP* methodological checklist. Data and code for the analyses are at <https://osf.io/ka7m3>. The analyses use the *mixed* command in Stata 15.1. This study was not preregistered, as it was run before the advent of preregistration. This study received IRB approval from the University of Pennsylvania (Protocol #812017 “Job Crafting”) and Yale University (Protocol #1006006974 “Job Crafting”).

Participants and Procedure

Participants included 398 full-time employees in the U.S. (47.00% female, age 18–68, $M_{\text{age}} = 32.00$, $SD_{\text{age}} = 9.79$).⁵ They were recruited through Amazon MTurk and compensated \$4.00 for completing the pretest survey/intervention, plus \$2.00 for each posttest survey completed. We used screening questions to ensure that participants were full-time employees in organizations. Participants averaged 12.50 years of work experience ($SD = 8.95$).

All participants first completed the pretest survey, which included measures of happiness and self- and job-growth mindsets. Then, participants were randomly assigned to online versions of the three mindset workshops from Study 1: self ($n = 130$), job ($n = 134$), and dual ($n = 134$). The procedures were virtually the same as the in-person workshops, except without human facilitators. Of the 398 participants who completed the pretest survey/intervention, 221 (55.53%) completed the 4-week posttest and 165 (41.46%) completed the 6-month posttest (only participants who completed the 4-week posttest were invited to complete the 6-month posttest). Our analyses include all available data/participants at each time point—the results are generally consistent when only the 165 participants with complete data (all three time points) are included in the analyses. Participants who missed the posttests did not significantly differ from those who completed the posttests in terms of pretest happiness, or either self- or job-growth mindset, within or across conditions.

Measures

Happiness. To complement the observer ratings in Study 1, Study 2 measured subjective happiness with six self-reported items adapted from measures used in prior research (Berkman, 1971; Hills & Argyle, 2002; Joseph et al., 2004). This provided a more comprehensive measure of the aforementioned definition of happiness (Lyubomirsky et al., 2005) than the two broad items in Study 1 (which mirror the first two items here⁶): “At work, I’ve recently felt [happy, satisfied, cheerful, proud, interested, calm].” Participants rated these items at all three time points using a Likert-type scale (1 = *strongly disagree* to 7 = *strongly agree*). The scale showed strong internal consistency at all time points—see Table 4.

Self- and Job-Growth Mindset. To capture the two mindsets, we adapted items that were developed to assess flexible versus fixed mindsets (Levy et al., 1998). We measured self-growth mindset with six items (e.g., “I can change my core abilities”) and job-growth mindset with six items (e.g., “I believe that I can change the attributes of my job”). See the online supplement for a complete list of items, validation of the scales using a separate sample of employees, and evidence of measurement invariance over time. Both mindset scales showed good internal consistency at all three time points (see Table 4).

Using the pretest data, confirmatory factor analysis on the three scales (happiness and the two mindsets) showed that a three-factor solution with correlated factors fit the data appropriately, $\chi^2(132) = 310.17$, $p < .001$, CFI = .95, TLI = .94, RMSEA = .058, SRMR = .053, significantly better than a two-factor solution with the two mindsets as one factor and happiness as the other, $\chi^2(134) = 1,240.20$, $p < .001$, CFI = .68, TLI = .63, RMSEA = .14, SRMR = .14, $\chi^2(2) = 930.03$, $p < .001$, and significantly better than a one-factor solution, $\chi^2(135) = 1,866.54$, $p < .001$, CFI = .50, TLI = .43, RMSEA = .18, SRMR = .17, $\chi^2(3) = 1,556.37$, $p < .001$.

Job Crafting Intentions (Supplementary). We used the same approach as in Study 1.

Results

Like Study 1, we used the random-intercept models in Table 5 for all Study 2 analyses. The models include a random intercept for participant to account for nesting within participant.⁷

Self- and Job-Growth Mindset (Manipulation Checks)

Models 1 and 2 in Table 5 test the effectiveness of our manipulations. As expected, from pretest to 4 weeks, the dual condition increased in both self- and job-growth mindset, the self condition increased in only self-growth mindset (not job-growth), and the job condition increased in only job-growth mindset (not self-growth). These results suggest that our manipulations performed as intended. By 6 months, the dual condition sustained its increases in both mindsets, while the self and job conditions returned to approximately pretest levels (see Table 6 and Figure 2). This is consistent with our theorizing, which posits that increases in dual-growth mindset are more sustainable than increases in only self- or job-growth mindset.⁸

⁵ To exceed the statistical power of Study 1, our goal for Study 2 was to have at least 50 participants with complete data (all three time points) in each condition. We expected substantial attrition over time and thus set the target of 400 participants to complete the pretest/intervention survey. A total of 414 respondents completed the survey but 16 were omitted because they entered meaningless content so the survey would advance.

⁶ Results for Study 2 were largely consistent when happiness was measured with only the two items used in Study 1 (“happy” and “satisfied”)—see the online supplement for full results.

⁷ At pretest, the three conditions did not significantly differ in happiness or self-growth mindset, but by chance, the self condition scored significantly higher in job-growth mindset than the dual and job conditions, $F(2,395) = 4.97$, $p = .007$. Testing the hypotheses in two studies helps mitigate this limitation to some extent, as does the analytic approach used in both studies, which focuses on whether within-participant changes from pretest differ by condition.

⁸ Although self- and job-growth mindset were both positively correlated with happiness at all three time points (see Table 4), neither mindset mediated changes in happiness from pretest to 4 weeks or 6 months.

Table 4
Study 2: Correlations and Descriptive Statistics

Variable	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
1. Self-rated happiness (pretest)	(.89)															
2. Self-rated happiness (4 weeks)	.62	(.91)														
3. Self-rated happiness (6 months)	.63	.66	(.92)													
4. Self-growth mindset (pretest)	.20	.22	.31	(.82)												
5. Self-growth mindset (4 weeks)	.30	.29	.29	.78	(.89)											
6. Self-growth mindset (6 months)	.30	.25	.32	.70	.79	(.84)										
7. Job-growth mindset (pretest)	.36	.27	.26	.18	.19	.12	(.83)									
8. Job-growth mindset (4 weeks)	.36	.45	.40	.32	.39	.31	.63	(.87)								
9. Job-growth mindset (6 months)	.34	.46	.56	.26	.31	.31	.59	.71	(.88)							
10. Dual condition (vs. job/self)	.03	.14	.29	.01	.13	.22	-.07	-.02	.08							
11. Self condition (vs. dual/job)	-.04	-.04	-.11	-.04	-.02	-.11	.16	-.02	.07							
12. Job condition (vs. dual/self)	.01	-.10	-.18	.03	-.11	-.11	-.08	.04	-.15	-.50						
13. Job crafting intentions	.03	.08	.34	-.02	.05	.17	.12	.11	.25	.25	n/a					
14. Gender (female vs. male)	.11	.06	.13	-.09	.00	.07	.08	.15	.17	.06	-.08	-.25				
15. Age	.05	.16	.07	.02	.03	.12	.08	.09	.10	.01	.08	.01	.20			
16. Work experience	.03	.16	.11	.08	.07	.15	.09	.14	.16	-.01	.08	-.06	.12			
Mean	4.99	5.08	5.05	4.84	5.00	4.89	4.63	4.90	4.70	0.34	0.33	0.34	4.45	0.47	0.07	.84
Standard deviation	1.13	1.21	1.24	1.01	1.14	1.12	1.02	1.02	1.16	0.47	0.47	0.47	3.98	0.5	0	9.79
Minimum	1.33	1.00	1.00	2.00	1.33	2.17	1.00	1.83	1.00	0	0	0	0	0	18	2
Maximum	7.00	7.00	7.00	7.00	7.00	7.00	7.00	7.00	7.00	1	1	1	25	1	68	52

Note. Correlations greater than .16 or less than -.16 were significant at $p < .05$. Cronbach's alphas appear in parentheses along the diagonal. See the online supplement for how variables were dummy-coded in the analyses. Sample sizes by condition were as follows: At pretest, dual ($n = 134$), self ($n = 130$), and job ($n = 134$). At the 4-week posttest, dual ($n = 77$), self ($n = 68$), and job ($n = 76$). At the 6-month posttest, dual ($n = 55$), self ($n = 56$), and job ($n = 54$). See the online supplement for means and standard deviations by condition at each time point for happiness, self-growth mindset, and job-growth mindset.

Table 5
Study 2: Random-Intercept Models

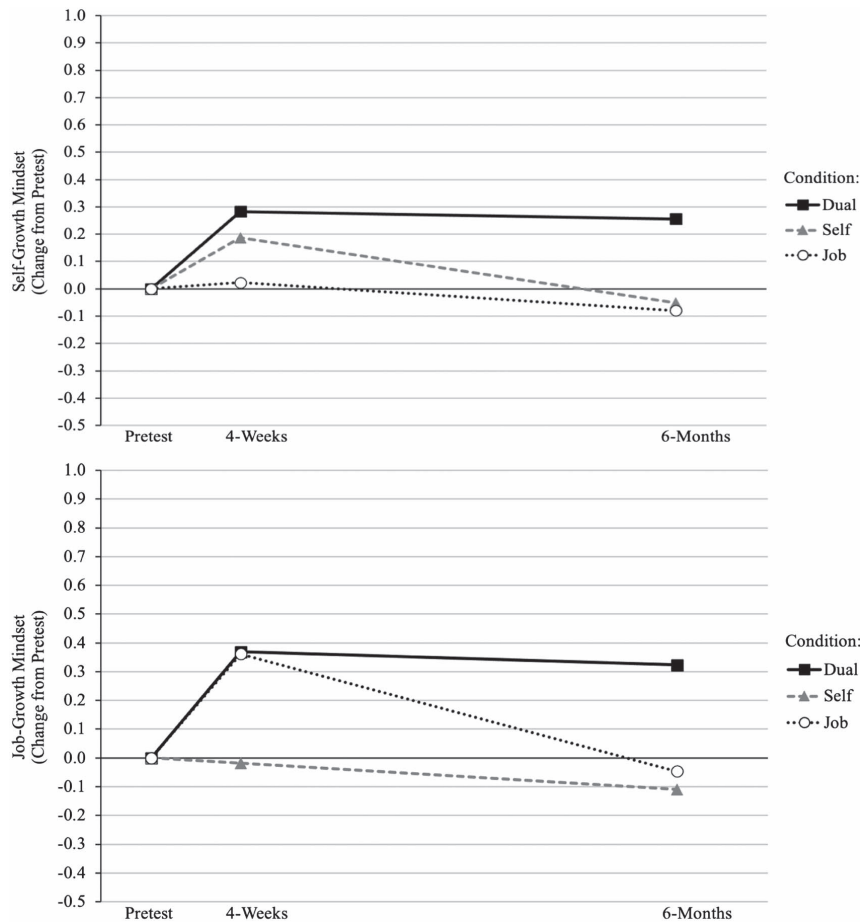
Dependent variable	Model 1		Model 2		Model 3		Model 4		Model 5	
	Self-growth mindset	Job-growth mindset	Job-growth mindset	Happiness (self-rated)	Job crafting intentions	Happiness (self-rated)	Job crafting intentions	Happiness (self-rated)		
Condition										
Self (vs. dual)	-.07 (.13)	.34** (.13)		-.10 (.14)						
Job (vs. dual)	.02 (.13)	-.01 (.13)		-.03 (.14)						
Time										
4 weeks (vs. pretest)	.28*** (.08)	.37*** (.10)		.19† (.11)						.19† (.11)
6 months (vs. pretest)	.26*** (.09)	.32*** (.11)		.40*** (.13)						.34*** (.13)
Condition × time										
Self (vs. dual), 4 weeks (vs. pretest)	-.10 (.12)	-.39*** (.14)		-.08 (.16)						
Self (vs. dual), 6 months (vs. pretest)	-.31* (.13)	-.43*** (.16)		-.42* (.18)						
Job (vs. dual), 4 weeks (vs. pretest)	-.26* (.12)	-.01 (.14)		-.26† (.16)						
Job (vs. dual), 6 months (vs. pretest)	-.34* (.13)	-.37* (.16)		-.57*** (.18)						
Job crafting intentions										
Job crafting intentions × time										
Job crafting intentions, 6 weeks (vs. pretest)										.01 (.08)
Job crafting intentions, 6 months (vs. pretest) ^a										.22* (.09)
Fixed intercept	4.85*** (.09)	4.52*** (.09)		5.03*** (.10)						5.03*** (.10)
Random intercept (participant)	.86*** (.07)	.70*** (.07)		.84*** (.08)						.79*** (.10)
Log-likelihood	-.977.95	-1.028.80		-1.120.46						-750.09

Note. Standard errors are in parentheses. Model 4 has one observation per participant and thus does not include a random intercept. Job crafting intentions is standardized (Models 4 and 5).

^aThis coefficient was used for the supplementary mediation analyses. This coefficient tests whether job crafting intentions predicted within-participant changes in happiness from pretest to 6 months.

† $p < .10$. * $p < .05$. ** $p < .01$. *** $p < .001$.

Figure 2
 Study 2: Change in Self- and Job-Growth Mindset by Condition (Estimates From Models 1 and 2 in Table 5)



Happiness

As predicted, participants in the dual condition increased significantly more in happiness from pretest to 6 months than the self ($b = -.42, p = .017, d = -.36$) and job ($b = -.57, p = .001, d = -.49$) conditions—see Model 3 in Table 5 (and Figure 3 for a visual). Planned contrasts showed that the dual condition increased in happiness marginally from pretest to 4 weeks ($b = .19, p = .089, d = .16$) and then significantly by 6 months ($b = .40, p = .002, d = .34$). No changes in happiness from pretest were significant within the self or job conditions (see Table 6). These results offer additional support for our main predictions.

Job Crafting Intentions (Supplementary Mediation Analyses)

We used Models 4 and 5 in Table 5 to test whether job crafting intentions mediated the effect of the dual condition on happiness. Like Study 1, we used Monte Carlo procedures to test the indirect effect. The confidence interval was significant for job crafting intentions mediating the effect of the dual condition on 6-month gains in happiness as compared to the job condition $[-.22, -.02]$.

This provides additional evidence for the plausibility of agentic change (in the form of job crafting) mediating the impact of dual-growth mindset on happiness.

General Discussion

Theoretical Implications

The present research introduces dual-growth mindset as a potentially powerful—but previously overlooked—driver of employee happiness in organizations. Scholars have increasingly documented a shift in focus from top-down, manager-driven changes to the bottom-up initiative that employees can take to shape their own happiness at work (Grant & Ashford, 2008; Parker et al., 2010). However, this body of research on agentic and proactive behaviors has largely overlooked mindsets as a central engine of change. Our research introduces mindsets about the job as an important complement to mindsets about the self. We find that fostering dual-growth mindset yielded gains in happiness 6 months later, while fostering either self- or job-growth mindset alone did not yield lasting gains in happiness—supplementary analyses suggest that these effects are mediated by job crafting intentions. Furthermore, we find that increases in dual-growth mindset

Table 6

Study 2: Estimated Marginal Means and Planned Contrasts From Pretest (Based on Models 1–3 in Table 5)

Condition	Time		
	Pretest	4 weeks post	6 months post ^a
Dependent variable = self-growth mindset			
Dual	4.85 (.09)	5.14*** (.11), <i>d</i> = .27	5.11** (.12), <i>d</i> = .25
Self	4.79 (.09)	4.97* (.11), <i>d</i> = .17	4.73 (.12), <i>d</i> = −.06
Job	4.88 (.09)	4.90 (.11), <i>d</i> = .02	4.80 (.12), <i>d</i> = −.08
Dependent variable = job-growth mindset			
Dual	4.52 (.09)	4.89*** (.11), <i>d</i> = .36	4.84** (.12), <i>d</i> = .31
Self	4.86 (.09)	4.84 (.11), <i>d</i> = −.02	4.75 (.12), <i>d</i> = −.09
Job	4.51 (.09)	4.87*** (.11), <i>d</i> = .35	4.46 (.12), <i>d</i> = −.05
Dependent variable = happiness (self-rated)			
Dual	5.03 (.10)	5.22 [†] (.12), <i>d</i> = .16	5.43** (.13), <i>d</i> = .34
Self	4.93 (.10)	5.04 (.13), <i>d</i> = .09	4.91 (.14), <i>d</i> = −.02
Job	5.00 (.10)	4.93 (.12), <i>d</i> = −.06	4.83 (.14), <i>d</i> = −.15

Note. Standard errors are in parentheses. For posttests, Cohen’s *d* effect sizes reflect changes from pretest and significant changes are flagged. After Bonferroni corrections for multiple comparisons, all significant results remained significant, except the 4-week increase in self-growth mindset in the self condition became marginal ($p = .065$).

^a Comparing the three conditions at 6 months (in absolute terms, not changes from pretest), dual finished significantly higher in self-growth mindset than self ($p = .021$, $d = .36$) and marginally higher than job ($p = .055$, $d = .29$), while self and job did not significantly differ. In job-growth mindset, dual finished significantly higher than job ($p = .026$, $d = .37$), but not self ($p = .57$, $d = .09$), and self-finished marginally higher than job ($p = .097$, $d = .27$), presumably due to self-starting higher at pretest. In happiness, dual finished significantly higher than self ($p = .006$, $d = .46$) and job ($p = .002$, $d = .52$), while self and job did not significantly differ.

[†] $p < .10$. * $p < .05$. ** $p < .01$. *** $p < .001$.

are more sustainable than increases in either self- or job-growth mindset in isolation. These results uncover valuable theoretical insights on dual-growth mindset as a bottom-up vehicle for change—and ultimately greater happiness—in organizations.

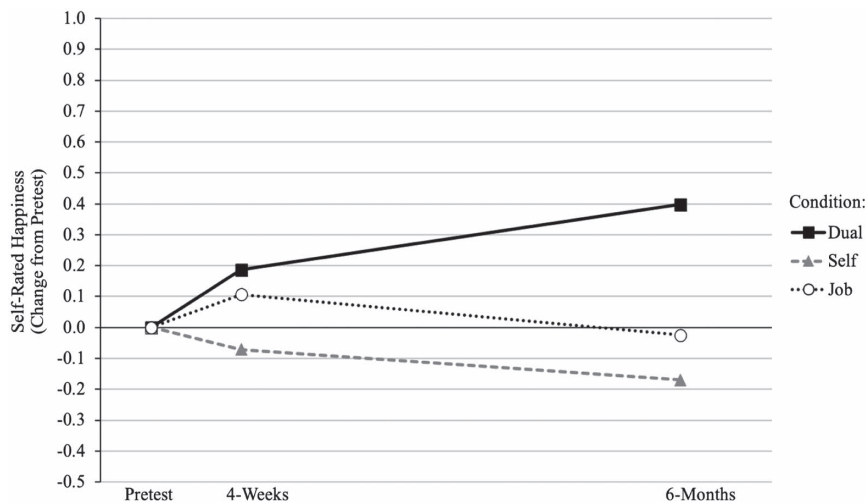
First, our research may help to explain the limited effects of self-growth mindsets that have been found in recent research and meta-analyses (Burgoyne et al., 2020; Sisk et al., 2018). In two meta-analyses, self-growth mindset had weak effects overall, which varied by sample and context (Sisk et al., 2018). Our work suggests that to achieve sustainable gains, it may be important for individuals to believe they can change their environments as well as themselves.

Second, our work opens up new possibilities for research on person–environment fit (Kristof-Brown et al., 2005), which tends to treat the person and environment as fixed entities. Our studies hint that seeing both entities as flexible may help employees create a more optimal fit. Bringing the concept of dual-growth mindset to research on person–environment fit may offer a more dynamic perspective on how person–environment fit evolves (or not) over time.

Third, our research offers a new explanation for why job redesign interventions often fail to achieve longer term gains (e.g., Frank & Hackman, 1975; Griffin, 1991). Typically, when jobs are enriched, employees are given a new set of fixed tasks and

Figure 3

Study 2: Change in Happiness by Condition (Estimates From Model 3 in Table 5)



trained in the skills necessary to accomplish those tasks (Campion & McClelland, 1993). If job redesign is complemented with dual-growth mindsets, employees may be better equipped to customize their redesigned jobs and accrue greater benefits over time. Similarly, dual-growth mindsets may help advance research on how employees and organizations can leverage the benefits of customized work arrangements, which are increasingly prevalent and important (Liao et al., 2016; Rousseau et al., 2006). Fostering dual-growth mindsets may help employees recognize opportunities for more optimal customized work arrangements. However, more research is needed to understand whether and when this is the case, as fostering dual-growth mindsets before employees customize their work arrangements could backfire by compelling them to take on more demands than they can handle.

Limitations and Future Directions

These studies have key limitations that can be addressed in future research. First, we focused on happiness (and job crafting intentions as one likely mediator), but dual-growth mindset may have many additional important consequences. Future research could explore the link between dual-growth mindset and other potential outcomes, such as job performance or organizational citizenship behavior (Organ, 1988), and other potential mediators, such as personal initiative (Frese & Fay, 2001), self-efficacy (Bandura, 1982), or curiosity (Harrison, 2011). Although we did not find sustained effects for job performance (see Footnote 4), dual-growth mindset may boost performance when the intervention is aimed more specifically at this goal. Second, we measured job crafting intentions, as opposed to job crafting behaviors. This approach avoided potential biases from surveying participants over time about the changes they have made, which could influence their behavior and thus undermine inferences about the effects of the initial intervention. Nonetheless, participants' actual behavior may have differed from their intentions (cf. Ajzen, 1991). Third, our measure of job crafting intentions was relatively effective at comparing the dual and job conditions, but by design, the intervention did not allow participants in the self condition to convey job crafting intentions. As a result, our measure may have missed job crafting that occurred in the self condition. Future research could use more granular behavioral measures of job crafting and other potential mediators.

Fourth, to control how often change was encouraged across conditions, the self and job were always mentioned together in the dual condition, which may be a key boundary condition. Future experiments could address this by manipulating one entity at a time in the dual condition (self then job or vice versa)—the self and job conditions could be delivered in two equivalent parts that both cover the same focal entity (self or job). Fifth, because happiness and the two mindsets were measured at only three time points, we could not test what may have occurred between measurements and after the 6-month posttest. Future studies could use more frequent measurements over a longer duration. Sixth, the job condition yielded a temporary short-term boost in happiness in Study 1 but not in Study 2. This may be due to differences between the two studies in organizational context, the timing of the initial posttest (6 vs. 4 weeks), or in how happiness was measured (observer- vs. self-rated). Future studies could explore moderators of the effect of job-growth mindset on happiness. Lastly, because our two experiments lacked a pure control group, we could not fully test the

statistical interaction of self- and job-growth mindsets, although results across both studies were suggestive of a multiplicative relationship. This can be addressed in future experiments that include a pure control condition.

Practical Implications and Conclusion

Employees have growing levels of autonomy to manage their own tasks and interactions, set their own work schedules, and develop their own skills (Grant & Ashford, 2008; Griffin et al., 2007). Our research suggests that fostering dual-growth mindset may help employees harness this autonomy, and in turn experience gains in happiness. Fostering either self- or job-growth mindset in isolation may be less effortful—but also potentially less rewarding—than fostering dual-growth mindset. Employees and managers may need to be selective about when they foster dual-growth mindset, as they must weigh whether the potential for eventual gains in happiness is worth the additional investment. Nonetheless, our studies suggest that dual-growth mindset may be a powerful way for employees to improve their own experiences of work.

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