The Implicit Meaning of (My) Change

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The concept of change simply entails the totality of ways in which a particular entity has grown better and grown worse. Five studies suggest that this is not how people actually understand it for themselves. Rather, when asked to assess how they have "changed" over time, people bring to mind only how they have *improved* and neglect other trajectories (e.g., decline) that they have also experienced; global change is specifically translated as directional change for the better. This tendency emerged across many populations, time frames, measures, and methodologies (Studies 1–3), and led to important downstream effects: people who reflected on "change" from their pasts experienced enhanced mood, meaning, and satisfaction in their presents, precisely because they had assumed to only think about personal improvement (Study 4). A final study shed light on mechanisms: people evaluated the word change in a speeded response task as more positive when they were instructed to interpret the word in relation to themselves versus a friend, while no differences emerged between conditions for nonchange control words (Study 5). This suggests that the basic pattern across studies stems (at least partly) from traditional self-enhancement motives—our own change spontaneously brings to mind only the ways in which we have improved, whereas change in someone else is not so immediately and uniformly associated with improvement. Taken together, these findings reveal novel insights into the content and consequences of change perception, and they more broadly highlight unforeseen biases in when and why people might subjectively (mis)interpret otherwise objective constructs.

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How have you changed over the past year? On the surface, answering this question involves a straightforward process. People need only bring to mind those things about themselves that have gotten better and other things that have gotten worse, and then use this assortment of evidence to assess their standing. In turn, these answers can guide many consequential decisions, from which partners we date, politicians we vote for, and policies we support; to when we start or stop a diet; to whether we spend now or later; and they more generally dictate how we feel about our lives. Questions of change matter.

Alas, the *actual* process of how we answer such questions is not so simple. People misperceive how they have changed over time because they do not bring to mind the full range of evidence. Many otherwise diagnostic experiences are either not registered as so at the time or simply forgotten later, because of basic limits of

attention and storage (Pashler, 1988; Rensink, 2002). People are thus left to piece together their movement through time from prototypical theories about how things generally go (e.g., "I've now graduated, so I must be smarter"), irrespective of real trajectories (e.g., if past and present testing scores do not differ: Eibach, Libby, & Gilovich, 2003; Kahneman & Riis, 2005; Ross, 1989).

This process, however, may also involve a deeper bias, not merely in terms of how people resolve cognitive constraints but in how they interpret the concept of change altogether. From an objective perspective, "change" lacks directional value; when asked how they have changed over time, people should be equivalently prompted to search for positive changes (e.g., "I've gained intelligence") as well as negative changes (e.g., "I've gained weight"), and then form their overall assessment. Nothing about the concept itself suggests we do differently. And yet, the very nature of the question may evoke something else *subjectively*. Most people believe their lives are constantly progressing, that they have learned and grown from previous experiences (Markus & Ruvolo, 1989; McAdams, 2008; Quoidbach, Gilbert, & Wilson, 2013) and that this "core self" who they are ever-approaching is inherently positive (Johnson, Robinson, & Mitchell, 2004; Judge, Locke, & Durham, 1997). Likewise, people tend to feel uncomfortable at the prospect that their life-course over time might be growing stagnant, failing, or meaningless, triggering the self-enhancing belief that bad past experiences must have ultimately been good for the present (Beauregard & Dunning, 1998; Chambers, Windschitl, & Suls, 2003; Dunning, 1995; Sedikides, Gaertner, & Toguchi, 2003). Motives to perceive improvement are so pervasive that people actively denigrate earlier selves to appear developed and judge past traits, talents, and tendencies as sizably worse than how they originally had judged themselves at those

previous points in time (Fleeson & Heckhausen, 1997; Ryff, 1991; Wilson & Ross, 2001), and then conveniently ignore feedback to the contrary (Green & Sedikides, 2004; O'Brien, 2013; Sedikides & Green, 2000). In short, good things about the present self are selectively conceived, prioritized, and embraced.

Because the personal past elicits these many negative connotations in the service of a positive present, reflecting on change *from it* may not evoke change of any kind (as it should to facilitate informed decisions), but change in one distinct direction. In other words, our search for answers to questions of change may not merely be limited in size but biased in scope: because people tend to think about their personal trajectories over time in a uniformly positive light—perceiving perennial growth for the better—even the mere concept of change may bring to mind only evidence of one's improvement but fail to cue people toward evidence of their decline. That is, when asking others about how they have "changed" in general, they may assume to search only for positive examples of improvement to answer the question, even though the objective concept entails other fluctuations as well. This selective thought process may be so pervasive that simply reflecting on how one has changed from past to present might lead people to naturally interpret the task and react to it in a patently positive way, in the absence of any explicit instructions or cues to translate global change as "change *for the better*." While most of us inevitably experience both ups *and* downs throughout our lives, trying to assess this global change over time could be inherently (and inadvertently) shaped by the task itself.

Five studies with over 1,600 participants explored this possibility. First, we used a correlational design to establish the extent to which perceptions of global change derive from specific perceptions of directional change (Study 1). Next, we extended these results causally to test whether inducing people to think about general change *actually* brings to mind uniformly positive experiences (Studies 2–3). We then examined consequences: reflecting on change from the past may serve as a novel strategy for enhancing wellbeing in the present, precisely because people only assume to reflect on improvement (Study 4). Finally, we sought to find evidence for the motivated component that we hypothesize as (at least partly) underlying these effects. People typically engage in self-enhancement as a means to enhance mood, maintain self-esteem, and impel behavior (Kunda, 1990; Sharot, 2012; Taylor & Brown, 1988). Accordingly, research has shown that people view their own lives as constantly improving but do not necessarily construe other people's lives in kind (see Chambers & Windschitl, 2004 for a review). Hence, people should be more inclined to interpret the word change as uniformly positive in relation to their *personal* trajectory over time, versus when they think about change in someone else (Study 5).

Lastly, it is worth noting up front that a variety of alternate mechanisms beyond self-improvement motives may lend further credence to our hypothesis. We believe these mechanisms likely work in concert to amplify the potential bias as it might exist in daily life, and each might exert varying levels of influence depending on the situation at hand. Nonetheless, in the current paper we sought to isolate the specific influence of self-improvement motives in contributing to the effect by ruling out some of these additional sources as best as possible. One rather mundane possibility is a tacit inference about conversational norms (Grice, 1975); if a person is asked how he or she

has changed without additional context, the person may simply assume the question is asking about improvement because it would be odd or impolite to probe otherwise (or, likewise, they may simply opt to conceal their decline). We attempted to rule out this possibility by using carefully designed and explicitly worded prompts, that clearly allow for people to discuss their decline in a private, comfortable, and normative way. We also used varied methodologies across studies beyond simply asking a direct question about change. Another, more psychologically interesting, possibility is that episodes of improvement are more richly represented in memory than episodes of decline. If people recount only the ways they have improved in response to a question about global change, this could simply reflect some *objective* feature of improvement (e.g., improvement is typically more planned than decline) that makes those episodes truly more salient. This seems unlikely given a large literature suggesting the opposite—that negative events are more salient and memorable than positive events (see Baumeister, Bratslavsky, Finkenauer, & Vohs, 2001 for a review)—which, if anything, works against our hypothesis. Still, we sought to account for this possibility by using a wide variety of designs and measures that together cannot be because of possible objective differences alone. Moreover, our self/other study (Study 5) more explicitly tests this account, as *others*' improvement should also more readily come to mind if there is something objectively special about such episodes.

Study 1

Change, for the Better

In Study 1, participants evaluated how much they had changed over time, in terms of global change as well in distinct positive, negative, and neutral ways. We investigated a diversity of time frames (i.e., change over the past 1, 5, or 10 years) and recruited from numerous populations (i.e., from both student and community samples) to help establish generalizable insight into the links between these various perceptions of change.

Method

In all studies, we predetermined samples of roughly 100 participants per condition as a rule of thumb, fully double the recommendation for when effect sizes are unknown (Simmons, 2014). This also far exceeds past research with similar designs (e.g., about 10 per cell in Dunning, 1995; about 30 per cell in Chambers et al., 2003; Eibach et al., 2003; Wilson & Ross, 2001; about 50 per cell in Cone & Ferguson, 2015). Power analyses were also conducted using G*Power 3 (Faul, Erdfelder, Lang, & Buchner, 2007). Samples of this size provide 80% power to detect $d = .40$ (two-tailed, $\alpha = .05$), marking a small-to-medium effect (Cohen, 1992). In actuality, the average observed size of the critical effect (i.e., the effect of change prompts and manipulations on improvement-related variables, across studies) was $d = .61$, marking a medium-to-large effect and achieving 99% power given the sample size. Hence, across various benchmarks, our studies appear to be amply powered. All manipulations and measures are reported. No participant was excluded.

Participants. We recruited 489 participants from three diverse sources: our campus lab, comprising students ($n = 187$; $M_{age} = 20.73$, $SD_{age} = 5.47$; 54.00% women; 46.50% White); our downtown lab, comprising community adults ($n = 150$; $M_{age} = 35.37$, $SD_{age} = 13.38$; 33.33% women; 30.70% White); and Amazon Turk, comprising a national panel ($n = 152$; $M_{age} = 31.38$, $SD_{age} = 9.93$; 37.50% women; 81.60% White). Participants volunteered to complete a study on judging life experiences for nominal pay.

Procedure. Participants across all sources followed identical procedures. They were randomly assigned to reflect on how they have changed either over the past 1 year ($n_{AllSources} = 157$), 5 years ($n_{AllSources} = 168$), or 10 years ($n_{AllSources} = 164$). First, they read:

> People change, for better and for worse, in both temporary and in longer-lasting ways. All things considered, how much do you feel you've changed as a person over the past 1 year [5 years] [10 years] of your life—how different are you now than you were about 1 year [5 years] [10 years] ago?

Note how this prompt does not add any objective demand nor instructs participants to interpret change with any particular meaning or direction. Participants responded on a scale from 1 (*I have changed very little*) to 9 (*I have changed very much*), which served as our measure of global change. Participants were then asked specific questions about different kinds of change over the same time: their degree of positive change, rated from 1 (*I have had few positive changes*) to 9 (*I have had many positive changes*); negative change, rated from 1 (*I have had few negative changes*) to 9 (*I have had many negative changes*); and neutral change, rated from 1 (*I have had few neutral changes*) to 9 (*I have had many neutral changes*). These items served as our measures of directional change. We intentionally presented global change before directional change to best assess what naturally came to mind when participants first made their estimate of change at large. Because of the independent nature of these items, higher estimates of one's global change should logically be associated with greater change along each of the directional estimates. Finally, participants reported demographic information and were thanked and debriefed.

Results and Discussion

Data were submitted to bivariate correlational analyses. We first report composite results across time and source (full sample) for the most direct and highest-powered test.

As predicted, perceptions of global change ($M = 6.88$, $SD = 2.08$) were strongly correlated with one's degree of positive change ($M = 6.38$, $SD = 2.15$), $r = .56$, $p < .001$, as well as with one's degree of neutral change ($M = 5.30$, $SD = 2.12$), $r = .23$, $p < .001$; however, marking the critical departure, global change was *not* correlated with one's degree of negative change ($M = 3.97$, $SD = 2.10$), $r = .05$, $p = .237$. Further, these global-positive correlations were stronger than global-neutral correlations ($Z = 6.21$, $p < .001$ two-tailed) and global-negative correlations ($Z = 9.08$, $p < .001$ two-tailed). Hence, when people assess how they have generally changed over time, they mainly draw on ways they have gotten better (and to a lesser extent other mundane fluctuations), but apparently *not* on ways they have gotten worse; more change is interpreted as more improvement but not more decline, in line with our hypothesis. Bolstering this observation, all results remain when controlling for participant sex, age, and ethnicity via regression: global change remains associated with positive ($\beta = .56$, $t = 14.38$, $p < .001$) and neutral ($\beta = .20$, $t = 4.46$, $p < .001$) change, and unassociated with negative change ($\beta = .06$, $t = 1.24$, $p = .215$). None of these variables exerted main effects ($\beta s \leq .25$, $ts \leq 1.58$, $ps \geq .114$) nor did they produce any interactions ($\beta s \leq .33$, $ts \leq 1.57$, $ps \geq .117$); lack of variation across demographics is consistent for all analyses and studies, so they are not discussed further.

Critically, all results also remained across time and source (see Table 1). With little exception, more global change was almost entirely associated with more positive change, and almost never with more negative change—regardless of population and whether judging the past 1, 5, or 10 years. These findings boost external validity and provide a robust internal replication, suggesting the patterns extend to how people perceive their change in general and do not hinge on one specific methodological nuance or measure.

Study 1 reveals the basic effect: when people are asked to estimate how much they have changed in general, they actually report how much they have changed *for the better* and in turn this

Table 1

Correlations Between Ratings of Global Change and Specific Directional Changes Across Each Condition and Population (Study 1)

Time	Source	Global-positive	Global-neutral	Global-negative
1 year	Student	$r = .518$, $p < .001$	$r = .200$, $p = .126$	$r = .101$, $p = .403$
	Community	$r = .511$, $p < .001$	$r = .216$, $p = .137$	$r = .137$, $p = .347$
	MTurk	$r = .744$, $p < .001$	$r = .200$, $p = .172$	$r = -.149$, $p = .312$
5 years	Student	$r = .219$, $p = .083$	$r = -.032$, $p = .083$	$r = .005$, $p = .967$
	Community	$r = .597$, $p < .001$	$r = .073$, $p = .609$	$r = .104$, $p = .462$
	MTurk	$r = .456$, $p = .001$	$r = .351$, $p = .011$	$r = .125$, $p = .378$
10 years	Student	$r = .384$, $p = .002$	$r = .144$, $p = .261$	$r = .103$, $p = .423$
	Community	$r = .407$, $p = .004$	$r = .330$, $p = .021$	$r = .178$, $p = .221$
	MTurk	$r = .590$, $p < .001$	$r = .043$, $p = .761$	$r = -.269$, $p = .054$

Note. The more participants reported changing overall, the more they had reported improving; however, more global change was not associated with more neutral and negative changes, consistent with our hypothesis.

estimate is unaffected by the number of negative changes that have also unfolded. Next, we sought to replicate this finding via an experimental design.

Study 2

Unpacking the Past Year

If change is intuitively interpreted as improvement, then directly asking people to describe how they have changed should prompt them to recount the ways they have improved but not the ways they have declined—even though nothing about the concept itself should objectively compel people to do so. This possibility was tested in Study 2.

Method

Participants. We recruited 402 participants (M_{age} = 36.49, SD_{age} = 12.37; 54.70% women; 79.90% White) to voluntarily complete a study on judging life experiences for nominal pay. Because Study 1 suggests the effect does not depend on time or source, all participants evaluated a time frame of 1 year and were recruited from Amazon Turk.

Procedure. Participants were randomly assigned to 1 of 4 conditions. Some participants were asked to describe how they have changed in general over the past year (n = 100), with no explicit indication or demand for how to interpret change. Their task was to list as many changes as they possibly could list, described via the prompt below:

> People change, sometimes and in some ways, for better and for worse. Your task is to list how you feel you have changed as a person over the past 1 year or so of your life—how are you different now than you were about 1 year ago? Please list as many real and unique ways as you can think of.

This method allows us to establish the average number of changes that people can recall experiencing over a year. Further, participants then saw their list in full and were asked to categorize each response as either a "negative change (a change for the worse)"; "positive change (a change for the better)"; or "neutral change (neither a change for the worse nor a change for the better, but simply a change over time)." In this way, we can specifically test our hypothesis that the majority of what people naturally list in response to the question of global change involves positive improvement and neglects other trajectories.

Critically, to properly test our hypothesis, this percentage needs to be compared against an appropriate reference point; otherwise, a majority of positive changes could simply reflect that people *actually did* experience more improvement than decline over the past year, rather than a biased interpretation of the word change. Accordingly, other participants read a similar prompt except they were explicitly asked to only list positive changes (n = 106), only negative changes (n = 104), or only neutral changes (n = 92), as many as they could think of. When added together, these numbers reflect the *actual* total number of changes that people can recall experiencing, with the *actual* percentage of each direction. We used a between-subjects design so to avoid confounds with anchoring and, thus, reinforce accuracy (e.g., if people first list X

positive changes, they may simply assume to list X negative changes even if this does not reflect their actual frequency).

Together, this design allows for a more proper test of our hypothesis. If people fairly interpret general change to include all the ways they have indeed changed, then global lists should include the same percentage of positive changes as in these actual lists; but if global percentages are higher, this discrepancy strongly suggests that global change only prompts people to reflect on the positive (even though they *did* experience negative and neutral changes from which they could have otherwise drawn if explicitly prompted). Finally, all participants completed a forced-choice attention check regarding what they had been asked to list, with the options: *Only ways I've changed for the better; Only ways I've changed for the worse; Only ways I've changed neutrally, neither for better nor for worse; Just ways I've changed with no specific instructions for "better," "worse," or "neutral."* They reported demographic information and were thanked and debriefed.

Results and Discussion

Only 7.21% of participants (29 of 402) failed the attention check. Eliminating these participants does not affect any result, so they are retained in analyses.

First, we report results from the three directional conditions to establish the comparison point for analyses (i.e., the *actual* number of changes that people can recall). On average, participants recalled a total of 9.60 changes, added across positive (M = 3.39, SD = 1.60), negative (M = 2.98, SD = 1.34), and neutral (M = 3.23, SD = 1.39). In other words, a fair draw from the mix of changes that people can actually recall should result in a list of 35.31% positive changes, 31.04% negative changes, and 33.65% neutral changes.

For our primary analyses, we compare these percentages to those generated in the global change condition, via one-sample t tests with each of the observed global means compared what would be expected for each based on the actual percentages. Given that change in general should objectively bring to mind the full range of changes, global lists should comprise the same breakdown of percentages. However, in line with our hypothesis, this was not the case. Global participants recalled far fewer than 9.60 changes (M = 3.80, SD = 1.50), likely because they *only* recalled improvement: their lists included a full 72.85% positive changes (M = 2.69, SD = 1.57), sizably more than the expected 35.31% positive changes, $t(99)$ = 8.58, p < .001, d = .86, 95% $CI_{difference}$ [1.04, 1.66]; and in turn, marking the necessary trade-off, only 17.84% negative changes (M = .72, SD = 1.23), fewer than the expected 31.04% negative changes, $t(99)$ = −3.74, p < .001, d = .37, 95% $CI_{difference}$ [.22, .70]; and only 9.31% neutral changes (M = .39, SD = .62), fewer than the expected 33.65% neutral changes, $t(99)$ = −14.41, p < .001, d = 1.43, 95% $CI_{difference}$ [.77, 1.01]. As predicted, when asked to exhaust a list of all the ways they changed in general in the past year—with no explicit cap or demand for interpreting the task—people mostly listed only their positive ways of changing for the better and then considered the task complete.

Supplementary analysis. Trying to measure actual accuracy here is inherently difficult, because we cannot be sure if participants truly exhausted all the ways they had changed or instead if they followed a general rule of thumb regarding how many things

to list given the context of the study (e.g., across all conditions: "Listing 3 or 4 things seems like enough to finish the study"). Although participants still listed a majority of positive changes, it may be effectively impossible to disentangle *everything* in participants' minds from broader inferences about how much work to do to complete the task (Grice, 1975).

We maintain that our results reflect the best attempt at capturing true accuracy since we directly instructed participants to bring to mind as many things as possible, and we also excluded any cues that would limit this search (e.g., we gave them an open-ended essay box rather than a set number of individual lines). Nonetheless, one additional way to test our hypothesis so to avoid this potential ceiling is to ask participants to list more. Specifically, if roughly nine changes represent a complete list with about a third each going to positive, negative, and neutral changes (as we find above), then participants who are instructed to list nine changes should also report this as an even divide across nine items if they are bringing to mind a fair draw. Deviations from these percentages, in particular an overrepresentation of positive changes above one-third, may provide supplementary support.

To this point, we recruited a new group of 102 participants ($M_{age} = 33.54$, $SD_{age} = 10.49$; 56.90% women; 73.50% White) from the same population to take the study in the same way as "global" participants, except we explicitly asked them to list nine changes (only 12.75% of these participants [13 of 102] failed the attention check; all are retained). These participants listed $M = 8.70$ ($SD = 1.48$) changes, confirming they followed basic instructions. Out of these 8.70 listed changes, however, participants reported a striking majority of 77.59% positive changes ($M = 6.75$, $SD = 2.50$), far more than the expected 35.31% positive changes, $t(101) = 14.87$, $p < .001$, $d = 1.47$, 95% $CI_{difference}$ [3.19, 4.17]; and again marking the necessary trade-off, they reported only 13.22% negative changes ($M = 1.15$, $SD = 1.80$), fewer than the expected 31.04% negative changes, $t(101) = 8.70$, $p < .001$, $d = .86$, 95% $CI_{difference}$ [1.19, 1.90]; and a mere 9.20% neutral changes ($M = 0.80$, $SD = 1.06$), fewer than the expected 33.65% neutral changes, $t(102) = 20.29$, $p < .001$, $d = 2.00$, 95% $CI_{difference}$ [1.92, 2.34]. These findings replicate our primary analyses from a different perspective, affording converging support.

On the whole, Study 2 extends the patterns of Study 1 experimentally. Despite also having experienced decline and other fluctuations, people were not cued to these other trajectories when asked to simply reflect back on their change in general; change on its own was subjectively translated as improvement alone. Next, we sought to clarify and bolster this effect further via more diverse measures and control comparisons.

Study 3

Change Versus Stability

If change is interpreted as improvement, then asking people to recount ways they have changed should lead them generate more positive answers (in their minds, how they have improved) than when asked how they have remained the same (in their minds, how they have failed to improve), even though nothing should objectively preclude the opposite; the presence of change could just as well be negative (e.g., "The spark is gone") or its absence positive (e.g., "I'm still in love"), but people may intuitively overlook these possibilities. Accordingly, participants in Study 3 wrote about ways they either have or have not changed. Without explicit demand for interpreting the task, we predicted that they would bring to mind more positive experiences when reflecting on personal change versus personal stability. This design allows for another replication of the basic effect, and can also provide clearer insight into what kinds of content people actually recount.

Method

Participants. We recruited 203 participants ($M_{age} = 33.32$, $SD_{age} = 10.79$; 47.80% women; 73.90% White) to voluntarily complete a study on judging life experiences in exchange for nominal pay. Following the logic of Study 2, all participants evaluated a time frame of 1 year and were recruited from Amazon Turk.

Procedure. Participants were randomly assigned to reflect on how they have either stayed the same ($n = 100$) or changed ($n = 103$) over the preceding year. First, they wrote a 3–5 sentence (minimum) essay based on what came to mind from having read the given prompt, which varied by condition. They read 1 of the 2 following prompts:

> People stay the same [change], sometimes and in some ways, for better and for worse. All things considered, describe how you feel you have stayed the same [changed] as a person over the past 1 year or so of your life—how are you no different [different] now than you were about 1 year ago?

Again, note how these instructions do not advocate any particular meaning or direction, and stability and change are depicted as equally normative. After writing, participants submitted their responses and on the next screen were shown their essay in full. They evaluated it across three blocks of questions, which were presented in counterbalanced order.

Positivity (DV block). Participants coded their essays in terms of valence. They rated, "Overall, how negative or positive are these ways of staying the same [changing]?" from 1 (*extremely negative*) to 7 (*extremely positive*); "Overall, does this reflect things staying the same [changing] in a bad way or in a good way?" from 1 (*very much in a bad way*) to 7 (*very much in a good way*); and "Overall, how would you describe these ways of staying the same [changing]" from 1 (*horrible*) to 7 (*fantastic*). Each midpoint was labeled at 4 (*neutral*). These items served as our main dependent measures—that is, the positivity of what comes to mind when naturally reflecting on the concept of change.

Categorization (control block). Participants were also asked to categorize their actual essay content to account for potential confounding differences in essays beyond positivity. They indicated how much the essay described something about the way they "look (e.g., physical appearance, physical health, age)"; "think (e.g., learned knowledge, beliefs, preferences)"; "feel (e.g., emotions, mental health, mood)"; "act (e.g., habits, routines, hobbies)"; and how much it described their "circumstances (e.g., relationships, jobs, locations)." Each category was rated from 1 (*definitely no*) to 7 (*definitely yes*).

Task experience (control block). Participants also rated how difficult it felt to figure out what to write and the level of detail in what came to mind while writing, each from 1 (*not at all*) to 7 (*extremely*) with the midpoints labeled at 4 (*neutral*). These task-

specific items helped to further account for potential differences beyond positivity.

Finally, participants completed a manipulation check by rating their objective change during the past year from 1 (*objectively, I am not at all different as a person now compared to 1 year ago*) to 7 (*objectively, I am extremely different as a person now compared to 1 year ago*), with the midpoint labeled at 4 (*neutral*). They also completed two forced-choice attention checks, regarding whether they were asked to write about *ways of changing* or *ways of staying the same*, and to evaluate the past *1 year* or *10 years* [bogus option]. They then reported demographic information and were thanked and debriefed.

Results and Discussion

Only 4.43% of participants (9 of 203) failed at least one of the attention checks. Eliminating these participants does not affect any result, so they are retained in analyses. Data were submitted to independent-samples t tests, with condition (stability/change) as the grouping variable and the reported items as dependent measures.

Manipulation check. The essay manipulation worked. Being reminded of ways they have changed led participants to rate themselves as having changed more over the past year ($M = 5.04$, $SD = 1.20$) than being reminded of their stability ($M = 3.14$, $SD = 1.48$), $t(201) = -10.03$, $p < .001$, $d = 1.41$, 95% CI$_{difference}$ [1.53, 2.27]. Participants thus reflected on experiences consistent with their task prompts, validating our manipulation.

Main analyses. Positivity items were collapsed into a scale ($\alpha = .97$). As predicted, participants who were asked to describe their general change over the past year brought to mind significantly more positive experiences ($M = 5.97$, $SD = 1.33$) than those who described personal stability over the same period ($M = 4.56$, $SD = 1.90$), $t(201) = -6.12$, $p < .001$, $d = .86$, 95% CI$_{difference}$ [.95, 1.86]. This replicates and extends our basic effect. Differences should have emerged only if people naturally interpret change as "improvement" and, thus, think of its presence as better than its absence; in turn, with no explicit instruction to do so, most participants wrote uniformly good examples of change such as "I've become a stronger, more confident person," "I'm more patient," and "I'm more relaxed and more happy," but bad examples of stability such as "I'm still a negative minded person," "I still haven't fixed the many flaws I have," and "I'm the same weight despite attempts at dieting." As predicted, though the task could have objectively elicited the opposite (e.g., thinking of change as having gained weight and "stability" as having stuck to one's values), it subjectively prompted an asymmetric assessment of *progress*.

Control analyses. Regarding the actual content that came to mind, participants mostly wrote about the same kinds of things. They did not differ in terms of how much they wrote about their "looks" as staying the same ($M = 2.48$, $SD = 2.25$) versus changing ($M = 2.14$, $SD = 1.86$), $t(201) = 1.19$, $p = .236$, $d = .16$, 95% CI$_{difference}$ [−.23, .91]; their "thoughts" as staying the same ($M = 5.04$, $SD = 2.07$) versus changing ($M = 5.22$, $SD = 1.78$), $t(201) = -.68$, $p = .500$, $d = .09$, 95% CI$_{difference}$ [−.72, .35]; their "actions" as staying the same ($M = 5.39$, $SD = 1.90$) versus changing ($M = 5.16$, $SD = 1.88$), $t(201) = .89$, $p = .377$, $d = .12$, 95% CI$_{difference}$ [−.29, .76]; or their "circumstances" as

staying the same ($M = 4.85$, $SD = 2.20$) versus changing ($M = 4.78$, $SD = 2.13$), $t(201) = .24$, $p = .810$, $d = .03$, 95% CI$_{difference}$ [−.53, .67]. The only difference in content was in "feelings": participants were more likely to describe their feelings as having changed ($M = 5.72$, $SD = 1.56$) than as having stayed the same ($M = 4.67$, $SD = 2.08$), $t(201) = -4.08$, $p < .001$, $d = .57$, 95% CI$_{difference}$ [.54, 1.56]. We also observed differences in terms of completing the task, such that writing essays about stability felt harder ($M = 2.48$, $SD = 1.50$) than writing essays about change ($M = 2.06$, $SD = 1.56$), $t(201) = 1.96$, $p = .051$, $d = .27$, 95% CI$_{difference}$ [−.003, .85], and likewise evoked less detailed imagery ($M = 4.78$, $SD = 1.35$) than change ($M = 5.18$, $SD = 1.30$), $t(201) = -2.18$, $p = .030$, $d = .30$, 95% CI$_{difference}$ [−.04, −.77]. However, critically, our primary results remain when controlling for category and task ratings via analysis of covariance (ANCOVA): even after accounting for all of these variables, change participants were still naturally induced to recount more positive experiences than "stability" participants, $F(1, 194) = 30.92$, $p < .001$, $\eta_p^2 = .137$, 95% CI$_{difference}$ [.81, 1.71]).

Together, Study 3 replicates and extends the basic effect in Studies 1–2. People mostly bring to mind evidence of improvement when assessing change versus stability, even though they could also be cued to decline and other trends. This effect cannot be explained by various relevant differences beyond people's interpretation of positivity.

Next, we explored consequences. If change is subjectively translated as change *for the better*, merely asking people to reflect on the concept may make them feel happier and more positively view their lives; thinking about past change might be a novel strategy for enhancing present wellbeing, because it actually only reminds us of our improvement.

Study 4

Cheerfully Changed

In Study 4, participants completed the essay task from Study 3, but afterward completed various measures of current wellbeing. We predicted that reflecting on change would lead people to feel better, and this boost would be mediated by the kinds of content they had been naturally induced to write. Moreover, we added a neutral control condition. This allowed us to assess if thinking about change can indeed *increase* wellbeing, not merely relative to thinking about stability but also to a baseline emotional state.

This design also helps provide a nuanced test of underlying mechanisms. If the effects so far have been driven purely by conversational norms—the possibility that the general concept of change does bring to mind decline and/or other mundane changes, but people either assume to not share these experiences or simply wish to conceal them—then they should *not* subsequently experience greater wellbeing on the surface.

Method

Participants. We recruited 300 participants ($M_{age} = 34.67$, $SD_{age} = 11.69$; 44.30% women; 73.70% White) to voluntarily complete a study on judging life experiences for nominal pay. Again, they evaluated 1 year and were recruited from Amazon Turk.

Procedure. Participants were randomly assigned to stability ($n = 95$), change ($n = 98$), or neutral ($n = 107$) conditions. First, participants across all conditions completed the essay task. Stability and change participants followed identical procedures as in the previous study. "Neutral" control participants were prompted to simply type the alphabet, ostensibly to ensure that our essay recording software was working properly. After, all participants indicated their wellbeing along three measures: they reported their current mood, felt meaning in life, and life satisfaction, in counterbalanced order. Like using a diversity of time frames and sources in Study 1, multiple wellbeing measures allow us to establish a general sense of consequences beyond any particular methodological nuance. Then, all participants completed the positivity coding items from our previous study, serving as a replication of the basic effect and also our proposed mediator (control participants rated the same items except pertaining to the "kinds of thoughts [that had been] naturally just going through [their] minds" during the task). We intentionally ordered the mediator (positivity coding items) after the dependent measures (wellbeing) so to reduce demand and contamination effects. Assessing the mediator first can risk artificially putting that variable top of mind, therefore inflating the likelihood that it statistically influences subsequent variables even if it is not a "true" mediator (Ellsworth & Gonzalez, 2003; Iacobucci, Saldanha, & Deng, 2007). This set order also maintains subtlety in testing whether merely thinking about change can boost wellbeing.

Current mood was assessed via the well-established Positive and Negative Affect Schedule (PANAS: Watson, Clark, & Tellegen, 1988), which includes 10-item subscales for positive mood (e.g., excited, enthusiastic) and negative mood (e.g., distressed, upset). Each item was rated according to how strongly it presently felt (1 = *very slightly or not at all*, 2 = *a little*, 3 = *moderately*, 4 = *quite a bit*, 5 = *extremely*). PANAS is one of the most valid and widely used indicators of experienced wellbeing and is linked with important health and behavioral outcomes (Crawford & Henry, 2004; Watson & Clark, 1997).

Felt meaning in life was assessed via the Presence of Meaning subscale from the well-established Meaning in Life Questionnaire (Steger, Frazier, Oishi, & Kaler, 2006). A sample item is, "I have a good sense of what makes my life meaningful." Participants rated 5 items in total, each according to how much it felt true for them at the present moment (1 = *absolutely untrue*, 2 = *mostly untrue*, 3 = *somewhat untrue*, 4 = *cannot say true or false*, 5 = *somewhat true*, 6 = *mostly true*, 7 = *absolutely true*). This captures a different but equally critical aspect of experienced wellbeing: not just moods and hedonic states but broader feelings of purpose and understanding. Higher meaning in life predicts a wide variety of outcomes, from greater happiness and successful goal pursuit to lower anxiety and even less physical pain (Heintzelman & King, 2015; Janoff-Bulman, 1992).

Life satisfaction was assessed via two items: participants rated how satisfied and how happy they felt with their lives as a whole these days, each from 1 (*not at all*) to 7 (*extremely*). These items are often used together in experiments and policy surveys to tap global (vs. momentary) happiness, which drives broader life decisions (Diener, Oishi, & Lucas, 2009; Kahneman & Krueger, 2006; Schwarz & Clore, 1983). They are directly recommended per OECD (2013) guidelines as representative assessments of wellbeing. Finally, participants completed a manipulation check and

attention checks like in our previous study, reported demographic information, and were thanked and debriefed.

Results and Discussion

Only 3.00% of participants (9 of 300) failed at least one of the attention checks. Eliminating these participants does not affect any result, so they are retained in analyses. Data were submitted to one-way ANOVA analyses, with condition (stability/change/control) as the factor and the reported items as dependent measures.

Manipulation check. The essay manipulation again worked, as reflected in an omnibus effect of condition, $F(2, 297) = 44.33$, $p < .001$, $\eta_p^2 = .23$. Planned contrasts confirm that stability participants saw themselves as having changed less ($M = 2.78$, $SD = 1.40$) than control participants ($M = 4.11$, $SD = 1.68$), $t(297) = -6.28$, $p < .001$, $d = .86$, 95% CI$_{\text{difference}}$ [.90, 1.76], whereas change participants saw themselves as having changed more ($M = 4.79$, $SD = 1.40$), $t(297) = 3.20$, $p = .002$, $d = .44$, 95% CI$_{\text{difference}}$ [.25, 1.11] (change participants also reported more change relative to stability participants, $t(297) = 9.26$, $p < .001$, $d = 1.43$, 95% CI$_{\text{difference}}$ [1.61, 2.41], which replicates Study 3).

Main analyses. Following their respective coding procedures, each measure was collapsed into its own scale: PANAS-good ($\alpha = .93$), PANAS-bad ($\alpha = .94$), meaning in life ($\alpha = .93$), and life satisfaction ($\alpha = .97$). As predicted, reflecting on general change induced *specifically positive* boosts in wellbeing (see Figure 1), demonstrated by omnibus effects for PANAS-good, $F(2, 297) = 16.79$, $p < .001$, $\eta_p^2 = .10$, meaning in life, $F(2, 297) = 5.44$, $p = .005$, $\eta_p^2 = .04$, and life satisfaction, $F(2, 297) = 2.56$, $p = .079$, $\eta_p^2 = .02$.

We first report planned contrasts between stability and change conditions, paralleling our analyses in Study 3. Participants experienced more positive moods after reflecting on change from the past ($M = 3.47$, $SD = .94$) than on stability with it ($M = 2.72$, $SD = 1.07$), $t(297) = -5.29$, $p < .001$, $d = 1.01$, 95% CI$_{\text{difference}}$ [.46, 1.04]; they felt greater meaning in their current lives after reflecting on change ($M = 5.05$, $SD = 1.37$) versus stability ($M =$
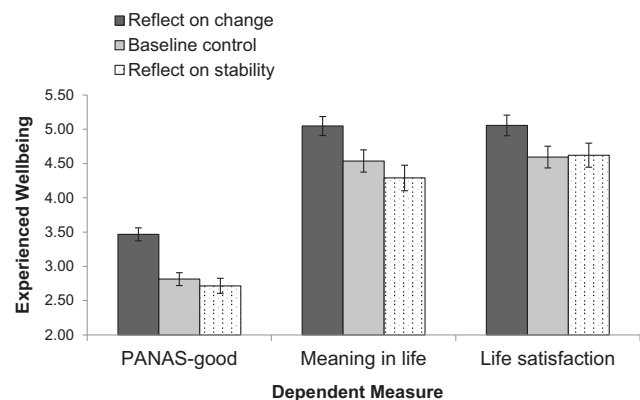

Figure 1. Degree of experienced wellbeing following each of the writing tasks, across our different measures of wellbeing (Study 4). Error bars represent ±1 *SE*. Consistent with our hypothesis, writing about "change" subsequently led participants to experience greater wellbeing across all measures than both "control" participants and "stability" participants, who did not differ from one another.

4.29, $SD = 1.81$), $t(297) = -3.22$, $p = .001$, $d = .47$, 95% $CI_{difference}$ [.31, 1.21]; and they felt more satisfied with their current lives after reflecting on change ($M = 5.06$, $SD = 1.49$) versus stability ($M = 4.62$, $SD = 1.71$), $t(297) = -1.87$, $p = .063$, $d = .27$, 95% $CI_{difference}$ [−.01, .89]. These boosts are further bolstered by the control contrasts: reflecting on stability did not *undermine* baseline mood ($M = 2.82$, $SD = .96$), $t(297) = -.71$, $p = .477$, $d = .10$, 95% $CI_{difference}$ [−.18, .38]; meaning ($M = 4.54$, $SD = 1.68$), $t(297) = -1.07$, $p = .286$, $d = .14$, 95% $CI_{difference}$ [−.23, .73]; or satisfaction ($M = 4.59$, $SD = 1.64$), $t(297) = .12$, $p = .904$, $d = .02$, 95% $CI_{difference}$ [−.50, .44]; however, reflecting on change indeed *enhanced* wellbeing, above baseline mood, $t(297) = -4.73$, $p < .001$, $d = .68$, 95% $CI_{difference}$ [.39, .91]; meaning, $t(297) = -2.24$, $p = .026$, $d = .33$, 95% $CI_{difference}$ [.09, .93]; and satisfaction, $t(297) = -2.05$, $p = .042$, $d = .30$, 95% $CI_{difference}$ [.04, .90].

The only measure that showed no differences was PANAS-bad, $F(2, 297) = 1.28$, $p = .279$, $\eta_p^2 = .008$, which did not differ across stability ($M = 1.62$, $SD = .86$) versus change ($M = 1.57$, $SD = .72$), $t(297) = .46$, $p = .647$, $d = .06$, 95% $CI_{difference}$ [−.17, .27]; stability versus control ($M = 1.45$, $SD = .72$), $t(297) = 1.55$, $p = .122$, $d = .21$, 95% $CI_{difference}$ [−.39, .05]; nor change versus control, $t(297) = -1.09$, $p = .276$, $d = .17$, 95% $CI_{difference}$ [−.32, .08]. Given these generally low means and the nonaversive nature of the study, this likely reflects an incidental floor effect. Nonetheless, these null results (plus all of the null contrasts for stability vs. control) provide further validation that the action of the effect rests in change creating an *actively positive* boost, as hypothesized.

Mediation. We further predicted these boosts in wellbeing would be driven by the types of experiences that naturally came to mind from the essay prompt. Accordingly, positivity was collapsed into a scale ($\alpha = .96$), and we observed an omnibus effect of condition, $F(2, 297) = 17.23$, $p < .001$, $\eta_p^2 = .10$. Planned contrasts reveal that participants naturally brought to mind more positive experiences when asked to reflect on change ($M = 5.61$, $SD = 1.49$) than on stability ($M = 4.42$, $SD = 1.99$), $t(297) = -5.31$, $p < .001$, $d = .68$, 95% $CI_{difference}$ [.69, 1.69]. This finding directly replicates the basic effect: thinking about change prompts people to think about *positive* changes. Control contrasts extend this effect even further: although reflecting on stability evokes equally positive thoughts as what people normally think about ($M = 4.56$, $SD = 1.12$), $t(297) = -.62$, $p = .534$, $d = .09$, 95% $CI_{difference}$ [−.30, .58], reflecting on change evokes even more positivity than people's baseline thoughts, $t(297) = -4.84$, $p < .001$, $d = .80$, 95% $CI_{difference}$ [.69, 1.41].

These differences fully mediated the effect of the essay task on wellbeing (see Figure 2). To simplify our mediation analyses, we summed the measures to create a general wellbeing index ($\alpha = .82$); PANAS-bad was coded in reverse, with higher sum scores reflecting greater wellbeing. In turn, condition became nonsignificant when controlling for positivity, whereas positivity remained significant even after controlling for condition. And the indirect effect of condition on experienced wellbeing, through positivity, was significant (95% $CI_{bootstrapping}$ [.72, 1.78]).

Study 4 strongly supports our basic framework, while also highlighting some important consequences of it. Asking people to reflect on change in general leads them to feel better about their lives, because they *actually* are just cued to improvement. Think-
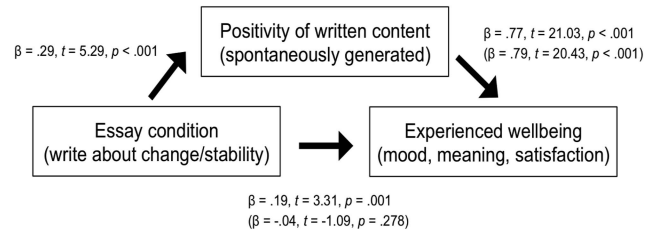

Figure 2. Mediation results (Study 4). Terms in parentheses were derived from models that included both factors. Participants who wrote about "change" experienced greater wellbeing precisely because they spontaneously brought to mind more positive content.

ing about how we have changed from our pasts can enhance how we perceive our presents.

Study 5

My Change Versus Yours

Last, we go beyond documenting the basic effect (Studies 1–3) and highlighting consequences (Study 4) to more directly addressing mechanisms. We have proposed a principal source of the bias reflects self-enhancement motives: people notoriously view their lives as ever-progressing in a positive light (Beauregard & Dunning, 1998; Kunda, 1990; McAdams, 2008; Wilson & Ross, 2001), and thus, even the mere concept of change may be coded as change for the better. Various aspects of Studies 1–4 support this idea. However, Study 5 tested it more directly via a moderation-based strategy: people should be less likely to associate global change with directional improvement when evaluating change in a *friend*, if self-enhancement is at play (Chambers & Windschitl, 2004). We also adopted a new methodological approach so to test for further converging evidence.

Method

Participants. We recruited 200 participants from Amazon Turk ($M_{age} = 34.28$, $SD_{age} = 10.01$; 47.50% women; 79.50% White) to voluntarily complete a study on "free associations and gut reactions" for nominal pay.

Procedure. Participants were randomly assigned to self ($n = 100$) or friend ($n = 100$) conditions. We told participants that we were ostensibly interested in people's spontaneous reactions to various kinds of words, and so they would complete a speeded response task. In the task, a phrase was presented in the middle of the screen and participants had 5 seconds to report and submit their reaction to the target word within the phrase, rated on a scale from 1 (*mostly a negative thing*) to 7 (*mostly a positive thing*). The screen then cleared and a new phrase appeared, and participants again had 5 seconds to rate their reactions to this new word. The task was described in thorough step-by-step detail across numerous instruction screens before participants were allowed to begin. They were exposed to 4 words in all: "to be *changed*" (word of interest), "to be *stressed*" (negative control word), "to be *relaxed*" (positive control word), and "to be *gilberted*" (nonsense control word), presented one at a time in this order. Critically, the focus of the task varied by condition. "Self" participants were told to have

a self-oriented interpretive mindset going into the task: they imagined how bad or good each word seems for their own lives, as if they were the subject of each phrase. "Friend" participants took an other-oriented focus: they were asked to imagine a friend as the subject of each phrase and to consider how bad or good the word seems for a friend. Otherwise the task was identical.

Note some important aspects of this study that help speak to mechanisms. Like Study 4, one benefit of the design (beyond testing our hypothesis via another perspective) is that it again bypasses inferences of conversational norms; we simply assess people's spontaneous associations with the concept of change, irrespective of having to respond to a prompt or recall X number of experiences. And the manipulation not only tests *for* self-enhancement, but also *against* the possibility that episodes of improvement possess some objective feature that renders them more salient; if so, this also applies to the experiences that should naturally come to mind when imagining someone else's change at large.

Finally, all participants completed an attention check (*I was asked to approach the words as if they applied to ME*; *I was asked to approach the words as if they applied to A FRIEND*), reported demographic information, and were thanked and debriefed.

Results and Discussion

Only 1.50% of participants (3 of 200) failed the attention check. Eliminating these participants does not affect any result, so they are retained in analyses. Data were submitted to independent-samples t tests, with condition (self/friend) as the grouping variable and spontaneous reactions to each of the four words as dependent measures.

As predicted, we observed only an effect for people's interpretations of *change* (see Figure 3). There were no differences between conditions for the three control words: to be stressed seemed equally negative for both the self ($M = 1.74$, $SD = 1.00$) and for a friend ($M = 1.91$, $SD = .93$), $t(198) = -1.24$, $p = .216$, $d = .18$, 95% CI$_{difference}$ [$-.44$, .10]; to be relaxed seemed equally positive

for both the self ($M = 6.57$, $SD = .64$) and for a friend ($M = 6.48$, $SD = .76$), $t(198) = .91$, $p = .365$, $d = .13$, 95% CI$_{difference}$ [$-.11$, .29]; and to be *gilberted* seemed equally neutral for both the self ($M = 3.76$, $SD = .71$) and for a friend ($M = 3.78$, $SD = .70$), $t(198) = -.20$, $p = .842$, $d = .03$, 95% CI$_{difference}$ [$-.22$, .18]. Conversely, however, to be changed seemed significantly more positive when thinking about self-change ($M = 4.84$, $SD = 1.47$) than when thinking about other-change ($M = 3.96$, $SD = 1.19$), $t(198) = 4.66$, $p < .001$, $d = .66$, 95% CI$_{difference}$ [.51, 1.25]. Hence, although participants were exposed to the same exact word in the same exact way, they spontaneously interpreted change in a positive light only when thinking about change in *themselves*. One-sample t tests against the scale midpoint (4) further corroborate this observation: while change in others was interpreted as relatively mixed (i.e., it was no different from the neutral midpoint: $t(99) = -.34$, $p = .737$, $d = .07$, 95% CI$_{difference}$ [$-.28$, .20]), self-change was interpreted as more uniformly positive (i.e., it was significantly greater than the midpoint: $t(99) = 5.72$, $p < .001$, $d = 1.15$, 95% CI$_{difference}$ [.55, 1.13]).

These findings extend our framework in important ways. First, we replicate the same core effect using yet another methodological approach. Second, this approach more directly captures the idea of natural, implicit interpretations of the concept of change and thus provides evidence against other accounts that may contribute to the effect. Third, we find a critical moderator: associations for "change = improvement" are more pronounced when thinking about change in the self than thinking about the same change in a friend.

Some readers may wonder how this result is affected by other factors that are unique to a self/other design. First, it could be that people still do express the bias when thinking about another person (i.e., they might indeed generate just as many examples of improvement when asked to reflect on a friend's change vs. their own change), but the perceived impact of a friend's change *on* the self undermines this positivity. In other words, people might still only bring to mind objectively positive changes about a friend (e.g., his or her recent marriage or promotion) when evaluating his or her change in general, but report less positive reactions because of the way these changes appear from their own perspective (e.g., by making the friend less available to socialize, or by stirring feelings of envy). On the one hand, this logic also applies to control words and we found no self/other differences for them. Nonetheless, Study 5 cannot definitively rule out this possibility. Testing for moderation using a more diverse set of other-oriented targets (e.g., distant or disliked others) would help account for the potential impact of such inferences. Second, given the fact that people simply know more about themselves than about others, it could be that fewer things overall may come to mind. Again, however, this same logic applies to control words yet we found no effects of the self/other manipulation for them; it cannot explain why self-change was above the midpoint; and the study was designed to evoke speedy associations that likely do not depend on some itemized list. Thus, such an asymmetry is typically taken as evidence for self-enhancement (Chambers & Windschitl, 2004), consistent with our rationale that people may associate their own change with improvement but not necessarily perceive change for *anyone* as so uniformly positive.
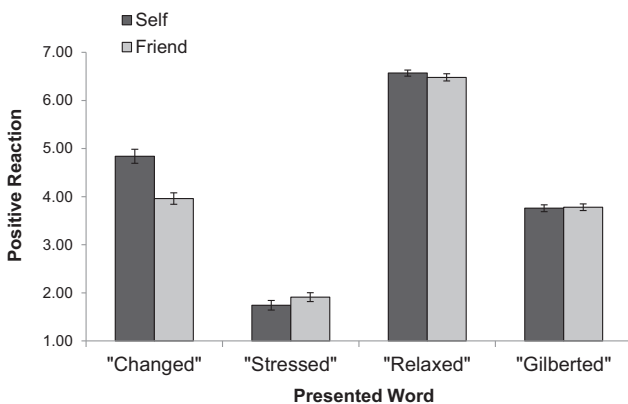
Figure 3. Spontaneous reactions to different words, with higher scores reflecting a more positive reaction (Study 5). Error bars represent ±1 *SE*. Consistent with our hypothesis, the concept of change was interpreted more positively when participants reacted to "change" in themselves versus change in a friend. Likewise, there were no differences between conditions in participants' reactions to the other control words.

General Discussion

The Oxford English Dictionary (2016) defines change as "the act or instance of becoming different." Five studies suggest a more selective definition to describe how people think about their own: when reflecting on how they have changed over time, people do not merely think about how they have become *different* but how they have become *better*. In Study 1, participants estimated their global change over the past 1 year, 5 years, or 10 years, which objectively should be rated as higher the more people have improved *and* the more they have declined; however, participants reported they had changed only when they had improved. In Studies 2–3, participants actively described how they have changed, but proceeded to recount mainly positive examples. In Study 4, reflecting on change led participants to subsequently experience greater wellbeing (i.e., better mood, more meaning, and higher satisfaction), precisely because they only brought to mind improvement. And in Study 5, these effects were more pronounced when people interpreted the concept of change in relation to themselves versus general change (i.e., in a friend), lending credence to a motivational component. Across studies, this same basic effect emerged among large samples, multiple measures and time frames, diverse age groups and other demographic differences, and various content categories of life events.

Theoretical Insights

At the theoretical level, the current findings run counter to standard accounts of negativity bias (Baumeister at al., 2001) and instead provide a robust demonstration of core assumptions within the self-enhancement literature. Most people strive to feel good about themselves (Leary & Downs, 1995; Higgins, 1996), so much so that they believe their lives have grown and will grow even more positive over time regardless of past and present circumstances (McAdams, 2008; Ryff, 1991; Sedikides & Green, 2000; Wilson & Ross, 2003). Our findings robustly replicate and expand upon these claims by revealing even the mere concept of change leads people to envision a change from bad to better.

One more nuanced and potentially fruitful insight of our studies pertains to their relevance for well-established research on lay theories of change (e.g., Conway & Ross, 1984; Dweck, 1999; Leith et al., 2014; Maruna & King, 2009; Robinson & Clore, 2002; Withey, 1954). These findings typically suggest that people who endorse an *incremental* theory of change as opposed to an *entity* theory—the belief that traits and abilities are malleable and can indeed change over time, rather than being fixed—are patently better off in the long run. In one study, students who held incremental beliefs about intelligence reported better grades and a more positive school experience over 2 years later compared with students who endorsed entity beliefs (Blackwell, Trzesniewski, & Dweck, 2007). And yet, nothing about the objective concept of malleability suggests it should be so positive; incremental students could have been just as prone to believing their intelligence can also disappear at any moment, leading to less resilience and optimism in the face of setbacks. However, this is not the case, in study after study. Why? Our framework suggests a provocative answer: incremental scales may be inadvertently capturing beliefs in *improvement* rather than malleability per se, to the extent the word change is presented in isolation which thereby triggers a biased interpretation akin to our effect (e.g., rating agreement with

items like, "I can substantially change the kind of person I am": Dweck, 1999). We may know much less than assumed about the nature and consequences of believing in "true" malleability, which may not be so axiomatically superior to believing in true stability.

This point more generally highlights the need for a better understanding of how participants might subjectively interpret the content of materials beyond the intentions of an experiment, which may inadvertently abet or impede confirmatory evidence. Research in relevant areas (e.g., lay theories, optimism) should be especially wary of the current effect, as a check to ensure that honest attempts at measuring perceptions of change do not inadvertently garner incomplete conclusions about how the mind works.

Practical Implications

Going beyond the laboratory, our findings also raise novel practical implications. Most obvious, to the extent that current action is driven by perceptions of recent change, people would be wise to specifically inquire about and discuss *directional* changes in lieu of change in general. Assessing experiences like the state of one's health or relationships are important to get right, which requires people to be informed about varied trajectories.

This observation raises everyday concerns about how people intuitively interpret questions, tasks, and concepts. Although the point here was to document these subjective interpretations, they could pose unintended confounds for other pursuits. Doctors might ask how our health has changed, partners about our happiness, politicians about our environments, and researchers about our attitudes, all with the goal of eliciting a literal reply. The current results suggest otherwise: what people might actually get in return are estimates of how things have specifically *improved*. This tendency could be intentionally exploited (e.g., political and advertising slogans that mention the general concept of change so to subtly prime positive progress), but equally concerning, unintentionally administered. For example, researchers might earnestly ask how one's "attitudes have changed," and conclude from the answer that people of one's demographic group feel surprisingly mature—but this may reflect a biased interpretation of the question. The converse ("How have your attitudes stayed the same?") might paint a less rosy picture.

Perhaps more intriguing than decisional accuracy, however, are possible benefits for happiness and wellbeing, as in Study 4. A burgeoning area of work documents time-related factors that improve how people feel, from imagining counterfactual memories (Kray et al., 2010) and experiencing nostalgia (Routledge et al., 2011) to merely thinking about distant moments (Waytz, Hershfield, & Tamir, 2015). To date these findings have been largely isolated as individual papers and effects, but ours point to a parsimonious basis: all such temporal manipulations might do the trick if they more broadly evoke thoughts of *change*, as they appear to do. Critically, the implicit function of the current effect could prove even more effective than existing strategies for enhancing wellbeing, which tend to directly ask people to recall positive events (Lyubomirsky, Sheldon, & Schkade, 2005). Explicit interventions often feel forced and make people aware of the source of the boost, leading them to discount their impact on wellbeing (Quoidbach, Mikolajczak, & Gross, 2015; Schwarz & Clore, 1983); subtly querying change at large may bypass this

reaction and instead lead people to naturally feel better "on their own."

However, we must note that Study 4 finds such benefits among ostensibly healthy and normally functioning adult populations. People with clinical depression or low self-esteem may actually feel worse after reflecting on their change over time, because they may immediately think of their decline. We hesitate to recommend this strategy of asking about change as a subtle, all-inclusive intervention for boosting others' wellbeing until more research is conducted on the parameters of the basic effect. To this point, below we outline directions for research that we believe may provide the most valuable extensions.

Future Directions

The present research affords diverse avenues for follow-up research. One avenue could further explore parameters beyond those explored here (population, time frame, category of life events, and target). For example, other methodologies could continue to extend generalizability. If people more broadly interpret change as progress, they might exhibit important (yet tested) biases in terms of basic implicit associations for "change = good," as well as overgeneralized preferences for any kind of change over a lack of it (e.g., being too quick to trade players from season to season). This possibility is hinted at by one recent IAT experiment in which some participants (political liberals) showed an automatic evaluative preference for primes related to flexibility versus rigidity (Jost, Nosek, & Gosling, 2008). This research could also further disentangle objective change from recollections of this change. While Study 2 suggests the basic effect does *not* reflect the possibility that people objectively improve more often or to a greater extent than they decline (thus explaining why people recall more improvement), still other designs could prove insightful. For example, researchers might explore bidirectional effects to explicitly equalize the objective amount of change; reflecting on one's academic progress from B-level work to A-level work might be coded as a bigger change than going from A-level work to B-level work, to the extent that people also associate "good = change" in addition to the currently documented perception of "change = good." On the other hand, researchers might test many interesting *differences* in natural interpretations of change in addition to these expansions of the basic effect, such as across cultures (e.g., Easterners could infer more heterogeneous definitions; Ji, Nisbett, & Su, 2001), tense (e.g., not only recalling past change but also predicting future change; O'Brien, 2015), and individual differences (e.g., narcissists may show a stronger effect than currently observed, while people with negative self-views might show the opposite effect). The presence of other diagnostic cues must also matter; if grandpa laments how his health or how society has changed, or if a relationship partner angrily yells about how the spark has changed, the recipient on the other end presumably will not infer a proclamation of improvement.

Another avenue could shed light on broader implications. A bias of associating change with positivity may appear contradictory to anecdotal preferences for stability, routine, and predictability in life, whereby change feels disruptive and alarming. Indeed, some other work finds that reflecting on connectedness and coherence can also enhance wellbeing (Bartels & Rips, 2010; Heintzelman, Trent, & King, 2013; Hershfield, 2011). Future studies could

fruitfully explore how these sets of findings relate to one another and the boundary conditions that moderate when people benefit from change versus stability. For example, perceptions of purpose could be critical: both change and stability may help so long as things do not feel random. There may also be differences across domains, such that reflecting on change could boost moods and emotions (as in Study 4) but reflecting on stability may better motivate goal pursuit, self-control, and other agentic pursuits.

Finally, future work should continue to tease apart the various mechanisms that could predict the basic effect observed here. At the broadest level, we have argued that people's propensity to selectively evaluate their temporal trajectories in a favorable light leads the general concept of change to evoke only the ways in which they have improved. In naturalistic settings, conversational norms (i.e., inferences of appropriateness such that if a person asks about my change, I assume he or she means improvement) and objective differences (i.e., improvement may be uniquely salient in some way) might amplify this basic effect—that people will respond only with how they have changed for the better. However, various aspects of the current studies do help isolate and highlight the role of self-enhancement motives beyond these other factors: we adopted diverse methods and measures across studies and found convergent results; designs were private, confidential, and used prompts making clear that change comprises other trajectories like decline; and some designs directly countered these alternative mechanisms (e.g., Studies 4 and 5).

Nonetheless, one remaining question *about* this self-enhancement component relates to whether it depends on motivated processing at the time of judgment or whether it reflects prior motivational processing. In everyday life, the motivation to self-enhance may lead people to reflect on improvement more often than on decline, which may then render episodes of improvement perennially on one's mind. Conversely, motivational processing might need to be active at the time of judgment. Self-affirmation interventions that subsequently reduce people's desire to self-enhance (Taylor & Sherman, 2008) could prove insightful here. If self-affirmed participants still subsequently interpret change as uniformly positive, this seems to support the former possibility; a subsequent attenuation of our effect seems to support the latter. Future research could fruitfully explore this idea.

Concluding Thoughts

When people look back to the past, they are reminded of how their lives have inevitably ebbed and flowed over time. Researchers have traditionally addressed this issue by studying the *process* of perceiving change, such as the role of attention, storage, beliefs, and expectations (Pashler, 1988; Rensink, 2002; Ross, 1989). The current article sought to complement and extend this impressive literature by examining the *content* of what actually comes to mind when doing so. In principle, the concept of change simply comprises the ways we have grown different from who we had been before, for better and for worse. However, in practice, it instead distinctly reminds us just how much we have grown.

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