



Pseudo-set framing

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RUNNING HEAD: Pseudo-Set Framing

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Abstract

Pseudo-set framing—arbitrarily grouping items or tasks together as part of an apparent “set”—motivates people to reach perceived completion points. Pseudo-set framing changes gambling choices (Study 1), effort (Studies 2 and 3), giving behavior (Field Data and Study 4), and purchase decisions (Study 5). These effects persist in the absence of any reward, when a cost must be incurred, and after participants are explicitly informed of the arbitrariness of the set. Drawing on Gestalt psychology, we develop a conceptual account that predicts what will—and will not—act as a pseudo-set, and defines the psychological process through which these pseudo-sets affect behavior: over and above typical reference points, pseudo-set framing alters perceptions of (in)completeness, making intermediate progress seem less complete. In turn, these feelings of incompleteness motivate people to persist until the pseudo-set has been fulfilled.

Max: 250 words

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Suppose you are asked to participate in a charitable service project by writing greeting cards to isolated senior citizens. You can complete as many or as few cards as you would like. How many would you write? And how would you determine when to stop? Perhaps you would write a card, maybe two, and finish when your output feels “good enough.”

Now imagine learning that the greeting cards would be arbitrarily batched in sets of four, or discovering that your card-writing progress would be tracked as part of a four-slice pie chart that “filled in” as you went. How might this change your stopping point?

Drawing on research in Gestalt psychology about the unique properties of whole units, we suggest that these arbitrary “sets”—or *pseudo-sets*—make people more likely to complete exactly four cards, even without any explicit reason for their presence or size. (Similarly, batches or pie charts with five or six component parts would make people more likely to complete exactly five or six cards, respectively.) We call this *pseudo-set framing*: arbitrarily grouping discrete items or tasks together as part of a cohesive set or group via visual cues and/or written descriptions. We show that even without providing an explicit target or goal, pseudo-set framing creates the notion of a group or set, which alters people’s perceptions of and activates their desire for completeness—even when such completeness entails additional effort with no additional reward.

We situate our conceptual account in Gestalt psychology, which explores the unique properties of whole units (e.g., Koffka, 1922) and suggests that people perceive sets to be “richly symbolic...[and] imbued with meaning” (Köhler, 1947, p. 139). Indeed, contemporary research has demonstrated that cohesive units or groups exert influence on perception (Ariely, 2001; Campbell, 1958; Corbett, 2016; Dasgupta, Banaji, & Abselson, 1999; Hamilton & Sherman, 1996; Veryzer & Hutchinson, 1998; Whitney, Haberman, & Sweeny, 2014), memory

(Feigenson, 2011), visual search (Beck & Palmer, 2002; Treisman, 1982) and choice (Evers, Inbar, & Zeelenberg, 2014; Smith, Faro, & Burson, 2013). For example, consumers choose products according to how well they fit within an existing set (Evers et al., 2014), and donors give more money to cohesive sets of victims (e.g., six disadvantaged children from one family) than to the equivalent number of discrete victims (e.g., six individual children; Smith et al., 2013). Further, a large body of qualitative research suggests that collectible sets—such as baseball cards or coins—signal the “right” number of goods to purchase, motivating a collect-them-all mentality (Belk, 1994; Carey, 2008; Danet & Katriel, 1994; McIntosh & Schmeichel, 2004; Stewart, 1993).

Why might sets—even arbitrarily constructed pseudo-sets—elicit such behavior? One possibility is that whole units or sets implicitly function as reference points, which related research has shown to influence people’s judgments and shape their behavior (e.g., Abeler et al., 2011; Cervone & Peake, 1986; Heath et al., 1999; Medvec & Savitsky, 1997; Rosch, 1975). Little is known however, about two questions of central importance: What precisely constitutes a reference point? And via what psychological processes do reference points affect behavior? The answers to these questions may be as vast and varied as reference points themselves; indeed, perhaps this is why scant theoretical progress has been made in answering them (Abeler et al., 2011). Although some research has identified conditions under which reference points exert stronger versus weaker influence (Simmons, Leboeuf & Nelson, 2010; Strack & Mussweiler, 1997; Wilson et al., 1996), it has tended to focus on identifying interesting and important effects of reference points on behavior (e.g., Heath et al., 1999; Pope & Simonsohn, 2011), with the reference points typically being either stipulated (Kahneman, Knetsch, & Thaler, 1991; Kahneman & Tversky, 1979) or assumed (Campbell & Cochrane, 1999; Becker & Murphy,

1988; Ryder & Heale, 1973). Using Gestalt psychology, we develop theory generating testable predictions of (a) what will—and will not—act as a pseudo-set and (b) the psychological process through which pseudo-sets affect behavior. Moreover, we differentiate between reference points and pseudo-sets: not only do we show that pseudo-sets can exert a greater influence on behavior than other types of reference points, but we also show that their effectiveness is more likely to be driven by a distinct psychological process—perceived incompleteness.

First, what constitutes a pseudo-set? Early Gestalt research explored when and why people perceive multiple, discrete objects as cohesive units or larger wholes (Koffka, 1922; Köhler, 1947; Wertheimer, 1923). For example, depending on their placement, the same dots on a page may appear either as individual, unconnected items or as distinctly unified figures or groups. Wertheimer (1923) hypothesized that the difference is driven by the items' configurations, and extensive cognitive research has since confirmed the role and importance of several configural properties in human perception (for reviews, see Wagemans et al., 2012a; Wagemans et al., 2012b). For instance, identical, tightly-clustered dots appear more “group-like” than varied and disparate dots (e.g., Ben-Av & Sagi, 1995; Oyama, 1961), closed shapes (e.g., a self-enclosed full circle or square) appear more unitary than open ones (e.g., Kovacs & Julesz, 1993), and dots inside (versus outside) a common region, such as a bounded figure, are more likely to appear grouped (e.g., Palmer, 1992). Once this larger unit or whole is perceived, individual component items tend to recede in importance, as people allocate attention to the larger unit or group (Ariely, 2001; Haberman & Whitney, 2010; Kimchi, 1992; Navon, 1977; Pomerantz, Sager, & Stoeber, 1977; Wertheimer, 1923). Critically, these effects occur at a basic perceptual level, and do not require additional information about why the stimuli have been grouped: groups can be strictly arbitrary yet produce similar perceptual effects (Köhler, 1947).

Building on this research, we suggest that arbitrary pseudo-sets can be constructed simply by grouping disparate items or tasks to evoke unity and togetherness, without requiring any explanation for why the groups have been formed. Given Wertheimer's (1923) multifaceted criteria, there are many ways to represent a cohesive set, and "set-ness" is likely to be a continuous rather than dichotomous characteristic; configurations can have stronger (or weaker) grouping properties than others (Wagemans et al., 2012a). For the purposes of this investigation, we instantiate pseudo-sets visually by depicting a single partitioned figure (e.g., one 25-cent coin subdivided into fifths, one "five-slice" pie chart)—which scores high on the dimensions of proximity, closure, and common region—and/or verbally by describing a single unitary group (e.g., "kit" or "batch"). Importantly, we compare these pseudo-sets to controls that are segregated and discrete (e.g., five five-cent coins, five individual circles), and therefore should appear less "set-like" on the basis of the same criteria of proximity, closure, and common region, among others (Wagemans et al., 2012a; Wertheimer, 1923).

Second, through what process do pseudo-sets exert their influence? We suggest that after perceiving the larger unit (or pseudo-set), people will exhibit a preference for—and take action to achieve—completeness. We draw again on Gestalt research that posits a link between whole units and perceptions of completeness: once a larger whole is perceived, completeness (or incompleteness) becomes a salient attribute (Köhler, 1947). Indeed, to perceive an object as complete or incomplete, people must necessarily have a larger unit in mind; as Köhler (1947) reasoned, "The reference to larger wholes is implied in many terms...a place can appear as a 'hole' only inasmuch as it constitutes an interruption of a larger entity" (p. 204). He further noted that the terms "'complete' and 'incomplete' belong in this class, in that their meanings refer to specific experienced units in which these adjectives are alone applicable" (p. 205). Relatedly, we

suggest that when a larger entity (such as a pseudo-set) is considered, its completeness (or incompleteness) will be salient.

To illustrate, consider again the batch of greeting cards: we suggest that while writing three cards may feel complete, writing three cards out of a batch of four feels markedly less so. Or imagine seeing three full bottles of beer sitting on a table. Here, the most salient unit is the individual bottle, of which there are three whole items; nothing appears to be missing and the three beers, unopened, seem “complete.” Now imagine that the same three bottles of beer are situated within a four-pack bottle carrier, but with one spot conspicuously empty. In this context, the most salient unit is the larger whole—the four-pack container, or set of four beers—and it is noticeably incomplete. The introduction of the larger entity (i.e., the batch of cards or set of four beers) concurrently introduces the notion of completeness, making otherwise identical items and quantities appear incomplete.

Incompleteness, then, looms large (Zeigarnik, 1927). People inherently desire and strive for completeness and everyday examples of this abound: people delight in crossing off to-do list items and finishing tasks they have already begun (Ovsiankina, 1928). Symptoms of obsessive-compulsive disorder also point to this desire; one of the four factors of the Dimensional Obsessive-Compulsive Scale (Abramowitz et al., 2010) measures people’s need for things to be “just right” and “complete,” suggesting a preference for completeness is a fundamental element of human cognition that—when taken to an extreme—can have adverse consequences. Therefore, we suggest that pseudo-sets activate people’s desire for completeness by highlighting *incompleteness*: people will be motivated to complete sets that appear incomplete. A card-writer who has completed three cards may feel satisfied and quit the task, while a card-writer who has completed three cards of a batch of four may feel compelled to write one more note. Likewise, a

shopper who has three single beers in her cart may be entirely satisfied with the quantity of her purchase, while a shopper who has three single beers inside a four-pack carrier may be motivated to purchase just one more.

Overview

We propose that pseudo-set framing shapes behavior by (a) conveying the notion of a larger group or entity, which (b) increases perceptions of incompleteness, and in turn (c) activates people's desire for completeness. As a result, we argue that pseudo-set framing encourages people to engage in actions to complete exact sets of tasks or items, all without changing the tasks or incentives themselves. We hypothesize that when facing an ongoing series of options or tasks—writing greeting cards to lonely seniors, deciding how many beers to purchase at the supermarket, or determining how many tasks to complete in a lab experiment—people will systematically expend effort or resources to fulfill the set (e.g., one “batch” of cards, one four-pack of beer, one full “set” of survey questions). Further, we predict that when viewing one's progress relative to a pseudo-set, intermediate effort (e.g., “three beers of a four-pack”) would feel subjectively less complete relative to a control (e.g., “three beers”); in turn, this feeling of incompleteness would drive people to persist until pseudo-sets are fulfilled. Thus we offer both a novel explanation for why people complete sets generally, and a demonstration of how sets can be arbitrarily constructed to elicit desired behaviors.

In a field study and five lab studies, we examine the effect of pseudo-sets of varying sizes (i.e., sets of four, five, six, and seven component parts), with both repeated tasks (Studies 1-4) and one-time decisions (Field Data and Study 5), using series of tasks that are both finite (i.e., for which an actual completion point is known; Field Data and Studies 1, 2, and 5) and infinite (i.e., for which there is no actual completion point; Studies 3 and 4). We compare pseudo-set framing

(e.g., a pie chart with three of five slices filled) to a simple control (e.g., telling people they have completed 3 units) and to a stringent visual control, in which progress is depicted but does not appear as a unitary set (e.g., three of five discrete circles filled). We find that the effect of pseudo-set framing persists across all of these contexts, offering a novel and simple intervention that meaningfully changes people's propensity to complete tasks, take gambles, and make charitable donations.

Field Data

To test the effect of pseudo-set framing in the field, we collaborated with the Canadian Red Cross (CRC), a humanitarian non-profit organization with the mission of improving the lives of vulnerable people. The CRC implemented pseudo-set framing in their online 2016 Holiday Campaign. In all cases, donors were invited to give either a cash gift or a “symbolic gift,”—six tangible items (e.g., blankets or food) that would be given to those in need. We explored whether pseudo-set framing could increase the number of unique items donors selected from the gift array, suggestive of people's efforts to complete the full “set” of items.

Potential donors ($N = 7,117$) were randomly directed to one of three landing pages (see Appendix A for images of each of the three sites). On one web page, the CRC emphasized cash donations (*cash* condition). Donors ($N = 2,354$) could either select the tangible item(s) they wanted to contribute, or specify a monetary amount to give and have CRC select the items later. The cash option was prominently displayed; however, if donors chose a symbolic gift, they could select multiples of the same item (e.g., 5 blankets) and/or select one-to-many of each unique item (e.g., 1 blanket plus 2 hot meals, etc.).

On a second web page, the CRC emphasized gift donations (*gift* condition). Donors ($N = 2,374$) saw instructions and gift options that were identical to those in the cash condition;

however, the cash option was less prominent and the group was told that the more items they chose, the more impact they could have. The site also included an image of a globe, and after each new unique gift was added to the cart, a “location marker” appeared in the geographic region where the items would be donated (e.g., a cart with 1 blanket and 1 hot meal would have a location marker in Europe and North America; see Fig. 1). Donors could amass all six individual location markers by giving one of each item.

On a third web page, the CRC emphasized full sets of donations (*pseudo-set* condition). As in the gift condition, the cash option was less prominent and the group was told that more gifts would result in more impact. However, donors ($N = 2,389$) in this condition were encouraged to give one of each of the six items in order to complete a “Global Survival Kit”—a pseudo-set. Rather than location markers appearing as items were added to the cart, a bar around the globe “filled in” and donors saw text describing the percentage of the kit that had been fulfilled (e.g., a cart with 1 blanket and 1 hot meal was described as “30% full”; Fig. 1). Donors could fill the whole kit by donating all six items. (For logistical reasons, participants in the pseudo-set condition could only donate one of each item—for a maximum of 6 items—and not unlimited multiples of the same item as in the two control conditions.)

[*Insert Figure 1*]

The CRC recorded whether donors chose to give cash and/or symbolic gifts, and which gift items were chosen. Of those who chose to donate gifts, pseudo-set framing greatly increased the likelihood that donors chose a complete set of items (i.e., at least one of each of the six items): significantly more participants in the pseudo-set condition (21%, 95% CI = [.19, .23]) chose to donate all six items than participants in the cash condition (3%, 95% CI = [.02, .04]; $\chi^2(1, N=2261) = 147.89, p < .001$, Cramer’s $V = .26$) and gift condition (5%, 95% CI = [.04,

.06]; $\chi^2(1, N=2603) = 146.80, p < .001$, Cramer's $V = .24$; overall $\chi^2(2, N=3475) = 255.62, p < .001$, Cramer's $V = .27$; Fig. 2).

Donors were also more likely to choose cash donations (versus gift donations, or cash plus gift donations) in the cash condition, where the cash donation option was most salient (63%, 95% CI = [.61, .65]), than in the gift condition (49%, 95% CI = [.47, .51]; $\chi^2(1, N=4728) = 95.23, p < .001$, Cramer's $V = .14$) and pseudo-set condition (42%, 95% CI = [.40, .44]; $\chi^2(1, N=4743) = 211.57, p < .001$, Cramer's $V = .21$; overall $\chi^2(2, N=7117) = 218.84, p < .001$, Cramer's $V = .18$).

[Insert Figure 2]

While significant, these field data have limitations. For example, the description of the pseudo-set condition's "Global Survival Kit" was left intentionally vague, but donors may have inferred there was something special about a "kit," which—rather than a desire for completeness—influenced their behavior; to address this issue, our lab studies use pseudo-sets that are explicitly arbitrary. Further, the three conditions were not as tightly controlled as would be ideal; the remainder of our studies eliminate any additional differences between conditions, providing careful tests of the specific effect of pseudo-sets against a variety of other points of comparison. The field data therefore provide tentative though meaningful evidence for the impact of pseudo-set framing on real-world behavior: donating money.

Study 1

Study 1 demonstrates pseudo-set framing in the domain of gambling, examining whether participants are willing to incur a cost—in the form of accepting a "bad" gamble—merely in order to complete a pseudo-set. All participants had the opportunity to accrue a small monetary bonus by accepting up to four different gambles. The first three were "good bets"—gambles with

positive expected value and no chance of losing money—while the fourth was a “bad bet,” conferring an equal chance of winning a small amount of money or losing twice as much. As such, it would have been reasonable to expect most participants to accept the first three gambles but pass on the fourth.

However, we wanted to test whether a simple pseudo-set framing manipulation could increase the percent of participants who took all four gambles. In one condition (*control*), we displayed bonuses as multiple, discrete units (five five-cent nickels), while in the other (*pseudo-set*), we displayed bonuses as a single, cohesive “set” (fifths of a 25-cent quarter). Despite holding the completion point (i.e., all four gambles) and stakes (i.e., winning the maximum \$0.25) constant across the two conditions, we expected that pseudo-set framing would make people more motivated to “complete the set” by taking all four gambles.

Method

Participants. Participants ($N = 201$; 58% male; $M_{\text{age}} = 33.5$ years, $SD = 11.0$) completed an online survey via Amazon’s Mechanical Turk. Participants were paid a flat rate of \$0.25 for their participation and received a bonus payment of up to \$0.25 based on the gambles’ outcomes.

Procedure. Study 1 used a two-condition, between-subjects design. All participants learned they had the chance to win up to \$0.25, thereby doubling their participation compensation; the only difference between the two conditions was how accrued bonuses were depicted. In a control condition, participants’ bonuses were framed as multiple, discrete units (i.e., five nickels), while in the pseudo-set condition, participants’ bonuses were framed as component parts of a set (i.e., fifths of a quarter). In addition to the visual graphic, we also included “Accrued Bonus” and a numerical value of their winnings beneath the graphic to ensure that the monetary amount was evident.

Participants were first told that they would see four gambles and have the chance to accept or decline each one; if they declined, they would cash out of the study with their current earnings and forfeit additional gambles. Each gamble was then displayed sequentially on a separate screen:

Gamble 1: You have a 90% chance of winning \$0.05 and a 10% chance of winning \$0.00

Gamble 2: You have a 75% chance of winning \$0.10 and a 25% chance of winning \$0.00

Gamble 3: You have a 50% chance of winning \$0.05 and a 50% chance of winning \$0.00

Gamble 4: You have a 50% chance of winning \$0.05 and a 50% chance of losing \$0.10

After seeing the details of each gamble, participants chose between two options: “Yes, I would like to take the gamble” or “No, I would like to cash out,” making it clear that they would electronically receive whatever bonus they had thus far accumulated. Participants who selected “No, I would like to cash out” were taken to the end of the study. Participants who selected “Yes, I would like to take the gamble” advanced to a new screen to learn whether they had won or lost; there was a 10-second delay before participants read, “You have won Gamble #X. You have now accrued a \$X bonus.”

The first three gambles had positive expected value (\$0.045, \$0.075, and \$0.025, respectively) and no chance of incurring a loss. In contrast, the fourth gamble—the one that offered the opportunity to “complete the set”—had a negative expected value (-\$0.025) and the chance of losing money. To ensure that all participants faced Gamble 4 with the same accrued

bonus amount, we rigged Gambles 1 through 3 so that participants won each round; thus, all participants who took Gambles 1 through 3 would have accrued \$0.20 out of \$0.25 when encountering the risky fourth gamble.

Participants saw their accrued bonuses displayed both numerically and graphically; in the control condition, participants saw a collection of five nickels with blank circles depicting unfulfilled winnings, while participants in the pseudo-set condition saw fifths of a quarter (see Fig. 3). Importantly, all participants were paid electronically and knew that they could cash out at any point and receive the amount displayed numerically on their screens; in other words, the graphics were effectively inconsequential.

The dependent measure was the percent of participants who “completed” (or accepted) all four gambles. We also compared the average number of gambles taken between the two conditions. This and all subsequent experiments concluded with demographic questions. No data were excluded and we report all conditions and measures. (We provide full details of our data collection process in Appendix B.)

[Insert Figure 3]

Results

The primary dependent measure was the percent of participants who “completed the set.” Our results in this and all studies are intent-to-treat, meaning that we analyze and report full completion rates (e.g., did you take all four gambles?) rather than conditional completion rates (e.g., conditional on your seeing Gamble #4, did you take it?) because it eliminates selection bias and adds to an understanding of who persists longer. Overall, as predicted, significantly more participants in the pseudo-set condition accepted all four gambles (29%, 95% CI = [.20, .38]) than in the control condition (16%, 95% CI = [.09, .24]; $\chi^2(1, N=201) = 5.00, p = .025$, Cramer’s

$V = .16$). Correspondingly, the average number of gambles taken was also significantly higher in the pseudo-set condition ($M = 3.2$, $SD = .62$, 95% CI = [3.1, 3.3]) than in the control ($M = 2.9$, $SD = .73$, 95% CI = [2.7, 3.0]; $t(199) = -3.43$, $p = .001$, $d = .49$; Fig. 4). In other words, fewer participants in the control condition even encountered Gamble #4, having cashed out of the study before reaching the last “bad bet.” When assessing those who both encountered and took Gamble #4 (Control = 72% vs. Pseudo-Set = 88%), the difference was similar but smaller (Control = 23%, 95% CI = [.13, .32] vs. Pseudo-Set = 33%, 95% CI = [.23, .43]; $\chi^2(1, N=161) = 2.27$, $p = .132$, Cramer’s $V = .12$).

These results suggest that there is something unique about cohesive yet arbitrary “sets”: despite encountering identical gambles, accrued bonuses, and total potential winnings, participants in the pseudo-set condition made different choices than those in the control. People in both conditions had the chance to win a bonus of \$0.25, but completing the set—or filling in one full quarter—prompted more participants in the pseudo-set condition to take one last (bad) gamble.

[Insert Figure 4]

Study 2

Study 2 again examined the effect of pseudo-set framing on completion, this time using a task completion paradigm and an even more arbitrary depiction of a pseudo-set. All participants were told there were 50 total tasks and that they could complete as many or as few tasks as they wanted and still be paid for their participation. As in Study 1, the only difference between the conditions was a simple and inconsequential framing manipulation designed to change participants’ subjective feeling of completion. Specifically, participants in the *pseudo-set* condition were shown a pie chart with five component parts and told that one “slice” would fill

after every ten tasks they completed. We compared this to a *visual control* condition, in which participants' progress was visually depicted but did not appear as a cohesive set (i.e., five discrete circles; Fig. 5). We also included a third *control* condition that did not provide any visual depictions of progress but measured baseline completion rates.

All participants knew the study's actual completion point was 50 questions; therefore, if people were inherently motivated to reach a complete stopping point, everyone could have done all 50 tasks. However, we predicted that pseudo-set framing would affect how many people actually did so. Our expectation was that, as in Study 1, participants in the pseudo-set condition would be more likely to complete the set by finishing all 50 tasks—for no reward other than the subjective feeling of completion.

Method

Participants. Study 2 was a three-condition, between-subjects design. Participants ($N = 336$; 57% male; $M_{\text{age}} = 33.2$ years, $SD = 12.5$) completed an online survey via Amazon's Mechanical Turk and were paid a flat rate for participation.

Procedure. All participants were informed that they would see 50 simple words (e.g., “Village”) and be asked to choose the correct definition from four multiple-choice options (e.g., “Penny / Small Town / Handgun / Lamb”). The stimuli were taken from *freerice.com*—a UN World Food Programme website that gives people the chance to answer an ongoing, repetitive stream of basic trivia questions in exchange for rice donations—and were categorized as the easiest level of English vocabulary questions. (Unlike *freerice.com*, the participants in our study were not answering questions for charity.)

All participants were told: “There are 50 total questions. You may answer as few or as many as you like; there is no penalty for skipping to the end. To skip to the end of the section

and end the vocabulary questions, click the ‘Skip to the end of this section’ button at the bottom of your screen.” Each vocabulary question was presented on its own screen, and the “Skip to the End” button appeared at the bottom of every page. The question number was displayed next to each question, so participants always knew how far into the task they had advanced.

To manipulate framing, we randomly assigned participants to one of three conditions: control, visual control, and pseudo-set. In the control condition, participants only saw the task number they were currently completing; thus, the tasks appeared as 50 repetitive questions, unrelated and discrete. In the visual control condition, participants saw the task number and a graphic with five discrete circles; they were told that one circle would fill after every ten questions (or one-fifth of the total questions) were answered. In the pseudo-set condition, participants saw the task number and a pie chart graphic with five component slices; they were also told that one slice would fill after every ten questions were answered (Fig. 5).

[Insert Figure 5]

Our primary dependent measure was the percent of participants who voluntarily completed all 50 vocabulary tasks. We also compared the completion patterns and average number of tasks completed across the three conditions.

Pretests. In order to ensure that the pseudo-set graphic appeared significantly more unitary than the visual control, we conducted two pretests. In the first, we showed an additional sample of online participants ($N = 145$; 59% male; $M_{\text{age}} = 36.7$ years, $SD = 11.4$) both the visual control and pseudo-set graphics from Study 2, and asked: “How many units or things would you say are depicted in the figure below?” Participants responded via an open-ended textbox and could reply in whatever format they wanted. (All but one participant provided a single numerical answer.) While virtually no participants reported that the visual control graphic depicted one

single unit (0.7%, 95% CI = [-.01, .02]), many participants reported that the pseudo-set graphic did (39%, 95% CI = [.31, .47]), McNemar Test: $p < .001$, Cramer's $V = .11$). Conversely, most participants (94%, 95% CI = [.91, .98]) answered that there were five units in the visual control condition, while far fewer gave that answer for the pseudo-set (48%, 95% CI = [.39, .56]; McNemar Test: $p < .001$, Cramer's $V = .17$). Other participants either reported there were six units (Pseudo-Set = 11%, 95% CI = [.06, .16] vs. Visual Control = 4%, 95% CI = [.01, .07]; McNemar Test: $p = .013$, Cramer's $V = .37$) or provided a different answer (Pseudo-Set = 3%, 95% CI = [.00, .05] vs. Visual Control = 0.7%, 95% CI = [-.01, .02]; McNemar Test: $p = .375$, Cramer's $V = .01$).

In a second pretest, we asked another sample of online participants ($N = 412$; 53% male; $M_{\text{age}} = 37.5$ years, $SD = 11.9$) to assess “the extent to which the graphic seems like individual items or a group” using a scale of individual versus group construal (adapted from Bartels & Burnett, 2011). Between-subjects, participants either encountered the visual control or pseudo-set graphic, and were told that “a rating of -3 means that they are individual units with distinct forms; a rating of +3 means that they are a tight group with a single form; a rating of 0 means that they are individuals and a group to equal degrees.” Participants rated the pseudo-set as appearing more group-like ($M = 1.3$, $SD = 1.5$, 95% CI = [1.1, 1.5]) than participants in the visual control ($M = 0.8$, $SD = 1.5$, 95% CI = [.6, 1.0]; $t(410) = 3.16$, $p = .002$, $d = .31$). Together, these results suggest that the pseudo-set indeed appeared more unitary than the visual control.

Results

Our primary dependent measure was whether participants completed the task set. A chi-square test revealed a significant effect of framing on participants' propensity to complete the set ($\chi^2(2, N = 336) = 12.69$, $p = .002$, Cramer's $V = .19$). As predicted, more participants in the

pseudo-set condition completed all 50 vocabulary questions—their “pseudo-set” (71%, 95% CI = [.63, .80])—than did participants in the visual control (57%, 95% CI = [.48, .67]; $\chi^2(1, N = 224) = 4.98, p = .026$, Cramer’s $V = .15$) and control conditions (48%, 95% CI = [.39, .58]; $\chi^2(1, N = 224) = 12.56, p < .001$, Cramer’s $V = .24$). Completion rates were statistically equivalent between the two control conditions ($p = .181$, Cramer’s $V = .09$).

The number of tasks completed varied across the three conditions, and a Kruskal-Wallis H test confirmed that the distributions were significantly different ($\chi^2(2) = 12.95, p = .002$; Fig. 6). Specifically, the average number of completed tasks varied significantly between the three groups ($F(2, 335) = 6.52, p = .002, \eta^2 = .04$). Pseudo-set participants completed significantly more tasks on average ($M = 39.9, SD = 17.2, 95\% \text{ CI} = [36.6, 43.1]$) than both control participants ($M = 30.8, SD = 20.2, 95\% \text{ CI} = [27.0, 34.6]$; $t(222) = -3.60, p < .001, d = .48$) and visual control participants ($M = 35.0, SD = 18.7, 95\% \text{ CI} = [31.5, 38.5]$; $t(222) = -2.04, p = .042, d = .27$). The number of tasks completed did not differ between the two control conditions ($t(222) = -1.59, p = .114, d = .21$).

[Insert Figure 6]

Study 2 shows that people will exert incremental effort to complete a full pseudo-set, further demonstrating the unique properties of cohesive sets relative to other visual demonstrations of progress. Taken together, Studies 1 and 2 provide converging evidence of the effect of pseudo-set framing on people’s desire to complete sets. Even holding actual completion points constant across conditions in both studies (i.e., four gambles or 50 tasks), people became especially motivated to reach a complete stopping point when they encountered pseudo-set framing—even when it entailed additional risk (Study 1) or additional unpaid work (Study 2).

Finally, we note that both the visual control and pseudo-set conditions could have visually conveyed the same experimenter expectation (i.e., complete 50 questions and fill the entire graphic) but resulted in a different pattern of results, offering some evidence that mere experimenter demand is unlikely to account for the full pattern of results.

Study 3

In Study 3, we test whether pseudo-set framing is effective even when the random size and arbitrary presence of the pseudo-set is made explicit—reducing the likelihood that participants assume that there is some inherent meaning behind the size of the pseudo-set. At the beginning of the study, all participants were informed that as they were doing a series of tasks, they would either see their task completion status indicated by a four-slice pie chart (*four-unit pseudo-set*), a seven-slice pie chart (*seven-unit pseudo-set*), or would see no visual indication of progress. Importantly, we conveyed that these visuals were inconsequential, noting that they have “no bearing on your compensation.” After learning of the three possibilities, participants were informed of their condition assignment. Despite being informed the pseudo-set graphic was randomly assigned and unrelated to their compensation, we anticipated that pseudo-set participants would complete their full “sets” at non-chance levels—in other words, four-unit participants would be most likely to complete multiples of 20 tasks (i.e., their full “pseudo-set”) and seven-unit participants would be most likely to complete multiples of 35 tasks. Additionally, Study 3 used an unlimited (rather than finite) series of tasks to investigate the effect of pseudo-set framing when there was no actual complete stopping point.

Method

Participants. Participants ($N = 362$; 62% male; $M_{\text{age}} = 32.6$ years, $SD = 9.7$) completed an online survey via Amazon’s Mechanical Turk and were paid a flat rate for participation.

Procedure. Study 3 was a three-condition, between-subjects design. All participants were informed that they would be solving CAPTCHA verifications (i.e., visual perception tests requiring people to decode obscure words and numbers). The instructions specified: “There are an unlimited number of CAPTCHA tasks, but you may solve as many or as few as you’d like; there is no penalty for skipping to the end. You will be paid a flat rate of \$0.30 no matter what.”

To unambiguously communicate that pseudo-sets are arbitrary and meaningless, the instructions further explained: “All of the CAPTCHAs are numbered. However, to help track your progress, some of you will see graphics that depict how many you have solved. This will be determined randomly and has no bearing on your compensation. If you see a progress graphic, one slice will fill after every 5 CAPTCHAs that are solved. The two progress indicators are as follows.” Below these instructions, participants saw two pie charts: one with four component parts and one with seven component parts.

After reading the instructions, participants advanced to the next screen and learned how their progress would be tracked. Participants in the *control* condition read: “You will not see a progress graphic, but you will be able to see which CAPTCHA number you are completing on each screen.” Participants in the *four-unit* and *seven-unit* pseudo-set conditions saw the appropriate progress graphic and read: “Your progress will be tracked with the below graphic. For every 5 CAPTCHAs you complete, one of these slices will fill.” The implication was that four-unit participants needed to complete 20 CAPTCHAs to fulfill a set, while seven-unit participants needed to complete 35 CAPTCHAs to fulfill a set. All participants had an option to “Skip to the end of the section” at the bottom of every page and could complete as many or few CAPTCHAs as they wanted. They could not advance until the CAPTCHA had been correctly solved.

Our primary dependent measure was the percent of participants who voluntarily completed a “pseudo-set”—exact multiples of 20 CAPTCHAs (i.e., a complete set for four-unit participants) or exact multiples of 35 CAPTCHAs (i.e., a complete set for seven-unit participants)—to investigate whether pseudo-sets changed people’s stopping points at beyond chance rates, even when the sizes of the pseudo-sets were explicitly arbitrary. We also compared the completion patterns and average number of tasks completed across the three conditions.

Pretest. We conducted a pretest to ensure that participants fully understood the instructions and did not mistakenly believe they needed to complete the pseudo-set graphics (i.e., experimenter demand). A separate sample of online participants ($N = 152$; 50% male; $M_{\text{age}} = 37.4$ years, $SD = 12.2$) was randomized into three groups and saw the exact instructions (by condition) that were shown to participants in Study 3. However, rather than actually completing CAPTCHA tasks, they were instead asked: “How many CAPTCHAs do you need to complete in order to get paid?” Participants responded in an open-ended textbox and could reply in whatever format they wanted. Most—75% (95% CI = [.67, .82])—gave the correct response (either the number 0 or some variation of “as many as I want”). Importantly, this did not vary by condition: those in the control condition (for which no graphic was displayed) had the same rate of correct answers (78%, 95% CI = [.63, .88]) as those in the pseudo-set four condition (73%, 95% CI = [.58, .84]) and those in the pseudo-set seven condition (75%, 95% CI = [.61, .86]). Of those who answered *incorrectly*, almost all (76% of the incorrect replies; 19% of full sample) replied “5,” which was neither correct, nor did it entail completion of the pie chart; this again did not vary by condition. Only two participants (1.3%, 95% CI = [.00, .05]) gave a number suggesting they believed they needed to complete the set to be paid. These results suggest that participants

understood the instructions as intended, such that completion rates in Study 3 are not artefacts of experimenter demand (i.e., a fear of not getting paid).

Results

The primary dependent measure of the main study was dichotomous: whether or not participants completed a full pseudo-set, as specified by the graphic they encountered (i.e., an exact multiple of 20 or 35). Despite full knowledge of the graphic's arbitrariness, pseudo-set participants nonetheless completed full sets at non-chance levels: participants in the four-unit pseudo-set condition were significantly more likely to complete exact multiples of 20 tasks (20%, 95% CI = [.13, .27]) than participants in the seven-unit pseudo-set condition (4%, 95% CI = [.01, .08]; $\chi^2(1, N=240) = 13.80, p < .001$, Cramer's $V = .24$) or control condition (3%, 95% CI = [.00, .06]; $\chi^2(1, N=243) = 16.33, p < .001$, Cramer's $V = .26$; overall: $\chi^2(2, N=362) = 25.27, p < .001$, Cramer's $V = .26$). Likewise, participants in the seven-unit pseudo-set condition were significantly more likely to complete exact multiples of 35 tasks (12%; 95% CI = [.06, .18]); no one completed exact multiples of 35 in the four-unit pseudo-set condition (0%; $\chi^2(1, N=240) = 15.12, p < .001$, Cramer's $V = .25$) or control condition (0%, $\chi^2(1, N=241) = 15.24, p < .001$, Cramer's $V = .25$; overall: $\chi^2(2, N=362) = 29.74, p < .001$, Cramer's $V = .29$). Further, Figure 7 depicts the percent of participants that completed each of the tasks; although a Kruskal-Wallis H test showed that the distributions were not significantly different ($\chi^2(2) = 1.64, p = .441$), the chart reveals obvious and predicted attrition immediately following full pseudo-set completion at 20 and 40 (four-unit condition) and 35 (seven-unit condition).

Pseudo-set framing also changed the average number of tasks participants completed ($F(2, 361) = 3.04, p = .049, \eta^2 = .02$): participants in the control condition completed fewer tasks ($M = 10.1, SD = 12.7, 95\% \text{ CI} = [7.9, 12.4]$) than in both the four-unit ($M = 14.6, SD = 16.9,$

95% CI = [11.6, 17.7]; $t(241) = -2.36, p = .019, d = .30$) and seven-unit conditions ($M = 14.9, SD = 20.2, 95\% \text{ CI} = [11.2, 18.5]; t(239) = -2.18, p = .030, d = .28$). There was no difference in the number of tasks completed between the two pseudo-set conditions ($p = .932, d = .01$).

[Insert Figure 7]

Like Studies 1 and 2, Study 3 demonstrates that people will exert effort to complete a pseudo-set. Adding to the previous results, Study 3 shows that this happens even when people are explicitly informed of the utter arbitrariness of the pseudo-set's size and presence. Moreover, the pretest data suggest that the effectiveness of pseudo-sets is not driven by experimenter demand: despite the fact that participants were aware that they were not required to complete pseudo-sets, pseudo-set framing continued to influence behavior.

Study 4

Studies 2 and 3 showed that pseudo-set framing changed participants' effort on tedious and meaningless tasks; Study 4 tests the effect of pseudo-set framing on a task that is more inherently meaningful, and for which participants may have more established (i.e., less malleable) ideas of "how much is enough." Dovetailing with the Field Data results, Study 4 uses a tightly-controlled design to show how pseudo-set framing could be inexpensively deployed in real-world settings to impact consequential behaviors.

As highlighted in the opening example, Study 4 asked participants to write holiday cards to seniors in nursing homes; participants could write as many (or few) as they wished. In the control condition, participants simply saw a count of their completed cards; in the pseudo-set condition, participants learned there were four cards in every "batch"—a number we selected arbitrarily and never further explained—and their progress was framed accordingly (e.g., "You have completed 75% of one batch!"). We expected that participants in the pseudo-set condition

would be more likely to complete cards in multiples of four, since four cards comprised the “pseudo-set.” We also included exploratory items assessing possible mechanisms underlying pseudo-set framing: perceived impact, meaningfulness, and enjoyment.

Method

Participants. Participants ($N = 192$; 54% female; $M_{\text{age}} = 23.5$ years, $SD = 4.7$) completed the study at a laboratory at a U.S. university as part of a multi-study session conducted a few weeks before the December holidays. All participants were randomly assigned to a desk that was separated by a partition, such that participants could not readily observe others seated nearby. Participants were paid a flat rate for their participation in the session.

Procedure. Study 4 was a two-condition, between-subjects design. All participants learned they would contribute to a nonprofit’s holiday card-writing drive, which “facilitates the sending of holiday cards to seniors in nursing homes who are particularly isolated.” On every desk, there were 12 identical, non-denominational, blank holiday greeting cards, one pen, and large collection envelopes in which completed greeting cards were placed; if participants wanted to complete more cards than had initially been provided on their desk, they were told that an experimenter would bring more. All participants had to complete at least one card; after that, they could choose to write more cards or end the task. Everyone was given a 10-line greeting to handwrite inside, ensuring that all cards were identical and there was no variety in the card-writing experience. The actual writing task was exactly the same for all participants and there were no incentives for completing cards.

Participants in both conditions read: “Our plan is to collect as many cards as possible, and send roughly 200 cards to each nursing home. Each senior will receive one card.” Pseudo-set participants received one additional line of instructions: “Within each nursing home package, we

are batching cards in sets of four.” Participants tracked their progress by clicking a button on their computer after finishing each card. On their screens, control participants received a simple count of how many cards they had completed: “Thanks! You have completed ONE card. Would you like to write another? (Yes/No).” If participants answered “No,” they were taken to the end of the section. If participants selected “Yes,” they completed another card and then received an updated progress report: “Thanks! You have completed TWO cards. Would you like to write another?” This loop continued for as long as participants agreed to write cards.

Pseudo-set participants’ progress was framed in terms of the pseudo-set, both verbally and graphically: “Thanks! You have completed 25% of one batch of cards. Would you like to write another? (Yes/No).” We also included a visual indication of progress: a pie chart with four component parts representing one set. One slice filled in for each card a participant completed (see Fig. 8). Once a full set was completed, an additional pie chart appeared to the right with four new empty slices.

Our primary dependent measure was the percent of participants who completed a “set” of cards—or an exact multiple of four. We also compared the completion patterns and average number of tasks completed across the two conditions.

[Insert Figure 8]

At the end of the study, participants completed six exploratory items. Everyone rated how meaningful they thought each card would be to the recipient (1 = *not at all meaningful* to 7 = *very meaningful*), how much they enjoyed the service project (1 = *not at all* to 7 = *very much*), and how much they agreed (1 = *strongly disagree* to 7 = *strongly agree*) with four prosocial impact statements (adapted from Grant et al., 2007): *I feel that my donation makes a positive difference in other people’s lives; I am very aware of the ways in which my donation is*

benefiting others; I am very conscious of the positive impact that my donation has on others; My donation really makes others' lives better. We averaged the four prosocial impact items to create a composite measure of impact ($\alpha = .96$).

Results

Our primary dependent measure was dichotomous: whether or not participants completed full sets of cards (i.e., multiple of four). The introduction of pseudo-sets significantly changed participants' propensity to complete full sets; in the control condition, just 4% (95% CI = [.00, .08]) of control participants completed a multiple of four cards, while 40% (95% CI = [.30, .50]) of pseudo-set participants completed a multiple of four cards ($\chi^2(1, N = 192) = 34.87, p < .001$, Cramer's $V = .43$). As in Study 3, these results confirm that pseudo-set participants' completion of exact sets was not due to chance.

The number of tasks completed varied across the two conditions, and a Kolmogorov-Smirnov test confirmed that the two distributions were distinct ($Z = 1.87, p = .002$); Figure 9 shows a clear spike at four cards completed for the pseudo-set condition, which is not present for the control condition. We also compared the average number of cards completed by each condition. Directionally, participants in the pseudo-set condition completed more cards ($M = 2.8, SD = 2.1, 95\% CI = [2.4, 3.2]$) than in the control condition ($M = 2.5, SD = 2.5, 95\% CI = [2.0, 3.0]$), although this difference was not significant ($t(190) = -.81, p = .420, d = .12$). (No participants asked for more cards beyond the 12 that were initially provided.) Consistent with our instructions, boxes filled with holiday cards were shipped to nursing homes after the study ended.

[Insert Figure 9]

Pseudo-set framing did not affect participants' perceptions of impact: participants saw the cards they wrote as equivalently meaningful ($M_{\text{control}} = 5.1$, $SD = 1.5$, 95% CI = [4.8, 5.4] vs. $M_{\text{pseudo-set}} = 4.8$, $SD = 1.71$, 95% CI = [4.4, 5.1]; $t(190) = 1.65$, $p = .100$, $d = .24$) and impactful ($M_{\text{control}} = 4.9$, $SD = 1.5$, 95% CI = [4.6, 5.2] vs. $M_{\text{pseudo-set}} = 4.7$, $SD = 1.5$, 95% CI = [4.4, 5.0]; $t(190) = 1.17$, $p = .242$, $d = .17$), suggesting that a heightened sense of personal impact cannot explain our results. It is also consistent with the notion the arbitrary “batches” of four were truly perceived as such; participants in the pseudo-set condition completed more arbitrary batches, yet they did not perceive their impact to be any greater than those in the control condition. Control participants rated the task as more enjoyable relative to pseudo-set participants ($M_{\text{control}} = 5.4$, $SD = 1.5$, 95% CI = [5.1, 5.7] vs. $M_{\text{pseudo-set}} = 4.8$, $SD = 1.7$, 95% CI = [4.5, 5.2]; $t(190) = 2.62$, $p = .010$, $d = .38$), and number of cards completed was more highly correlated with enjoyment for control participants ($r = .30$, 95% CI = [.17, .41], $p = .004$) than pseudo-set participants ($r = .19$, 95% CI = [.00, .39], $p = .067$), suggesting that something other than mere enjoyment drove pseudo-set participants to continue writing cards. We more fully explore our proposed mechanism underlying pseudo-set framing—perceptions of completeness—in Study 5.

Study 5

Study 5 had two primary goals. First, we wanted to demonstrate how the psychology of pseudo-sets differs from the psychology of other types of reference points studied in previous research (e.g., simply showing people the number 25). Second and relatedly, we wanted to document the mechanism by which pseudo-sets (e.g., displaying a pie chart that fills in after 25 tasks) influence behavior: perceived incompleteness. We propose that while pseudo-sets do serve as a point of reference in that they cue a certain behavior, they operate via a distinct psychological process: in contrast to other reference points, pseudo-sets are more likely to

increase perceptions of incompleteness, which in turn activate people's desire for completeness and prompt them to finish full sets.

To test this hypothesis, we used a common real-world scenario in which people regularly encounter pseudo-sets: product packaging. Beers are typically grouped into six-packs, such that (as we will show below) people often use six beers as a reference number when purchasing. While these packs are often prefilled (i.e., customers have no choice but to buy the full set of six), some retailers (e.g., Whole Foods) allow consumers to fill the packs with "loose beers"—meaning that in theory, people can take a six-pack package and fill it with only five beers. Our pseudo-set framing hypothesis would predict that, even when people need not "complete the set" and fill the package entirely, they would view any pack that had empty slots as "incomplete"—motivating them to complete the set.

To cleanly test the effect of typical reference points against the effect of pseudo-sets, we reminded all participants of the "six-pack" reference point. In the reference point condition, we then told participants that they had already chosen a certain number of loose beers (one, two, three, or four) and we displayed an image of the bottles. In the pseudo-set condition, we also told participants that they had already chosen a certain number of loose beers (one, two, three, or four), but these beers were depicted within a "four-pack" container (see Fig. 10 for an example). All participants then indicated how many additional beers they would purchase. In both conditions, participants could add as many or few beers as they wanted and everyone was given the typical reference point of six beers; however, we expected that participants would purchase different quantities as a function of framing. Specifically, reference point participants would likely use the reference point of "6 beers" and add however many beers they needed to reach six,

while participants in the pseudo-set condition would add only the number of beers required to complete the new, arbitrarily-created set of four.

[Insert Figure 10]

(Note that this pattern of results cannot be explained simply by an aversion to carrying loose beers. One possibility is that people in the pseudo-set condition will purchase four so that they could have a container that held all of the beers they purchased; however, in the reference point condition, containers are not depicted and yet we expected participants to purchase six loose beers, suggesting that convenience of transport is not the critical factor.)

In addition to asking participants how many more beers they would add to their cart, we also asked them how complete or incomplete they perceived their existing purchase quantity to be (counterbalanced). This design allowed for crucial tests of our conceptual account. First, we directly compared the influence of a reference point (six beers) against a pseudo-set (complete “set” of four beers) to see which was most effective in shaping behavior: Were people more likely to buy six or four total beers? More importantly, however, the study allowed us to compare the process by which pseudo-sets versus mere reference points change behavior. Specifically, we predicted that pseudo-set framing would uniquely affect perceptions of (in)completeness, in turn driving the number of additional beers purchased.

Method

Participants. Study 5 was a 2 (framing: *reference point* vs. *pseudo-set*) by 4 (number of beers already in cart: 1, 2, 3, or 4) between-subjects design. Participants ($N = 802$; 46% male; $M_{\text{age}} = 35.6$ years, $SD = 11.8$) completed an online survey via Amazon’s Mechanical Turk and were paid a flat rate for participation.

Procedure. All participants read the same scenario: “Imagine you’re at the grocery store

and you decide to buy some beer. Your supermarket allows you to buy ‘a la carte’ beers; in other words, you can buy bottles of beer separately. Each beer is priced individually and there is no discount for volume. Beer is often sold in multiples of 6 (e.g., 6-packs) but you can get as many or as few as you’d like. The store offers optional carriers to hold the beer.” We then varied the number of beers participants had supposedly already purchased: “You have already placed the below [1 / 2 / 3 / 4] beer[s] into your cart.” Below this text, we included a graphic depicting the number of beers already purchased—either as single, loose beers (reference point condition) or as beers situated within a four-pack bottle carrier (pseudo-set condition; see Fig. 10).

Participants then responded to two measures: evaluate the perceived completeness of the initial purchase quantity (the original one, two, three, or four beers in the cart), which was our proposed mediator, and make a decision about how additional a la carte beers they would purchase, our dependent measure. (We counterbalanced the order in which participants saw these two questions to ensure that asking about perceived completeness did not influence subsequent choices.) To measure perceived completeness, participants were shown the beer graphic and asked: “How complete or incomplete does your current purchase (shown above) appear to you?” Completeness was measured on a sliding scale with endpoints labeled “*Appears very incomplete*” and “*Appears very complete.*” Actual scale values were hidden, but responses were recorded as a number between 0 and 100. To collect purchase decisions, participants were asked: “At this point, how many (if any) more beers would you decide to buy?” Their choices were 0, 1, 2, 3, 4, 5, or 6+.

Pretest. We first ensured that pseudo-sets changed perceptions of completeness. We asked a separate sample of Mechanical Turk participants ($N = 273$; 50% male; $M_{\text{age}} = 37.3$ years, $SD = 11.3$) to evaluate perceived completeness without making any corresponding purchase

choices. In one condition (*reference point*), participants saw a graphic with five individual beer bottles; in the other condition (*pseudo-set*), participants saw a graphic with five beer bottles situated within a six-pack container. All participants were asked: “How complete or incomplete do these five beers appear to you?” Completeness was measured on a sliding scale with endpoints labeled “*Appears very incomplete*” and “*Appears very complete*.” Actual scale values were hidden, but responses were recorded as a number between 0 and 100. Confirming our hypothesis that pseudo-sets alter perceptions of completeness—and that “unfinished” pseudo-sets appear uniquely incomplete—participants in the pseudo-set condition thought the same five beers appeared far less complete ($M = 39.7$, $SD = 31.3$, 95% CI = [34.4, 45.0]) than participants in the reference point condition ($M = 64.1$, $SD = 29.8$, 95% CI = [59.1, 69.2]; $t(271) = 6.60$, $p < .001$, $d = .80$).

Results

First, we confirmed that there was no effect of order for either of the two dependent variables (for number of beers added to cart, $p = .178$; for perceived completeness, $p = .282$); all results held regardless of whether participants chose first and then evaluated completeness or evaluated completeness first and then chose. Therefore, we collapse across this factor when reporting all results.

Recall that participants in both conditions were given the same reference point of six beers in their instructions: “Beer is often sold in multiples of 6 (e.g., 6-packs).” We first confirmed that, in the reference point condition, participants indeed used six beers as their reference point: after computing the total beers purchased (number of beers already in cart + number of additional beers selected), we found that six was the modal number of beers purchased among these participants. In contrast, despite the fact that participants in the pseudo-

set condition were given the same six-beer reference point, the modal number of beers purchased among pseudo-set participants was four—the number needed to complete the pseudo-set. A Kolmogorov-Smirnov test confirmed that the two distributions were significantly different ($Z = 3.15, p < .001$). Although pseudo-set participants encountered both the four-pack pseudo-set and six-pack reference point, completing the four-pack pseudo-set seemed to exert more influence.

A final way to view the differential impact of reference points and pseudo-sets is to examine the number of additional beers purchased by condition (Fig. 11). In the reference point condition, at each “number of beers already in the cart,” the most popular response was to buy the number that would result in six total beers (gray bars); in contrast, in the pseudo-set conditions, the most popular response at each “number of beers already in the cart” was to buy the number that would result in four total beers (black bars). In aggregate, a Kolmogorov-Smirnov test confirmed that the two distributions again differed significantly ($Z = 1.47, p = .027$). These results suggest that pseudo-sets may exert greater influence on behavior than common reference points.

[Insert Figure 11]

We next examined how the two interventions differ in the manner in which they influence behavior. To explore the underlying mechanism, we analyzed the role of perceived completeness using a moderated mediation model. We predicted that perceived completeness would mediate the relationship between the number of beers already in the cart and the number of additional beers purchased, but that this would be especially true for people in the pseudo-set versus the reference point condition.

We first tested whether framing influenced both the dependent variable and the proposed mediator. For the dependent measure, a linear regression revealed that framing influenced the

relationship between number of beers already in cart and number of additional beers people wanted to purchase: there was a significant effect of number of beers already in cart ($B = -.40$, $SE = .07$, 95% CI = $[-.54, -.26]$, $p < .001$), a nonsignificant effect of framing ($B = .27$, $SE = .29$, 95% CI = $[-.29, .83]$, $p = .341$), and importantly, a significant interaction between the two ($B = -.21$, $SE = .10$, 95% CI = $[-.42, .00]$, $p = .045$).

For the proposed mediator, a linear regression showed that framing also influenced the relationship between the number of beers already in cart and perceived completeness: there was a significant effect of number of beers already in cart ($B = 7.14$, $SE = 1.36$, 95% CI = $[4.46, 9.81]$, $p < .001$), a significant effect of framing ($B = -27.95$, $SE = 5.29$, 95% CI = $[-38.33, -17.57]$, $p < .001$), and importantly, a significant interaction between the two ($B = 8.60$, $SE = 1.93$, 95% CI = $[4.80, 12.39]$, $p < .001$; Fig. 12). Even for the same number of beers, pseudo-set participants perceived the quantity as less complete than reference point participants—except once the “set” (four beers) was completed, at which point the pattern reversed.

[Insert Figure 12]

Finally, we used a moderated mediation analysis (Edwards & Lambert, 2007; Preacher, Rucker, & Hayes, 2007). As reported above, framing influenced both the dependent variable (number of beers to purchase) and the mediator (perceived completeness). Further, when number of beers purchased was regressed on framing, number of beers already in cart, their two-way interaction, and perceived completeness, the mediator was significant ($B = -.02$, $SE = .002$, 95% CI = $[-.026, -.019]$, $p < .001$), and the effect of the interaction between framing and number of beers already in cart was reduced to non-significance (from $B = -.21$, $SE = .10$, 95% CI = $[-.42, .00]$, $p = .045$ to $B = -.01$, $SE = .10$, 95% CI = $[-.20, .18]$, $p = .892$). A 10,000 sample bootstrap analysis showed that the 95% bias-corrected confidence interval for the size of the indirect effect

excluded zero (95% CI = [-.29, -.11]), suggesting a significant indirect effect. In addition, the effect size of the mediator was greater in the pseudo-set condition (-.36) than in the reference point condition (-.16), supporting our account that pseudo-set framing works by changing perceived completeness—to a greater extent than such perceptions influence behavior driven by reference points.

General Discussion

Five lab studies and a field study documented the impact of pseudo-set framing on behavior. The presence of arbitrary and inconsequential pseudo-sets altered the number of gambles taken (Study 1), tasks completed (Studies 2 and 3), greeting cards written (Study 4), and purchase quantities selected (Study 5). Although the exact stimuli differed between studies, the designs were conceptually analogous—five nickels vs. five-fifths of a quarter (Study 1), five small circles vs. five-fifths of a larger circle (Study 2), individual cards vs. one batch of cards (Study 4), individual beers vs. four-pack of beer (Study 5)—and provide converging evidence of the effect of pseudo-set framing. Pseudo-sets predictably changed behavior in both one-time decisions (Field Data and Study 5) and ongoing series of tasks (Studies 1-4) via perceptions of completeness (Study 5), encouraging people to reach a satisfying endpoint: the completion of the pseudo-set.

Our field study demonstrates just one simple and impactful example of how this framing can be used to encourage charitable behaviors in everyday life; we suggest that other real-world applications are abundant. People frequently encounter tasks with no obvious stopping point, prompting the question: “How much is enough?” How many products should we buy? How many times should we donate? How many tedious questions should we voluntarily complete as lab participants? Firms, fundraisers, and researchers typically prefer more purchases, donations,

and data, but lacking a desirable stopping point—and absent incentives for completion—people may feel their efforts are “complete enough” and quit prematurely. In other cases, the preference is for people to complete only a certain number of tasks: think of parents who want their child to eat only six pieces of Halloween candy at a sitting, or a gym that hopes customers come at least twice a week (so they use the gym enough to renew their membership) but not more (so the gym is not overcrowded). In these contexts, pseudo-set framing may change engagement and attrition patterns in meaningful, predictable, and malleable ways.

We have suggested that pseudo-sets work even when the essence of the set itself is entirely meaningless, highlighted in the random pseudo-set design of Study 3 (and the pretest suggesting that we were not communicating implicit experimental demand). However, much as we tried to strip away meaning and significance from the pseudo-sets themselves (e.g., a pie chart that arbitrarily appeared or an unexplained “batch” of cards that could be written), even the most arbitrary pseudo-sets likely convey some level of meaning—albeit symbolic, not practical. As Köhler (1947) argued, cohesive units are perceived as “richly symbolic” and “imbued with meaning” just by the very nature of their existence (p. 139). Therefore, it is likely impossible to display a pseudo-set fully devoid of meaning; indeed, it is precisely because of this spontaneous attribution of meaning that pseudo-sets work. However, this kind of meaning differs critically from “actual” meaning—such as an explanation that “batching greeting cards in groups of four is important for shipping purposes” or “a full this pack of beer costs less than a partial one”—which could offer reasons, in and of themselves, for higher rates of completion. Pseudo-sets influence behavior despite lacking any practical meaning or consequential implications.

Phenomenologically, our findings contribute to the wide-ranging research on reference points in two ways. First, we offer a novel case that meaningfully changes people’s effort

expenditure—even when not accompanied by extrinsic or discontinuous rewards (Heath et al., 1999). A defining feature of pseudo-set framing—theoretically, and also in practice—is that it is substantively meaningless. Unlike other research on sets in which sets are inherently meaningful (e.g., a family; Smith et al., 2013), contain variety (e.g., baseball cards; Stewart, 1993) and/or come with a reward (e.g., completing a loyalty card; Kivetz, Urminsky, & Zheng 2006; Nunes & Drèze, 2006; completing a collectible set; Carey, 2008), pseudo-sets are effective in the absence of these factors. As a result, the size of pseudo-sets is usefully flexible: the parent hoping to encourage six pieces of candy eaten or the gym hoping for four visits per month can easily change the size of these sets to change the relevant behavior.

Second, although previous research has documented that arbitrary reference points affect behavior, the question of which psychological processes lead to behavior change has received less empirical attention. Drawing on Gestalt psychology, we present a conceptual and empirical account that suggests that pseudo-sets are formed when disparate items are grouped in a way that evokes togetherness, and exert their impact on behavior by activating people’s intrinsic desire for completeness. As such, we also contribute a novel empirical understanding of people’s inherent desire to complete things—regardless of what those things actually are. For instance, qualitative researchers have examined what motivates collectors to amass complete sets of items, and found that variety, inherent meaning, and value can explain this behavior (Belk, 1994; Danet & Katriel, 1994; McIntosh & Schmeichel, 2004; Stewart, 1993). However, even stripping those explanations away, our results indicate that once people think in terms of “sets,” they will be motivated to reach a satisfying and complete endpoint. Finally, we contribute to research suggesting that, in addition to conscious calculation of costs and benefits when choosing how

much effort to exert for some reward, people are also influenced by more subtle factors and unconscious processes (see Bijleveld, Custers, & Aarts, 2012a; 2012b).

While we have demonstrated pseudo-set framing across a variety of contexts, pseudo-sets are likely to be bounded in their effectiveness. For example, if pseudo-set completion is too difficult or simply impossible, pseudo-set framing may lose its appeal; we generally chose tasks that participants could complete given a moderate amount of effort. Relatedly, if the number of tasks required to fill a “pie piece” is prohibitively high, people may decide not to engage at all to avoid anticipated dissatisfaction with partial completion. Future research may investigate the boundary conditions—including the type and number of tasks required to make progress—under which pseudo-set framing is most effective and impactful. Additional research may also test different configurations or instantiations of pseudo-set framing and the role of perceptual grouping in motivating completion. Still, our results show pseudo-set framing is effective across a wide variety of tasks, increasing effort—even at mind-numbing tasks—and promoting both prosocial behavior and gambling.

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APPENDIX A—Field Data Stimuli

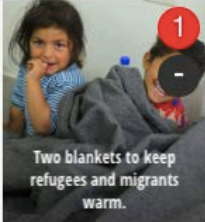
The below screenshots depict what a donor would encounter after adding 1 Blanket and 1 Hot Meal to the donation cart (from top: *cash* condition, *gift* condition, *pseudo-set* condition).

CASH CONDITION

Deliver gifts of survival that help others this holiday season


Simply select the items you want to send those in need:

INTERNATIONAL




Two blankets to keep refugees and migrants warm.

\$60



FOOD AND WATER RELIEF


\$60



MOTHER-CHILD HEALTH PARCEL


\$25

IN CANADA




TWO-DAY GROCERY SUPPLY

\$20



INFANT CARE PACKAGE

\$35




Three days of healthy meals delivered to someone in need.

\$60


Or choose your budget and we'll put together a gift box for the most urgent needs both at home and abroad.

SELECT AMOUNT:

\$75
\$150
\$250
\$500
\$ Other



All gifts are matched by BMO Bank of Montreal.
That means your impact is DOUBLED!



Your Donation
RESET

\$60

COMPLETE DONATION >

GIFT CONDITION

Deliver gifts of survival that help others this holiday season

Simply select the items you want to send those in need. The more gifts you choose, the more impact you can make!

These gifts go a long way! Vulnerable Canadians rely on hot meal deliveries every day.

INTERNATIONAL

 <p>\$15 BLANKETS: WARMTH FOR REFUGEES</p>	 <p>\$60 FOOD AND WATER RELIEF</p>	 <p>\$25 MOTHER-CHILD HEALTH PARCEL</p>
--	--	---

IN CANADA

 <p>\$20 TWO-DAY GROCERY SUPPLY</p>	 <p>\$35 INFANT CARE PACKAGE</p>	 <p>\$45 HOT MEALS</p>
---	--	--

CHOOSE YOUR OWN AMOUNT

\$



All gifts are matched by BMO Bank of Montreal. That means your impact is DOUBLED!



Your Donation
RESET

\$60

COMPLETE DONATION >

PSEUDO-SET CONDITION

Deliver a Red Cross Survival Kit that helps others this holiday season

Simply select the gifts you'd like to send. Choose all six to deliver the most impact with a complete Red Cross Holiday Survival Kit.

INTERNATIONAL



\$15

BLANKETS: WARMTH FOR REFUGEES



\$60

FOOD AND WATER RELIEF



\$25

MOTHER-CHILD HEALTH PARCEL

IN CANADA



\$20

TWO-DAY GROCERY SUPPLY



\$35

INFANT CARE PACKAGE



\$45

HOT MEALS

[SELECT ALL >](#)

[CHOOSE YOUR OWN AMOUNT](#)

\$



These gifts go a long way! Vulnerable Canadians rely on hot meal deliveries every day. **Keep going with another gift in your Survival Kit?**



All gifts are matched by BMO Bank of Montreal. That means your impact is DOUBLED!



Your Donation
RESET

\$60

[COMPLETE DONATION >](#)

APPENDIX B—Data Collection Process

We pre-specified when data collection would end for all studies. No data were excluded and we report all conditions and measures.

For online studies, the sample sizes were pre-set through Mechanical Turk with the objective of collecting at least 100 participants per cell. We pre-specified 100 per cell in Study 1. We increased this target slightly to 120 per cell in Studies 2 and 3 to accommodate the dichotomous dependent measure and additional condition. With the continuous measures used in Study 5, we again pre-specified 100 per cell. Attrition across the four online studies was low (often 0% and never surpassing 2.5%) and did not vary across all conditions (Zhou & Fishbach, 2016).

For Study 4, which was conducted in the lab, we collected as much data as possible in two days of prescheduled sessions.

For the Field Data, we collected donor data during the predetermined CRC Holiday Campaign timeframe. The data were obtained via the Canadian Red Cross and extracted from the site using Google Analytics. Due to sampling constraints imposed by Google Analytics, the results reported above represent a random sample of approximately 80% of the total sample population. Additionally, these data reflect decisions made by donors who arrived at the three donation web pages and pressed the “Complete Donation” button (after which they were directed to a separate payment page to complete the transaction); overall transaction follow-through rates were high (~80%) and did not vary across conditions. We reported “Number of Unique Items Donated” as the dependent variable both to capture the extent to which participants were completing donation sets and to create the most parallel comparison across the three groups: pseudo-set participants could only donate one of each gift, while cash and gift participants could

donate multiples of each gift. However, the results also hold using “Total Number of Items Donated” as the dependent variable.

Fig. 1

Progress indicators displayed in the gift condition (left) and pseudo-set condition (right)



Fig. 2.

Histogram of the number of unique items donated by Canadian Red Cross donors; six items constituted the “pseudo-set” (Field Data)

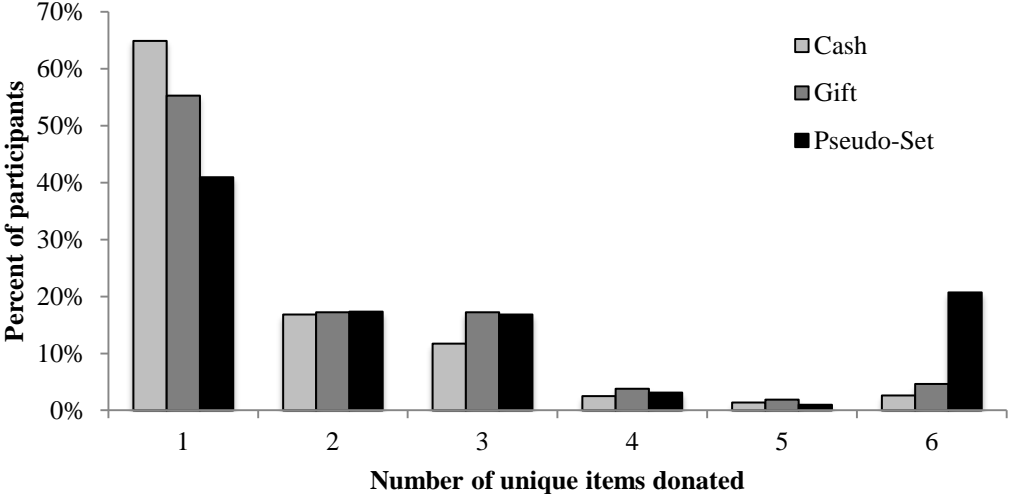


Fig. 3.

Bonus accrual display for the control (left) and pseudo-set (right) conditions, immediately after Gamble #3 (Study 1)



Fig. 4.

Histogram of the number of gambles taken by condition (Study 1)

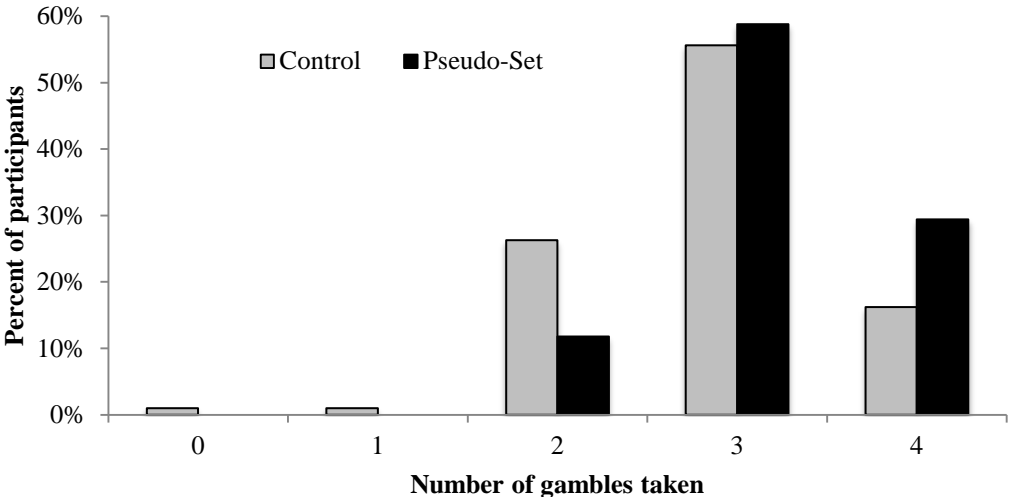


Fig. 5.

Progress display for visual control (left) and pseudo-set (right) conditions (Study 2)

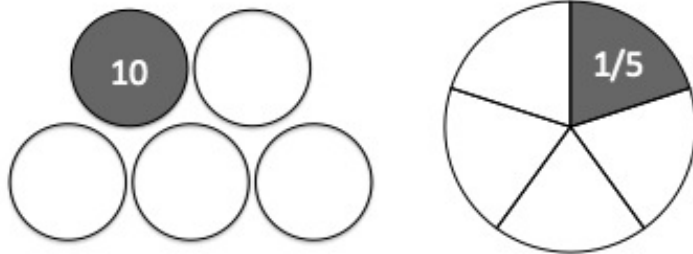


Fig. 6.

Percent of participants completing each number of tasks; completing the pseudo-set required completing all 50 tasks (Study 2).

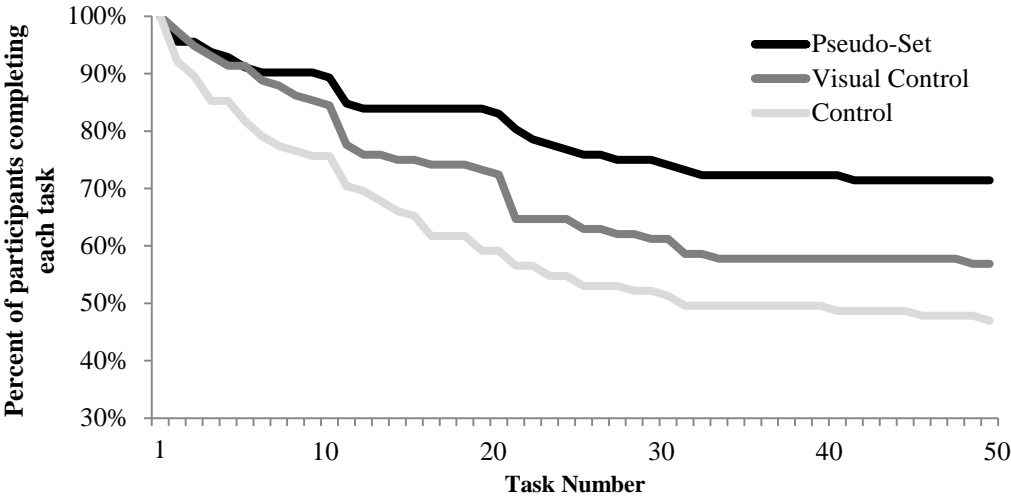


Fig. 7.

Percent of participants completing each number of tasks; completing the pseudo-set in the “four unit” condition required completing 20 tasks, while completing the pseudo-set in the “seven unit” condition required completing 35 tasks (Study 3)

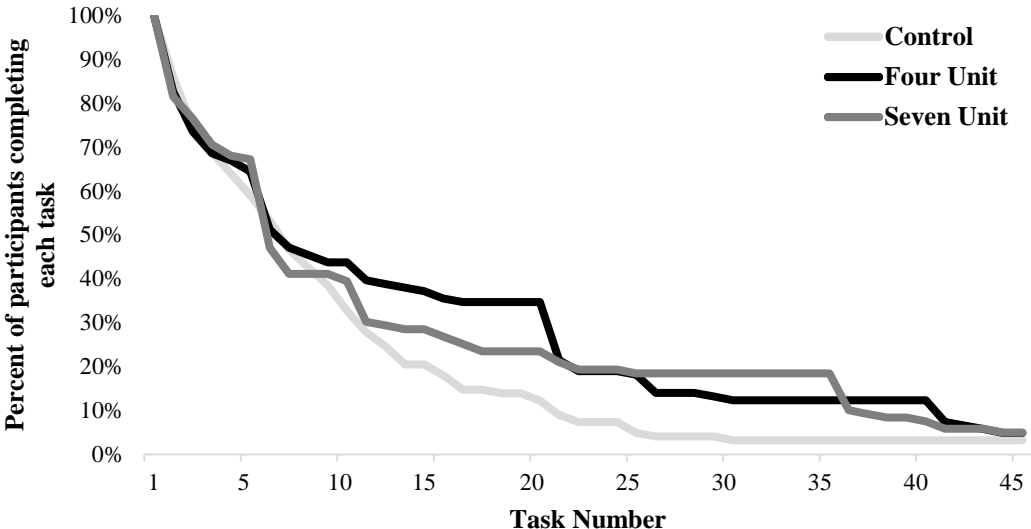


Fig. 8.

Progress display for control (left) and pseudo-set (right) conditions (Study 4)

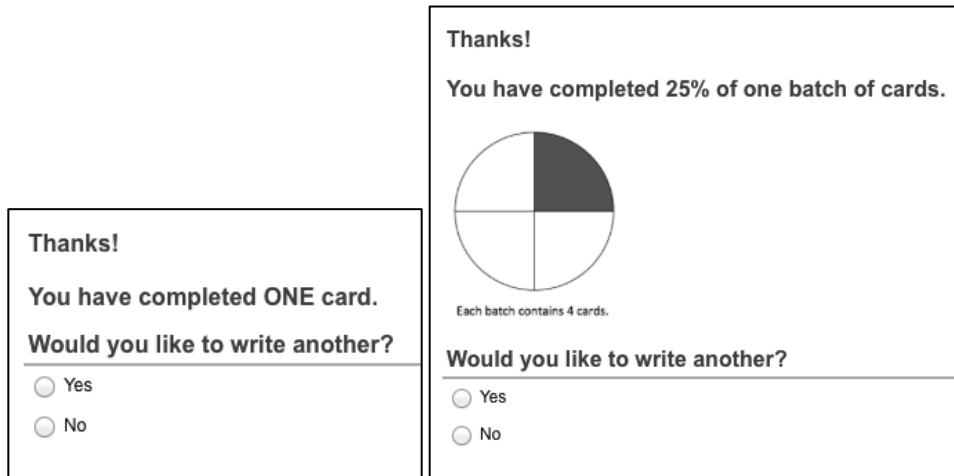


Fig. 9.

Number of cards completed by condition (Study 4)

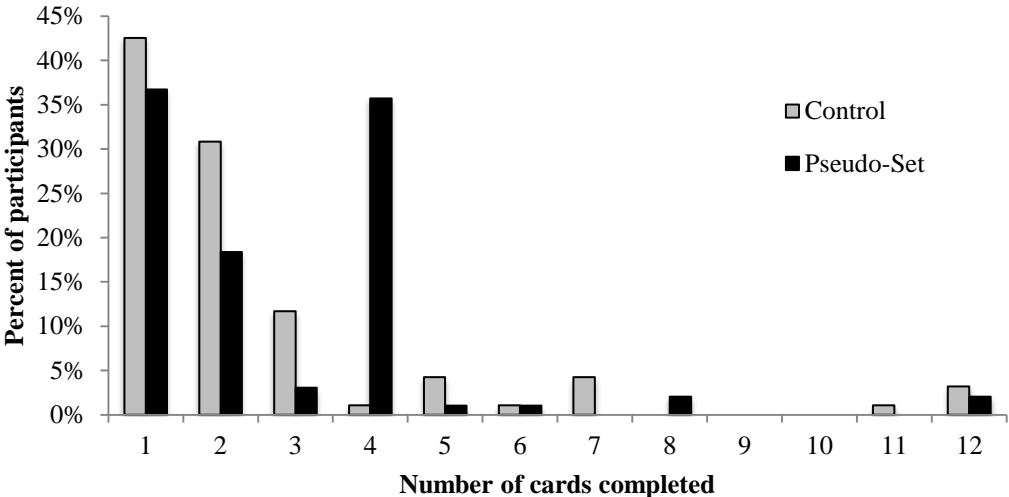


Fig. 10

Graphics shown to participants who learned they already had three beers in their cart, for the reference point (left) and pseudo-set (right) conditions (Study 5)



Fig. 11

Histogram of number of beers participants added to cart by framing and number of beers already in cart (Note: clustered bars sum to 100%) (Study 5)

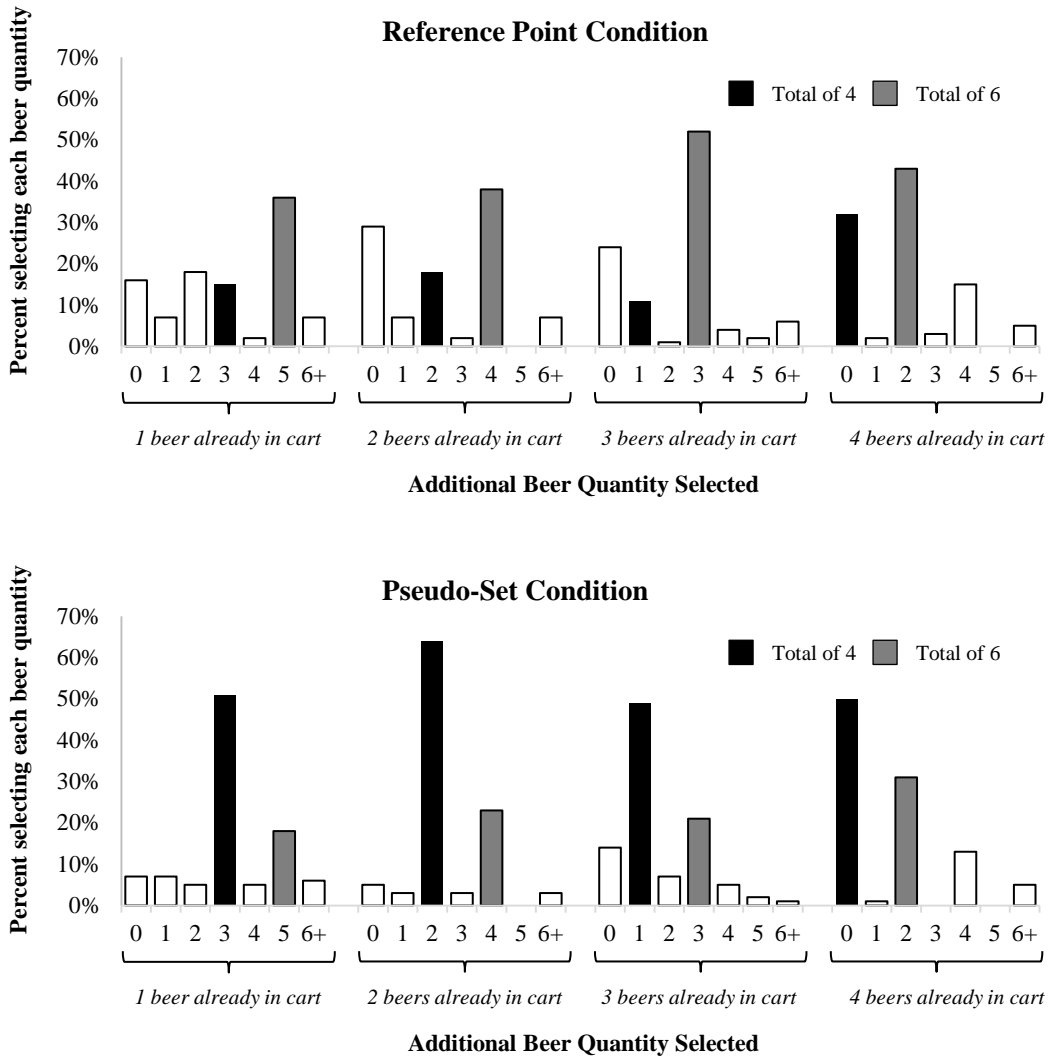


Fig. 12

Perceived completeness by condition (Error bars represent standard error) (Study 5)

