

ABSTRACT

Title of Document: TRACKING THE COST HEURISTIC: A RULE OF THUMB IN CHOICE OF COUNTERFINAL MEANS.

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Domain-specific empirical evidence shows that people sometimes infer a correlation between the cost of an item or behavior (e.g., price, effort) and its quality or efficacy (e.g., Labroo & Kim, 2009; Shiv, Carmon, & Ariely, 2005). In the present research, I proposed and tested a general mechanism that may underlie these instantiations. I termed this mechanism the *cost heuristic*, whereby people may infer that if a means is costly or detrimental to alternative goal(s) (i.e., *counterfinal*), then it must be highly instrumental to whatever focal goal it serves. Three preliminary studies provided inconsistent support for the cost heuristic. However, two of these studies suggested that a reverse halo effect (Thorndike, 1920) might have interfered with the cost heuristic. In three additional studies, I controlled for this reverse halo effect and tested more nuanced hypotheses about conditions under which the cost heuristic might be particularly likely to emerge. In Study 1, after statistically controlling for reverse halo effects, I found marginal support for a general cost heuristic, but not for the proposed moderators of alternative goal magnitude

and the perceived ecological validity of the cost heuristic. In Study 2, the alternative goal magnitude manipulation was only effective in one goal domain; however, in this domain the results fully supported the hypothesis. Those who perceived the cost heuristic as ecologically valid were more likely to exhibit it under high (vs. low) alternative goal magnitude, whereas for those who did not perceive it as ecologically valid, alternative goal magnitude did not make a difference. In Study 3, the results did not support my hypothesis, but rather suggested that the vignettes' length/complexity may have obscured the detrimentality cue on which the cost heuristic is based. Taken together, the evidence from these studies is suggestive but inconclusive with regard to a cost heuristic and the conditions under which it might manifest. Limitations and future directions are discussed.

TRACKING THE COST HEURISTIC: A RULE OF THUMB IN CHOICE OF
COUNTERFINAL MEANS.

By

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Table of Contents

Acknowledgements	ii
Table of Contents	iii
List of Figures	vi
Introduction	1
Self-Defeating Behaviors	2
Evidence of an Inference: Costly Behaviors as Highly Instrumental	4
Costly Means in Goal Systems Theory	6
Preliminary Study 1: Swine Flu Medications	8
Pilot Study 1: Healthy and Unhealthy Beverages	10
Method	10
Participants	11
Design and procedure	11
Results	12
Beverage instrumentality	12
Alternative goal magnitude	13
Discussion	14
Pilot Study 2: Eating Disordered Behaviors	16
Method	16
Participants	17
Procedures and measures	17
Results	18
Manipulation check	19
Means instrumentality	20
Alternative goal magnitude	21
Discussion	22
The Cost Heuristic	23
Theory of the cost heuristic	24
Hypotheses	27
Hypothesis 1	28
Hypothesis 2	29
Study 1	30
Method	32
Participants	32
Procedure	33
Results	36

Manipulation/attention checks	36
Alternative goal magnitude	37
Ecological validity of the cost heuristic	37
Means type (counterfactual vs. unifinal)	38
Hypothesis test	38
Hypothesis test, controlling for reverse halo (“spillover”) effects	38
Discussion	40
Study 2	42
Method	43
Participants	44
Procedure	44
National security focal goal – Governmental transparency alternative goal	45
Governmental transparency focal goal – National security alternative goal	46
Dependent measures	47
Results	48
Manipulation check	48
Hypothesis test	49
Discussion	51
Study 3	53
Method	54
Participants	54
Procedure	55
Results	59
Manipulation check	59
Hypothesis test	60
Exploratory analyses	62
War goal domain	63
Charity goal domain	64
Discussion	64
General Discussion	69
Limitations and Future Directions	71
Reverse halo effects	71
Difficulty of cost heuristic	73
Prior beliefs about means	73
Alternative goal magnitude	74
Ecological validity variable	75
Salience assumption	76
Social proof	76
Degree vs. presence of cost	77
Domain-specific or general	78
Implications for choice	80
Conclusion	80

Appendices	82
References	88

List of Figures

Figure 1. Ratings of counterfinal, multifinal, and unifinal medications' instrumentality to the focal goal of treating an illness (swine flu or common cold). All pairwise comparisons are statistically significant ($ps < .001$).

Figure 2. Ratings of counterfinal and multifinal beverages' instrumentality to a focal thirst-quenching goal, as a function of focal goal magnitude.

Figure 3. Ratings of the 7 most and 7 least counterfinal/detrimental eating behaviors' instrumentality to a focal weight loss goal, as a function of focal goal magnitude.

Figure 4. Ratings of focal goal achievement probability as a function of alternative goal magnitude and the perceived ecological validity of the cost heuristic. Results displayed are within the alternative goal domain of "exposing personal scandal among government officials".

Figure 5. Ratings of focal goal achievement probability for counterfinal and unifinal means in the charity vs. war goal domains.

Figure 6. Ratings of focal goal achievement probability for counterfinal and unifinal means among those who scored high vs. low on the perceived ecological validity of the cost heuristic.

Figure 7. Ratings of focal goal achievement probability for counterfinal and unifinal means in the easy vs. difficult digit rehearsal task conditions. Results depicted are for the war goal domain among those who scored high on the perceived ecological validity of the cost heuristic.

Tracking the Cost Heuristic: A Rule of Thumb in Choice of Counterfactual Means

One of the most fascinating and longstanding research topics at the intersection of psychology, economics, political science, and other related disciplines is what leads people to act in ways that run counter to their interests. In many economic and mathematical decision models, such decisions or behaviors are labeled irrational, because they either fail to serve a desired end or serve an infeasible end (for a review, see Kruglanski & Orehek, 2009). In political science, researchers have struggled to understand self-destructive phenomena such as suicide attacks, hunger strikes, self-immolation, and other extreme behaviors (e.g., Gambetta, 2005; Wintrobe, 2006). In anthropology and ecological or biological science, researchers have explored self-sacrificial rituals and behaviors in both humans and non-human animals (e.g., Henrich, 2009; McAndrew, 2002; Sosis & Alcorta, 2003). In psychology, researchers have examined countless behaviors that run counter to people's self-interest, including suicide, self-harm, and other self-defeating behaviors (e.g., Baumeister, 1990; Baumeister & Scher, 1988).

Within and across these fields, researchers have proposed and tested a variety of domain-specific mechanisms that might give rise to particular types of costly or self-harmful behaviors or phenomena. However, there have been few attempts to synthesize the literature and identify general mechanisms that may underlie all instances of costly or self-harmful behavior. The goal of the present research is to propose and test such a general mechanism. In the pages that follow, I present preliminary evidence that such a mechanism exists, including literature reviews and three pilot studies I conducted in an attempt to establish the basic phenomenon. I then provide a more nuanced analysis of

conditions under which the phenomenon should be particularly likely to emerge and report on three additional studies I conducted to test these ideas.

Self-Defeating Behaviors

A notable (and rare) review of the domain of self-defeating behaviors was offered by Baumeister and Scher (1988). Here I examine their analysis in particular reference to issues at the heart of the present research. Baumeister and Scher (1988) define self-defeating behavior as “any deliberate or intentional behavior that has clear, definitely or probably negative effects on the self or on the self’s projects” (p. 3). After reviewing the psychological literature on this topic, they arrived at three conceptual subtypes of self-defeating behaviors. The first is deliberate or primary self-destruction, in which self-harm is the primary or focal goal of the behavior. Baumeister and Scher’s literature review led them to conclude that deliberate or primary self-destruction is virtually nonexistent among people in non-clinical adult populations. Rather, people in non-clinical populations tend to engage in the remaining two types of self-defeating behaviors: counterproductive strategies and tradeoffs. In counterproductive strategies, people engage in behavior that is intended to produce focal goal progress or achievement, but that instead leads to unintended and unforeseen self-harm, because the behavior was ineffective or perhaps even detrimental to the focal goal. For instance, an individual who engages in transparent ingratiation in the hopes of currying favor with authority figures may actually harm his or her reputation (Jones & Wortman, 1973). In tradeoffs, by contrast, people engage in a behavior that they believe will facilitate pursuit of an important or salient focal goal, but the behavior also risks or definitely incurs costs to alternative goal(s). Moreover, in tradeoffs (as Baumeister and Scher define them) the

costs to the alternative goal are known and foreseen but accepted as a necessary part of focal goal pursuit. For example, Baumeister and Scher cite self-handicapping (Berglas & Jones, 1978) as an example of a tradeoff wherein one pursues the goal of making or maintaining positive attributions about the self, but at the expense of the alternative goal of actual success.

The tradeoff configuration is the focus of the present research; however, I expand Baumeister and Scher's notion of tradeoffs to also include behaviors in which costs to alternative goal(s) may *not* be salient (i.e., foreseen) at the time the individual decides to *engage* in the behavior. Indeed, people may suppress alternative goal considerations at the time they embark on a behavioral path, for instance due to the magnitude of a salient, important focal goal that conflicts with the alternative goal (Shah, Friedman, & Kruglanski, 2002). Conversely, people may foresee the costs at the time they choose to engage in the behavior and simply decide that the focal goal is more important than whatever alternative goals are undermined by the behavior. In either case, the salience of the costs at the time the individual *engages* in the behavior are irrelevant to the present analysis. Rather, what is most pertinent here is that, at the time the individual first learns or is exposed to the *relation* of the behavior to the focal and alternative goals, I propose that the individual does perceive (consciously or unconsciously) the behavior's risk or the costs it carries to alternative goal(s).

Perhaps of greatest relevance to the present research, Baumeister and Scher (1988) discussed the potential role of judgment errors as a source of tradeoffs and counterproductive behaviors, noting as specific examples: a) temporal discounting of costs (particularly uncertain or risky costs), b) misestimating self-efficacy, and c)

distorting or misestimating *means expectancy*, or the probability that a behavior will achieve or afford progress toward a goal. This latter mechanism, the distortion or misestimation of means expectancy, is the focus of the present research. However, Baumeister and Scher's analysis only identifies instances of distortion or misestimation of means expectancy in cases of counterproductive behaviors, which are not of focal interest here. In the present research, I propose and test a novel means expectancy mechanism that may apply to tradeoffs. Specifically, people may infer (correctly or incorrectly) that a behavior which is costly to alternative goal(s) must then be highly or particularly instrumental to a focal goal. I now turn to an outline of previous research that suggests this general phenomenon exists.

Evidence of an Inference: Costly Behaviors as Highly Instrumental

Previous research and popular cultural wisdom contain plentiful evidentiary examples of the hypothesized, general inference that costly behaviors (i.e., costly to an alternative goal) are especially instrumental to whatever focal goal they serve. However, researchers have thus far presented this evidence piecemeal, in limited goal domains or for specific goal contents, without the suggestion of a unifying framework or common thread among instantiations. In contrast, the inference that I am proposing here is a general mechanism that should be evident regardless of the specific *contents* of goals to which a behavior is detrimental and instrumental. The studies I propose in the present research test for this inference in a range of new goal domains, thus investigating its proposed generality. Nonetheless, evidence from previous research is strongly suggestive of the general inference I am proposing, at least for specific goal domains or contents.

One well-known example is the price-quality association, by which products that are more expensive are assumed to be of higher quality (Shiv, Carmon, & Ariely, 2005). For instance, researchers have recently suggested that although the association between the price and *perceived* quality of wine may be positive, the relation between price and *actual* quality may be zero except among wine experts (Goldstein et al., 2008; Plassmann, O’Doherty, Shiv, & Rangel, 2008). Accordingly, Rao (2005) suggested that the accuracy of a price-quality association may not hold in the modern marketplace, but placebo effects due to advertisers’ instillation of price-quality expectancies among consumers may nonetheless produce a strong price-quality association, which is then borne out as a sort of self-fulfilling prophecy.

The Protestant work ethic is another example of the proposed inference between the cost of an object or behavior and its effectiveness, specifically implying the value of hard work and effort for goal pursuit (Weber, 1904/2001). Researchers (Labroo & Kim, 2009) have accordingly found that people do tend to view effort as implying efficacy for whatever goal the effort was directed toward. The ideas that nothing in this world comes free, that nothing worth doing or having is easy or cheap, and the “no pain—no gain” dictum are all examples of this inference in popular cultural wisdom.

Another domain-specific example of the inference that costly behaviors are especially effective is the notion of great sacrifice for a valued cause, which resonates throughout much cultural and religious literature in both monotheistic and polytheistic traditions (Henrich, 2009). Researchers have suggested that people view individual sacrifice for the benefit of the ingroup as an evolutionarily adaptive strategy for preserving one’s genetic and/or cultural lineage, or perhaps for rendering the ingroup

more cohesive and thus triumphant in outgroup conflict (Atran & Henrich, 2010). Relatedly, people may engage in costly signaling behaviors (e.g., dangerous or self-harmful ritual displays, such as walking across fire, symbolic skin etchings, genital mutilation, and so on) in order to demonstrate commitment to a valued group cause or belief (Henrich, 2009; Sosis & Alcorta, 2003).

These domain-specific instantiations of the inference that costly behaviors are especially effective hint at the possibility of a more general inference that applies irrespective of goal contents. I now turn to goal systems theory (Kruglanski et al., 2002), which provides a useful terminology for establishing the basic elements and properties of the general inference I propose in the present research.

Costly Means in Goal Systems Theory

Goal systems theory (Kruglanski et al., 2002) describes the structure and consequences of various patterns of interconnected goals and means. In goal systems theory, means are often described in terms of the number and nature of their relations to goals. Several such configurations of means-goal relations are relevant to the present research. Most notably, goal systems researchers (Kruglanski, Klein, & Belanger, 2013) have begun to use a new term to describe means that are instrumental to a focal goal but incur costs, or are *detrimental*, to alternative goal(s). Such means are called *counterfinal* means: instrumental to a focal goal, but detrimental to alternative goal(s).¹ The general inference I have proposed in the present research pertains to counterfinal means. Specifically, people may infer that *because* counterfinal means are detrimental to

¹ Note that a counterfinal means has the same configuration in relation to focal and alternative goals as does Baumeister and Scher's (1988) notion of tradeoffs: instrumental to a focal goal, detrimental to alternative goal(s).

alternative goal(s), they must then be particularly or highly instrumental to whatever focal goal they serve.² For example, consider a drug that treats erectile dysfunction (ED), but that has costly, heart-related side effects. If the proposed inference exists, people would infer that because this drug is detrimental to heart health, it must then be particularly effective at treating ED.

This proposition begs the question: *In relation to what* are counterfinal means perceived as particularly or highly instrumental? As a useful comparison for counterfinal means in the present research, goal systems theory (Kruglanski et al., 2002) outlines two additional types of means that have different configurations of relations to focal and alternative goals. One is a *unifinal* means, which is instrumental to a focal goal but unrelated to alternative goals; in other words, a unifinal means is uniquely instrumental to a focal goal. For instance, a drug to treat ED that has no side effects would be a unifinal means, instrumental only to the goal of treating ED. A *multifinal* means is instrumental to a focal goal, but it is also instrumental to at least one alternative goal. Continuing with the example, a drug that treats ED and also boosts heart health would be a multifinal means, instrumental to multiple goals.

Returning to the proposed phenomenon, if a counterfinal means is perceived as highly instrumental to its focal goal, *relative to* unifinal and/or multifinal means to that same focal goal, this would constitute evidence for the proposed, general inference about counterfinal means. In other words, the ED drug that has negative side effects on heart health would be perceived as more effective for treating ED than either the drug with no

² This perception is related to but importantly different from what Kelley (1971) defined as the *augmentation effect*. The augmentation effect is an inference that, given an actor's choice of counterfinal means to a focal goal, their focal goal must be of high magnitude. In other words, people infer that the focal goal must be extremely important if one is willing to use costly means to pursue that goal.

side effects or the drug that actually increases heart health. In what follows, I outline three preliminary studies I conducted, including one study from my master's thesis and two additional pilot studies, in an attempt to demonstrate this general phenomenon. That is, counterfinal means may be perceived as more instrumental than multifinal or unifinal means to a focal goal.

Notably, these preliminary studies also included an additional independent variable, focal goal magnitude, which afforded tests of additional hypotheses that were of interest at the time. *Goal magnitude* is defined in cognitive energetics theory (Kruglanski et al., 2012) as goal importance, but can also involve notions of goal salience, commitment, value, concern, and related concepts. All of these concepts center on the idea that the goal is an important and salient driving force for the individual at a particular time. However, the hypotheses that required the inclusion of focal goal magnitude in my preliminary studies are not pertinent to the present research. Therefore, although I included focal goal magnitude as an independent variable in the following preliminary studies to maintain fidelity to the methodology, my hypothesis for the present research is a main effect: regardless of focal goal magnitude, counterfinal means should be perceived as most instrumental to the focal goal.

Preliminary Study 1: Swine Flu Medications

The first study I conducted to test my hypotheses was part of my master's thesis and is thus reported in detail elsewhere. For brevity's sake, here, I summarize its methodology, results, and implications. This study provided some evidence for the cost heuristic in a health and finance domain (Klein, 2011). In the week following the H1N1 virus (swine flu) outbreak in spring of 2009, I presented participants ($N = 262$) with an

article ostensibly from the Centers for Disease Control (CDC), which detailed information about three medications designed to treat either a) the H1N1 virus or b) the common cold. This served as a focal goal magnitude manipulation; during the H1N1 outbreak, participants presumably were more concerned with combating H1N1 than the common cold. In counterbalanced order, I presented one medication as unifinal (instrumental only to treating the illness), another as multifinal (instrumental to treating the illness, as well as cheap and easy to obtain), and a third as counterfinal (instrumental to treating the illness, but expensive and difficult to obtain). As predicted, participants perceived the counterfinal medication as the most effective of the three for treating the focal illness ($F[2,243] = 67.34, p < .01$; see Figure 1). This main effect was not qualified by an interaction with focal goal magnitude, supporting my hypothesis.

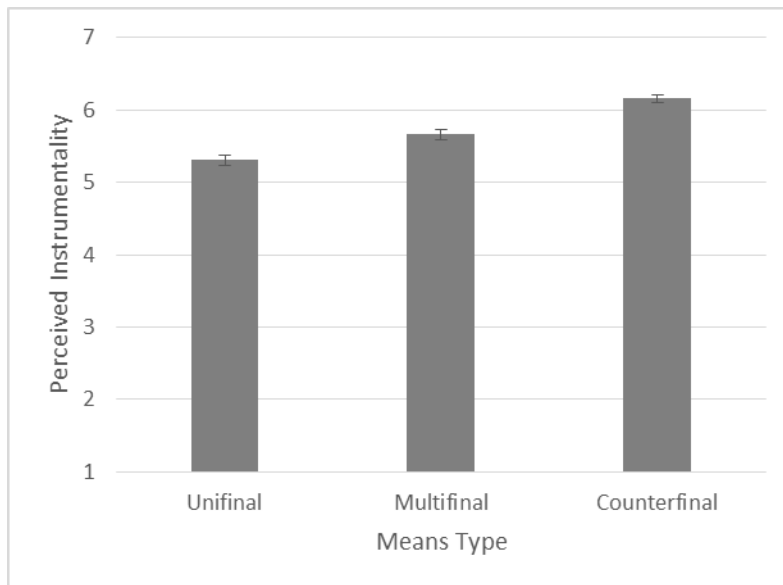


Figure 1. Ratings of counterfinal, multifinal, and unifinal medications' instrumentality to the focal goal of treating an illness (swine flu or common cold). All pairwise comparisons are statistically significant ($ps < .001$).

Despite this result, the swine flu study did not necessarily provide the cleanest test of a general, all-purpose inference about counterfactual means that applies regardless of the contents of the focal and alternative goals. For one, the description of the unifinal medication did not contain any filler information to render its description equally informative or lengthy as description of the counterfactual medication, which contained additional information about the medication's impact on alternative goal pursuits (money and ease of obtainment). As such, several participants ($n = 26$) stated that they felt there was not enough information about the unifinal means. Although many more participants ($n = 100$) reported perceiving the unifinal medication as falling between the multifinal and counterfactual medications in financial cost and difficulty of obtainment, a fairer test of the proposed inference would involve a unifinal means for which the same amount of information is provided as for the counterfactual means.

Additionally, and perhaps more importantly, the goal domains in which the counterfactual means in this study incurred costs (monetary cost and difficulty of obtainment) render this instantiation of the proposed inference redundant with previous research that has already demonstrated this inference in these domains. Specifically, research on the price-implies-quality heuristic (e.g., Shiv et al., 2005) and the scarcity principle (Cialdini, 2001) has already shown that more costly means in terms of money or difficulty of obtainment (the two alternative goal concerns to which the counterfactual medication in my study was detrimental) are perceived as better. These heuristics may be domain-specific instances of a general, all-purpose inference about counterfactual means, but without additional evidence outside of these domains, this claim is speculative. In Pilot Studies 1 and 2, I therefore sought to obtain less impeachable evidence.

Pilot Study 1: Healthy and Unhealthy Beverages

Pilot Study 1 provided an additional test of the inference that counterfinal means are perceived as especially instrumental to a focal goal, in this study, relative to multifinal means. Participants were preselected for the study on the basis of their high commitment (evinced in pretesting) to the alternative goal of a healthy diet. As a manipulation of the magnitude of a focal thirst-quenching goal, participants were asked to refrain from drinking any beverages for either 1 or 5 hours prior to the study. When participants arrived at the lab, they proceeded to rate the focal goal instrumentality (i.e., thirst-quenchingness) of counterfinal (unhealthy) and multifinal (healthy) beverages. I predicted that participants would perceive counterfinal (vs. multifinal) beverages as more thirst-quenching (i.e., instrumental to the focal goal), regardless of focal goal magnitude.

Method

Participants. Forty-four undergraduate students at a large mid-Atlantic university participated in exchange for extra course credit. All participants had mean scores of 5 or greater (on a 7-point scale) on a 16-item questionnaire measuring commitment to the alternative goal of dieting/health (see Appendix A for the measure; K. Fujita, personal communication, February 26, 2008). I collected these scores in mass pretesting conducted online throughout the semester, and I selected and recruited participants via email on the basis of their scores on this measure.

Design and procedure. The study design involved the manipulation of a single independent variable between participants: focal goal magnitude (high vs. low).

I emailed participants prior to their arrival at the lab with instructions to refrain from consuming any beverages for either 1 hour (low focal goal magnitude condition) or

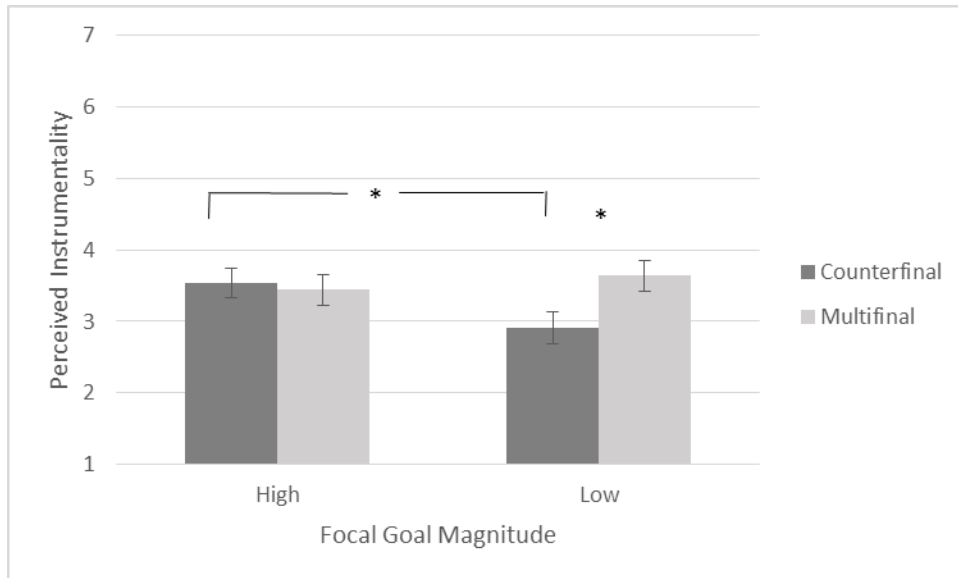
5 hours (high focal goal magnitude condition) before participating in the study. This constituted the focal goal magnitude manipulation for the goal of thirst-quenching. The researcher rescheduled any participants who reported failing to follow these instructions. Upon their arrival at the lab, participants provided informed consent and completed the rest of the study on a computer. They saw several instructions pages, one of which stated: “You have been selected for this study because in pretesting, you scored higher on a health and dieting attention measure than most participants. In other words, health and dieting are more important to you than they were to most other participants.” This was intended to make their alternative goal salient. Then, they were informed that the study concerned people’s beverage preferences at different times of day.

Participants completed several filler items (e.g., mood states), as well as two items concerning their commitment to the alternative goal of a healthy diet (“My health is very important to me” and “Maintaining a healthy diet is very important to me”), which they rated on a 10-point scale from 1 (*Strongly Disagree*) to 10 (*Strongly Agree*). Participants then completed several filler items, in addition to rating how thirst-quenching (i.e., instrumental to the focal goal) each beverage was on a scale from 1 (*Not at all*) to 7 (*Very much*) for 18 non-alcoholic beverages. In pretesting, eight of these beverages were rated as healthier and as having lower caloric content than eight others, $t(14) = 9.15, p < .01$. However, all 16 were pretested as equally thirst-quenching ($t = 1.63, p = .13$) and equally tasty ($t = 0.35, p = .73$). The healthy beverages (e.g., V8 juice) were *multifinal* means, instrumental to quenching thirst and to a healthy diet, whereas the unhealthy beverages (e.g., root beer) were *counterfinal* means, instrumental to quenching thirst but detrimental to a healthy diet.

Lastly, participants underwent a funnel debriefing process to ascertain suspicion about the purpose and hypotheses of the study. None of the participants guessed any of the research hypotheses. The researcher then debriefed and dismissed them.

Results

Beverage instrumentality. I conducted a 2 (focal goal magnitude: high vs. low) x 2 (means type: multifinal vs. counterfinal) mixed ANOVA on ratings of beverages' thirst-quenchingness (i.e., focal goal instrumentality), where focal goal magnitude was between-subjects and means type within-subjects. The interaction effect was significant, $F(1,42) = 8.99, p < .01$. Contrary to my prediction, participants did not under any conditions perceive counterfinal (vs. multifinal) beverages as more instrumental to the focal thirst-quenching goal. Under low focal goal magnitude, participants perceived the multifinal, healthy beverages ($M = 3.64, SE = 0.22$) as more instrumental than counterfinal, unhealthy beverages ($M = 2.91, SE = 0.22; p = .001$). However, this difference disappeared under high focal goal magnitude ($p = .60$). Additionally, counterfinal means were perceived as more instrumental under high (vs. low) focal goal magnitude ($p = .04$), but the perceived instrumentality of multifinal means' did not change across focal goal magnitude conditions ($p = .51$; see Figure 2).



Note: * $p < .05$.

Figure 2. Ratings of counterfinal and multifinal beverages' instrumentality to a focal thirst-quenching goal, as a function of focal goal magnitude.

The main effect of means type was significant ($p = .03$) but was qualified by the interaction reported above. Moreover, the pattern of means showed that *multifinal* (vs. counterfinal) beverages were rated as more instrumental to the focal goal, precisely opposite what was predicted. The main effect of focal goal magnitude was not significant ($p = .43$).

Alternative goal magnitude. I conducted an independent t test to determine the effect of focal goal magnitude on the magnitude of the *alternative* goal of a healthy diet. The dependent measure in the analysis was the mean of two commitment rating items for the dieting and health goals ($\alpha = .84$). Participants in the high focal goal magnitude condition reported lower commitment ($M = 8.46$, $SD = 1.07$) to the alternative healthy diet goal than did participants in the low focal goal magnitude condition ($M = 9.14$, $SD = 0.82$), $t(42) = 2.37$, $p = .02$.

Discussion

The results of Pilot Study 1 did not support my hypothesis; under no conditions did participants perceive counterfinal means as more instrumental than multifinal means to the focal goal. However, the results did reveal that only ratings of the counterfinal means (and not the multifinal means) were impacted by focal goal magnitude. Specifically, counterfinal means were perceived as *more* instrumental under high (vs. low) focal goal magnitude, and to the same degree as were multifinal means. In other words, focal goal magnitude uniquely impacted the perceived instrumentality of counterfinal (but not multifinal) means. This suggests that further investigation of focal goal magnitude and other factors that affect ratings of counterfinal means might be worthwhile.

One possible explanation for these findings is an unanticipated spillover effect for counterfinal means, akin to what Thorndike (1920) termed a reverse halo effect. Specifically, counterfinal means' detrimentality to alternative goals is a negative quality that participants may have generalized to other qualities of the counterfinal means, including its perceived instrumentality to the focal goal. Absent any explicit instructions or similar steps that lead participants to provide a clean rating of counterfinal means' instrumentality to the focal goal that lacks any halo component, participants may rate counterfinal means as relatively low in instrumentality (i.e., negatively), consistent with a reverse halo effect. However, such results only obtained under *low* focal goal magnitude in Pilot Study 1; under high focal goal magnitude, participants rated counterfinal means as *equally* instrumental to multifinal means, thus *not* exhibiting a reverse halo effect. Why might this be the case?

A reverse halo effect for counterfinal means may be more apparent under low (vs. high) focal goal magnitude because of the well-established effects of *focal* goal magnitude on *alternative* goal magnitude. That is, previous research has demonstrated that under high (vs. low) focal goal magnitude, alternative goals become less accessible or salient (Kopetz, Faber, Fishbach, & Kruglanski, 2011; Shah, Friedman, & Kruglanski, 2002). Consistent with these previous findings, the results of Pilot Study 1 confirmed that those in the high (vs. low) focal thirst goal magnitude condition indicated lower commitment to the alternative goal of health/dieting. Such reduced magnitude of the alternative health/dieting goal may likewise reduce the salience of counterfinal means' (negative) detrimentality to this alternative goal. And if the salience of this negative quality is reduced, so would be any reverse halo effects that spill over from this negative quality to other attributes of the counterfinal means. Thus, under high (vs. low) focal goal magnitude, counterfinal means should no longer be rated as less instrumental than multifinal means to a focal goal – and this is precisely what I found.

Another potential explanation might be that heightened focal goal magnitude simply increases the perceived instrumentality of any and all means to that focal goal. That is, the heightened desire to attain a goal may make any means to that goal appear more effective, via a sort of hopeful distortion mechanism. However, this explanation is inconsistent with the results of Pilot Study 1, because focal goal magnitude did *not* impact the perceived instrumentality of multifinal means – rather, only the counterfinal means were affected. Again, therefore, it seems that exploring focal goal magnitude and other factors that might impact perceptions of counterfinal means could yield greater insight into the conditions under which the proposed phenomenon might occur.

It is not clear whether a reverse halo effect is in fact the mechanism responsible for the findings of Pilot Study 1, but the results are consistent with this possibility. In Pilot Study 2, I provided an additional test in a new domain of the general hypothesis that counterfactual means would be perceived as especially instrumental to a focal goal. In light of the results of Pilot Study 1, I also examined the possibility that evidence consistent with a reverse halo effect for counterfactual means would be less apparent under high (vs. low) focal goal magnitude.

Pilot Study 2: Eating Disordered Behaviors

Using a weight loss goal progress manipulation developed by Fishbach and Dhar (2005), I attempted to manipulate the magnitude of female participants' focal weight loss goal. Participants then rated the focal goal instrumentality of a series of 14 eating disordered behaviors, taken from the 26-item Eating Attitudes Test [EAT-26] (Garner & Garfinkel, 1979), a measure commonly used to identify individuals who engage in behaviors associated with eating disorders. Based on participants' ratings, I split these eating behaviors into subgroups of the most and least detrimental (i.e., counterfactual) behaviors to the alternative health goal. I predicted that participants would perceive the most (vs. least) detrimental (i.e., counterfactual) eating behaviors as more effective for losing weight (i.e., instrumental to the focal goal), regardless of focal goal magnitude.

Method

Participants. I recruited 27 female undergraduate students from the Psychology Department participant pool at a large mid-Atlantic university. All participants received extra course credit in exchange for their participation. Two participants expressed either a desire to gain weight or to remain at their current weight, and one participant failed to

follow the instructions for the manipulation. I excluded these three participants from analyses, leaving a total of 24 participants for analyses.

Procedure and measures. The study design involved the manipulation of a single independent variable between participants: focal goal magnitude (high vs. low).

Participants completed the study in a laboratory setting. After providing informed consent, I manipulated the magnitude of participants' focal weight loss goal (Fishbach & Dhar, 2005). They received a paper titled "How Far Are You from Your Ideal Weight" and were asked to a) list their current weight above the center of a bidirectional arrow with endpoints representing specific weight losses or gains and then b) color in the arrow to the point that represented their ideal weight. In the high focal goal magnitude condition, participants received a sheet with the arrow endpoints labeled as +/- 5 lbs. (low perceived goal progress); in the low focal goal magnitude condition, the arrow endpoints were labeled +/- 25 lbs. (high perceived goal progress). The logic of this manipulation is that participants who receive the +/- 5 endpoints should color *more* of the arrow than those who receive the +/- 25 endpoints, making it appear to the former group as though they have very far to go in working toward their weight loss goal. As such, the goal should be of *greater* magnitude for the +/- 5 (vs. 25) endpoint group.

Following this manipulation, participants completed the remainder of the study on the lab computer. First, participants responded to 28 items on how extreme and okay (reverse-scored) 14 eating behaviors were (1 = *Strongly Disagree*, 6 = *Strongly Agree*), using the behaviors from the adapted EAT-26 (e.g., "Cut food into small pieces",

“Measure weight daily”, “Vomit after eating”)³. Next, participants rated how effective (1 = *Not at all effective*, 7 = *Highly effective*) and harmful (1 = *Not at all harmful*, 7 = *Highly harmful*) each of the 14 behaviors would be to two goals: a) achieving their ideal weight, and b) maintaining their overall health. Participants then rated the importance of each of those two goals (1 = *Not at all important*, 7 = *Extremely important*), their commitment to those goals (1 = *Not at all committed*, 7 = *Extremely committed*), and their perceived progress toward the goals (1 = *No progress at all*, 7 = *Very much progress*). Participants subsequently rated on a behavior scale how often they currently do or previously did each of the 14 behaviors listed (1 = *Never*, 6 = *Always*). Finally, participants completed some demographic items, responded to a suspicion check about the purpose of the study, and were debriefed. No participants guessed the hypotheses.

Results

Manipulation check. I conducted independent *t* tests to examine the effects of the focal goal magnitude manipulation on a) the perceived goal progress item and b) the average of the focal goal commitment and importance manipulation check items. Neither analysis revealed significant differences across conditions ($ps > .05$). However, an independent *t* test revealed that participants in the low focal goal magnitude condition ($M = 2.76$, $SD = 2.80$) colored fewer linear cm of the arrow than did those in the high focal goal magnitude condition ($M = 8.02$, $SD = 3.96$), $t(21.40) = 3.80$, $p < .01$. Thus participants in the high (vs. low) focal goal magnitude condition indirectly indicated a

³ The order of presentation of the 14 behaviors was randomized within each block of questions, and the order of each block was randomized for conceptually related blocks of measures (i.e., “extreme” and “okay” ratings, instrumentality and detrimentality ratings).

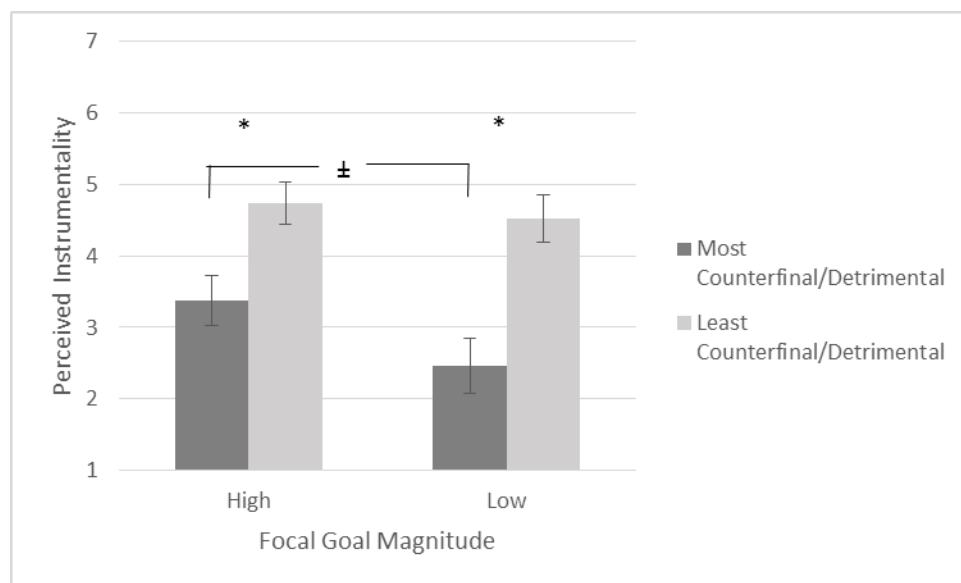
greater magnitude of the focal weight loss goal, although they did not explicitly indicate this on items that more directly asked about their goal commitment and progress.

Means instrumentality. To test the primary hypothesis, I created two subgroups of eating behaviors comprising the 7 most and 7 least (perceived) detrimental (i.e., counterfinal) behaviors to the alternative health goal. An independent t test revealed that the 7 most detrimental behaviors ($M = 4.90, SD = 1.26$) were indeed perceived as significantly more detrimental to the alternative health goal than the 7 least detrimental behaviors ($M = 2.93, SD = 1.08$), $t(23) = 5.97, p < .001$. Thus, as the dependent variable for my hypothesis test, I computed two separate scores representing the average perceived focal goal instrumentality of a) the 7 most detrimental/counterfinal eating behaviors, and b) the 7 least detrimental/counterfinal eating behaviors.

I then conducted a 2 (focal goal magnitude: high vs. low) x 2 (means type: most vs. least detrimental/counterfinal) on the eating behaviors' perceived instrumentality to the focal weight loss goal. The interaction was not significant, $F(1,22) = 2.70, p = .12$; however, this finding approached marginal significance despite very low sample size and power in this study. Therefore, I proceeded to test simple effects.

Under both high and low focal goal magnitude, the least counterfinal eating behaviors were perceived as more instrumental to the focal weight loss goal than the most counterfinal eating behaviors ($ps < .001$), although this difference was greater under low (vs. high) focal goal magnitude. Moreover, participants perceived the most counterfinal eating behaviors as marginally more instrumental to the focal weight loss goal under high ($M = 3.37, SE = 0.35$) vs. low ($M = 2.46, SE = 0.38$) focal goal magnitude, $p = .09$. By contrast, participants' perceptions of the least detrimental (counterfinal) eating behaviors

did not change as a function of focal goal magnitude (high: $M = 4.74$, $SE = 0.30$; low: $M = 4.52$, $SE = 0.33$), $p = .63$. See Figure 3 for a graphical display of these patterns.



Note: * $p < .001$, ‡ $p < .10$.

Figure 3. Ratings of the 7 most and 7 least counterfinal/detrimental eating behaviors' instrumentality to a focal weight loss goal, as a function of focal goal magnitude.

The main effect of means type was significant ($p < .001$) but was qualified by the interaction reported above. Moreover, the pattern of means showed that the *least* (vs. most) counterfinal eating behaviors were rated as more instrumental to the focal weight loss goal, precisely opposite what was predicted. The main effect of focal goal magnitude was not significant ($p = .20$).

Alternative goal magnitude. I conducted an independent t test to examine the effect of the focal goal magnitude manipulation on ratings of the alternative health goal magnitude. I computed alternative goal magnitude by taking the mean of participants' importance and commitment ratings for the alternative goal of maintaining overall health. These two items were significantly and strongly correlated, $r(22) = .76$, $p < .01$. An independent t test revealed that the focal goal magnitude manipulation had a marginally

significant effect on the magnitude of participants' alternative health goal, $t(22) = 2.09$, $p = .05$. Participants in the high focal goal magnitude condition ($M = 5.42$, $SD = 1.13$) rated the magnitude of the alternative health goal lower than did those in the low focal goal magnitude condition ($M = 6.27$, $SD = 0.79$).

Discussion

The results of Pilot Study 2 were weaker (i.e., marginally significant) than those of Pilot Study 1, likely due to lower sample size and power. However, the marginal results obtained in Pilot Study 2 are consistent with those of Pilot Study 1. Under no conditions did participants perceive the most counterfactual means as more instrumental to the focal goal than the least counterfactual means, thus failing to support my primary hypothesis. However, the results again revealed that the most (vs. least) counterfactual means were uniquely impacted by focal goal magnitude. Specifically, the most counterfactual means (but not the least counterfactual means) were perceived as *more* instrumental to the focal goal under high (vs. low) focal goal magnitude. Similarly, although the most counterfactual means were perceived as less instrumental to the focal goal than the least counterfactual means, this difference was attenuated under high (vs. low) focal goal magnitude. These results provide a weak but consistent replication of the findings of Pilot Study 1.

Again, these results could be explained by a reverse halo (spillover) effect, just as in Pilot Study 1. Participants in the high (vs. low) focal goal magnitude condition rated lower magnitude of the alternative goal, which could have attenuated any reverse halo effects for the most counterfactual means in this condition, consistent with the pattern observed. However, it is still unclear whether a reverse halo effect is indeed the

mechanism responsible for the findings of Pilot Studies 1 and 2. Moreover, I have yet to find consistent support for my hypothesis that counterfactual means will be perceived as especially instrumental to a focal goal. Nonetheless, the positive results of my preliminary swine flu study, taken together with the intriguing results of Pilot Studies 1 and 2, suggest that exploring focal goal magnitude and other factors that impact perceptions of counterfactual means could yield greater insight into the conditions under which the proposed phenomenon might occur.

In the next phase of the present research, I conducted three additional studies to test more nuanced hypotheses about such conditions under which the proposed, general inference about counterfactual means' instrumentality may be especially likely to emerge. Specifically, it is possible that the inference I am proposing operates as a heuristic, and thus it may be particularly likely to emerge under the conditions that typically elicit heuristic use. In the three new studies, I also attempted to correct (methodologically or statistically) for the possibility of reverse halo effects such as those that may have impacted the results of Pilot Studies 1 and 2. These steps were intended to provide cleaner, halo-free ratings of counterfactual means and more clearly demonstrate the predicted inference, if it exists, in the subsequent three studies. I now turn to a more fine-grained theoretical analysis which reconceptualizes the proposed inference as a heuristic.

The Cost Heuristic

The phenomenon I have proposed and attempted to demonstrate in the present research is an inference that counterfactual means are especially instrumental to whatever focal goal they serve. Furthermore, I have proposed that this inference is based on the fact that counterfactual means are detrimental to alternative goal(s). That is, people may

infer that, *because* counterfactual means are detrimental to alternative goal(s), they must then be especially or highly instrumental to whatever focal goal they serve. For example, people might infer that, *because* a drug has negative heart-related side effects, it must then be especially or highly effective for treating ED.

When stated in this manner, the general inference I am proposing in the present research appears strikingly like a heuristic, a simple if-then rule whereby people infer that *if* a means is detrimental to an alternative goal *then* it must be highly instrumental to a focal goal. Accordingly, in the next phase of the present research I will refer to this simple rule or inference as the *cost heuristic*. In what follows, I present a more nuanced analysis of the hypothesized cost heuristic, predicting conditions under which it may be particularly likely to emerge and reporting on three studies I conducted to test these predictions.

Theory of the cost heuristic. As with any heuristic, the cost heuristic may be used by different people under different conditions. According to Kruglanski and Gigerenzer's (2011) analysis of the use of heuristics in judgment and choice, as well as cognitive energetics theory's (Kruglanski et al., 2012) expansion on this analysis, the heuristic implementation process should unfold in several stages. These authors proposed that heuristics are rules just like any other, albeit typically simpler or easier to implement than more complex rules like Bayesian calculations. They further proposed that a rule will be implemented when it is perceived as the most ecologically valid rule available for the judgment or decision task at hand, given sufficient motivation and resources to apply the rule to the task. In terms of the present research, then, the cost heuristic should be implemented when it is perceived as the best rule for the judgment or

choice, but only if the individual has adequate motivation and resources to implement it. As such, there are several steps at which implementation of the cost heuristic may be stalled.

Assuming that the cost heuristic is an available rule (due to its prior acquisition), the task type and memory constraints determine whether the individual includes the rule among the set of rules he or she could use for the task (Kruglanski & Gigerenzer, 2011). Specifically, as long as the task calls for a judgment involving counterfactual means, and as long as the cost heuristic is an accessible rule, then the cost heuristic may be included in the rule set under consideration for the task. For example, if one is considering which drug to use to treat ED, he would only consider using the cost heuristic if it is an accessible rule (e.g., due to prior exposure to the cost heuristic) and if a drug with negative side effects (i.e., a counterfactual drug) is one option for treating ED.

Once a rule like the cost heuristic has entered the rule set under consideration for a task, the most (perceived) ecologically relevant rule that the actor has the motivation and resources to implement will be applied to the task (Kruglanski & Gigerenzer, 2011). Rules that are more difficult to use require more motivation and/or cognitive resources to implement (Kruglanski et al., 2012). For instance, Bayesian rules may typically be more difficult to implement than a simple recognition heuristic (Gigerenzer & Goldstein, 1996). The cost heuristic seems a relatively simple rule to implement, as it is based on a single cue: a means' detrimentality to an alternative goal. For instance, one could apply the cost heuristic in deciding which drug is most effective for treating ED simply by noting that one drug has negative heart-related side effects (i.e., is detrimental to an alternative goal); no more information would be needed to apply the cost heuristic. As

such, people should generally be able to apply the cost heuristic even under conditions of low motivation or cognitive resources.

In fact, some people may be reluctant to exhibit the cost heuristic when they have the motivation or cognitive resources to opt for (what they perceive as) a better rule. If an actor views use of the cost heuristic as ecologically invalid (inferior) relative to other rules he or she could use, the cost heuristic may *only* be employed when the individual does not have the resources or motivation to search for and implement another rule. With ample motivation and resources, an actor who does not perceive the cost heuristic as ecologically valid would instead opt for a more (perceived) ecologically valid rule. For instance, limiting an actor's cognitive resources may lead him to opt for the simple, single-cue cost heuristic and rate a drug with negative side effects as highly effective for treating ED. That is, an individual who does not perceive the cost heuristic as ecologically valid may be more likely to apply it if he is under cognitive load (vs. not). By contrast, an individual who does perceive the cost heuristic as ecologically valid may apply it even when he has ample cognitive resources, because he perceives the cost heuristic as well-suited to the task. For example, this individual would rate the drug with negative side effects as highly effective for treating ED, regardless of cognitive load.

Motivation may also impact people's application of the cost heuristic. Most relevant to the present analysis is the notion of *goal magnitude*, which is defined in cognitive energetics theory (Kruglanski et al., 2012) as goal importance, but can also involve notions of goal salience, commitment, value, concern, and related concepts. All of these concepts center on the idea that the goal is an important and salient driving force for the individual at a particular time. In the case of the cost heuristic, *alternative goal*

magnitude refers to the magnitude of the alternative goal to which a counterfactual means is detrimental. Recall that detrimentality to an alternative goal is the sole cue by which the cost heuristic should operate: *If detrimental to alternative goal(s), then highly instrumental to focal goal.* When alternative goal magnitude is high (vs. low), this detrimentality cue may be stronger and thus produce stronger implementation of the cost heuristic for those who view it as ecologically valid.

Specifically, those who do perceive the cost heuristic as ecologically valid should be more likely to apply it under high (vs. low) alternative goal magnitude. This is because the cue (detrimentality to the alternative goal) upon which they base their inference of heightened focal goal instrumentality is stronger when the alternative goal is of higher magnitude. For example, such an individual would rate a drug with negative heart-related side effects as especially effective for treating ED, and more so when heart health is of greater concern. By contrast, those who do *not* perceive the cost heuristic as ecologically valid should not apply it, regardless of alternative goal magnitude. Such an individual, for instance, would *not* rate a drug with negative heart-related side effects as especially effective for treating ED, regardless of how great a concern heart health is.

In what follows, I will first outline testable research hypotheses that derive from the foregoing theoretical analysis. I will then present results from three studies that tested these hypotheses.

Hypotheses

The cost heuristic is the perception that counterfactual means are highly or especially instrumental to whatever focal goal they serve. As such, the cost heuristic could be evinced in the present research in one of two ways. First, the cost heuristic

would be evinced if participants perceive counterfinal (vs. unifinal) means as more instrumental to a focal goal. Unifinal means are an informative, appropriate comparison group because they are identical to counterfinal means in every respect, except detrimentality to an alternative goal. That is, unifinal and counterfinal means are both uniquely instrumental to a focal goal (i.e., both are non-instrumental to alternative goals), but counterfinal means have one property that unifinal means do not: detrimentality to an alternative goal. Thus, in comparing perceptions of unifinal and counterfinal means' instrumentality to a focal goal, any differences can be attributed to the counterfinal means' detrimentality to alternative goal(s).⁴

The second way the cost heuristic could be evinced is if counterfinal means are perceived as more instrumental under the aforementioned conditions in which people would be particularly likely to apply the cost heuristic (vs. conditions in which they would not). This involves a comparison of counterfinal means' (perceived) instrumentality across experimental conditions and is more specifically discussed below along with the description of Study 2. The following hypotheses thus address two types of comparisons (a) of counterfinal means in comparison with unifinal means, and (b) counterfinal means under some conditions as compared to others.

Hypothesis 1. As the foregoing discussion suggests, high (vs. low) magnitude of the alternative goal should heighten the salience of the cue upon which the cost heuristic is based (detrimentality to an alternative goal). It should thus augment the use of the cost

⁴ A comparison of *multifinal* vs. counterfinal means, as in my preliminary/pilot studies, does not provide the cleanest test of the cost heuristic hypothesis. This is because multifinal means, in addition to not being detrimental to the alternative goal, are actually *instrumental* to that goal. Therefore unifinal means, which have *no* relation (instrumental or detrimental) to the alternative goal, are a cleaner comparison means for the counterfinal means than are multifinal means.

heuristic among those who perceive it as ecologically valid, but should not impact (non)use of the cost heuristic among those who do not perceive it as ecologically valid.

Hypothesis 1: I predict an interaction effect of alternative goal magnitude and the perceived ecological validity of the cost heuristic on the extent to which participants exhibit the cost heuristic. Among participants who perceive the cost heuristic as ecologically valid, those under high (vs. low) alternative goal magnitude should perceive counterfactual means as even more instrumental (relative to unifactual means, or to counterfactual means in other conditions). By contrast, among those who do not perceive the cost heuristic as ecologically valid, alternative goal magnitude should not impact perceptions of counterfactual means' instrumentality.

Hypothesis 2. Hypothesis 2 similarly follows from the foregoing theoretical analysis of the role of *cognitive resources* in people's application of the cost heuristic. Assuming the cost heuristic is indeed a simple, single-cue heuristic, it should be relatively easy to apply. Therefore, those who perceive the cost heuristic as ecologically valid should be able and willing to apply it regardless of cognitive load. However, those who do not perceive the cost heuristic as ecologically valid should only apply it under high cognitive load, when they lack the resources to search for (what they perceive as) a more ecologically valid rule.

Hypothesis 2: I predict an interaction effect of cognitive resources and the perceived ecological validity of the cost heuristic on the extent to which participants exhibit the cost heuristic. Among those who do perceive the cost heuristic as ecologically valid, participants should perceive counterfactual means as

more instrumental than unifinal means, irrespective of cognitive resources. By contrast, among those who do not perceive the cost heuristic as ecologically valid, those under high cognitive load should perceive counterfinal means as more instrumental than unifinal means, whereas those under low cognitive load should perceive unifinal and counterfinal means as equally instrumental.

Each of the following studies was designed to test either Hypothesis 1 or 2, which involved the interaction of either alternative goal magnitude or cognitive resources (respectively) with the perceived ecological validity of the cost heuristic. In Studies 1 and 2, I tested Hypothesis 1 by manipulating the magnitude of an alternative goal that a counterfinal means undermines, and then measuring the extent to which participants exhibited the cost heuristic. In Study 3, I tested Hypothesis 2 by manipulating participants' cognitive resources via a depletion task and then measuring the extent to which they exhibited the cost heuristic. Moreover, in Study 1 I manipulated the perceived ecological validity of the cost heuristic, and in Studies 2 and 3 I measured it. Lastly, Studies 1 and 3 allowed a comparison of the perceived instrumentality of counterfinal vs. unifinal means, whereas Study 2 involved a comparison of counterfinal means' perceived instrumentality across experimental conditions.

Study 1

In Study 1, I tested the cost heuristic under conditions in which I manipulated both a) the magnitude of the alternative goal undermined by a counterfinal means and b) the perceived ecological validity of the cost heuristic. Per Hypothesis 1, individuals who do not perceive the cost heuristic as ecologically valid should rate counterfinal and unifinal means as equally instrumental, regardless of alternative goal magnitude. By

contrast, those who do perceive the cost heuristic as ecologically valid should rate counterfinal means as more instrumental than unifinal means, and even more so when alternative goal magnitude is high (vs. low).

In Study 1, I also attempted to show that the perceived ecological validity of the cost heuristic is malleable, and that manipulating it (rather than measuring it) can also produce the predicted interaction effect (with alternative goal magnitude) on the application of the cost heuristic (Hypothesis 1). In other words, it may be possible to experimentally induce people to perceive the cost heuristic as an ecologically valid judgment rule (or not). If these predictions were borne out, Study 1 would demonstrate a causal impact of perceived ecological validity on implementation of the cost heuristic at different levels of alternative goal magnitude, as well as the malleability of perceptions of the cost heuristic's ecological validity. Accordingly, in Study 1, I manipulated participants' perceptions of the cost heuristic's ecological validity by assigning them to read an article that either supports or undermines the cost heuristic's ecological validity.

To my knowledge, there is not yet a measure of the extent to which people perceive the cost heuristic as ecologically valid, which would be useful here as a manipulation check and in subsequent studies as a measure of this construct. Therefore, I designed a new, 4-item measure to use for these studies (see Appendix B), modeled after Dweck's (2000) Theories of Intelligence Scale. Using a Likert scale, participants were asked to rate their agreement with a series of face valid items that describe the cost heuristic and its inverse or null (reverse-scored). In Study 1, I used this measure as a manipulation check on the article-based manipulation of the perceived ecological validity of the cost heuristic.

I also manipulated the magnitude of the alternative goal of quenching thirst by giving some participants pretzels and others water. This manipulation has been successfully used in previous research to induce high vs. low levels of thirst, respectively (Atance & Meltzoff, 2006; Balcetis & Dunning, 2010; Ferguson & Bargh, 2004). Finally, I asked participants to rate the instrumentality of either counterfinal or unifinal means (between-subjects) to the focal goal of drinking tasty beverages. The counterfinal means was a beverage powder that would purportedly increase participants' thirst (i.e., detrimental to the alternative goal), whereas the unifinal beverage powder would purportedly leave participants' thirst at its current level (i.e., unrelated to the alternative thirst-quenching goal).

I predicted that people would apply the cost heuristic under the predicted conditions in Hypothesis 1. Specifically, I predicted that among participants who read an article supporting the cost heuristic (i.e., high ecological validity), those who read about the counterfinal (vs. unifinal) beverage powder would rate it as tastier (i.e., more instrumental to the focal goal). Moreover, this difference should be augmented for participants who ate pretzels (high magnitude of the alternative thirst-quenching goal) vs. those who drank water (low alternative goal magnitude). However, those who read an article undermining the validity of the cost heuristic (i.e., low ecological validity) should rate the counterfinal and unifinal beverage powders as equally tasty, regardless of alternative goal magnitude.

Method

Participants. This study employed a 2 (alternative thirst-quenching goal magnitude: high-pretzels vs. low-water) x 2 (ecological validity of cost heuristic article:

high vs. low) x 2 (means type: counterfactual or unifactorial) entirely between-subjects design. I recruited 391 participants from undergraduate psychology courses to complete this study in the lab in exchange for extra or partial course credit.

Procedure. After providing informed consent, participants completed the study in Qualtrics on a lab computer. Participants first read that we were conducting a marketing research study on consumer perceptions of a new, FDA-approved beverage powder. Participants were also told that they would be asked to make some consumer ratings of the beverage powder and other products, and would then have the opportunity to taste the beverage powder in some water. We also asked participants to refrain from consuming any other foods or beverages during the study.

Participants were then asked to complete a reading comprehension and writing task. In the first task, participants were randomly assigned to read an article ostensibly from *Science* that either supports (high ecological validity condition) or undermines (low ecological validity condition) the ecological validity of the cost heuristic. Those in the high ecological validity condition read about scientists' recent discoveries that reveal the accuracy of common adages like "Great sacrifices lead to great rewards." Those in the low ecological validity condition read about scientists' recent discoveries that show no correlation whatsoever between the effort, time, or other costs people incur and their success in pursuit of whatever goal they were trying to achieve. After reading their assigned article, participants were asked to respond to several reading comprehension items to lend credence to the cover story, including an attention check on the perceived ecological validity manipulation.

All participants were then asked to write a brief essay to make the focal goal of drinking tasty beverages salient. Specifically, participants were asked to write about why drinking good tasting beverages is important and to list some positive things about drinking tasty beverages, as well as some negative things about drinking bad tasting beverages.

Participants then received their first consumer product to taste test and rate, which constituted the manipulation of the alternative thirst-quenching goal magnitude. Participants in the high thirst goal magnitude condition tasted Bachmann's pretzels from a 1 oz. individual serving package, whereas those in the low thirst goal magnitude condition tasted Nestle Purelife water from an 8 oz. bottle. We asked all participants to consume at least half of their consumer product, and reminded them to refrain from consuming any other foods or beverages during the study. Participants then tasted their assigned consumer product and rated it on several qualities (e.g., tastiness, appeal) to give credence to the cover story. When participants finished their ratings and we removed their consumer product, we also noted whether participants followed the instructions about eating/drinking at least half the pretzels/water.

Next, participants were told that we would like for them to rate their perceptions of a new, FDA-approved beverage powder. They were also told that they would not actually taste the beverage powder before they make these ratings, because we wanted to see how consumers would perceive the powder before they actually buy or try it; however, they would have the opportunity to try the beverage powder later. At this point, participants were randomly assigned to read about and rate various qualities of either a counterfactual or unifactual beverage powder. All participants were told that the beverage

powder had a pleasant taste. Participants in the counterfactual condition read that the beverage powder made people thirstier or increases people's thirstiness. Participants in the unifactual condition read that the beverage powder was not thirst-quenching, but it doesn't make people any thirstier either, and thus leaves people at their current level of thirst.

Participants were then asked to rate several qualities they anticipated the beverage powder would have. Using a slider bar that ranged from *Not at all* to *Extremely*, participants rated how much they anticipated the beverage powder would be tasty, good tasting, refreshing, filling, smooth in texture, and healthy, as well as how likely they would be to buy the beverage powder. As a manipulation check on the means type manipulation, they were also asked to rate how thirsty they thought the beverage powder would make them compared to their current level of thirst, using a 7-point scale from 1 (*Very much LESS thirsty than I am*) to (*Very much MORE thirsty than I am*).

Participants then completed manipulation checks on the alternative thirst goal magnitude manipulation, along with several filler items. Specifically, they rated on a 7-point scale from 1 (*Not at all*) to 7 (*Extremely*) how much they felt each of the following states right now: thirsty, refreshed, healthy, hungry, bloated, and content. Participants then rated their commitment to and the personal importance of the alternative goal (quenching their thirst) and focal goal (drinking tasty beverages), along with several filler goals (e.g., eating low-calorie foods). Participants gave the commitment ratings on a 7-point scale from 1 (*Not committed*) to 7 (*Very committed*) and the importance ratings on another 7-point scale from 1 (*Not at all important*) to 7 (*Extremely important*). Participants then completed the 4-item measure of the perceived ecological validity of the

cost heuristic as shown in Appendix B, as a manipulation check on the perceived ecological validity manipulation.

Participants then received a new 8-oz. water bottle and were told that they could drink as much of it as they would like while they complete the following questionnaires. This second water bottle actually served as another manipulation check on the thirst goal magnitude manipulation; those in the pretzel condition should drink more from the second water bottle than those in the water condition. Participants then completed several demographic items, including gender, birth year, and year in school, and completed a suspicion check. Finally, participants were told to drink as much they'd like from the second water bottle if they had not already done so, and then give the bottle to the experimenter. Participants were then debriefed concerning the nature and the purpose of the study.

Results

I excluded 44 participants (11.25%) who failed to follow instructions on the eating/drinking or essay tasks, who failed the attention check on the ecological validity of the cost heuristic manipulation, or who came close to guessing the hypothesis (i.e., guessed that the article on sacrifices was applied to the taste-thirst goals in the beverage paradigm). As the primary dependent measure for all hypothesis tests, I computed participants' mean score on two items, in which they rated how a) tasty and b) good-tasting they thought their assigned beverage powder would be. These two items were highly correlated, Spearman's $\rho(343) = .86, p < .001$.

Manipulation/attention checks.

Alternative goal magnitude. As a manipulation check on the alternative thirst goal magnitude manipulation, I conducted two independent t tests. I first computed the mean of three thirst items participants rated just prior to consuming a bottle of water at the end of the study: a) how thirsty they were, b) how committed they were to quenching their thirst, and c) how important it was to them to quench their thirst. These three items were all significantly correlated (Spearman's $\rho_{s[345]} \geq .27, p_s < .001$). In my first manipulation check t test, I treated participants' mean thirst rating as the dependent variable. Participants in the high thirst condition ($M = 5.65, SD = 0.98$) gave a higher mean thirst rating than those in the low thirst condition ($M = 4.45, SD = 1.15$), $t(339.10) = 10.43, p < .001$, Cohen's $d = 1.12$. In my second manipulation check t test, I found that participants in the high thirst condition ($M = 68.96, SD = 70.78$) had fewer milliliters of water left in (i.e., drank more from) their final water bottle than those in the low thirst condition ($M = 138.72, SD = 78.56$), $t(319) = 8.35, p < .001$ Cohen's $d = 0.93$. The alternative thirst goal magnitude manipulation was effective.

Ecological validity of the cost heuristic. I also conducted an independent t test as a manipulation check on the ecological validity of the cost heuristic. The 4-item measure of the ecological validity of the cost heuristic had marginal reliability in this study ($\alpha = .62$). The reliability was improved but still marginal ($\alpha = .69$) when Item 2 was dropped from the measure. Therefore, I conducted an independent t test to evaluate the efficacy of the article manipulation of the perceived ecological validity of the cost heuristic, using the mean score on Items 1, 3, and 4 as the outcome variable. This analysis showed that the manipulation was effective. Participants in the high ecological validity condition ($M = 3.38, SD = 0.87$) scored higher on the ecological validity measure than those in the low

ecological validity condition ($M = 2.67$, $SD = 0.89$), $t(345) = 7.49$, $p < .001$, Cohen's $d = 0.81$.⁵

Means type (counterfinal vs. unifinal). Lastly, I conducted an independent t test on the continuous, 7-point attention check item for the counterfinal vs. unifinal beverage powder manipulation. Participants who read about the counterfinal beverage powder ($M = 5.16$, $SD = 1.12$) rated the beverage powder as making people thirstier than they currently are, relative to those who read about the unifinal beverage powder ($M = 3.66$, $SD = 0.90$), $t(345) = 13.75$, $p < .001$, Cohen's $d = 1.48$.

Hypothesis test. To test the hypothesis, I conducted a 2 (alternative goal magnitude: high-pretzels vs. low-water) x 2 (ecological validity of cost heuristic: high vs. low) x 2 (means type: counterfinal or unifinal) entirely between-subjects design ANOVA on ratings of the beverage powders' tastiness. None of the main effects or interaction terms were significant ($ps > .15$). The main effect of the alternative goal magnitude manipulation was closest to significant ($p = .15$) but is not meaningful with regard to the hypothesis (all other $ps > .27$). The planned analysis did not support Hypothesis 1.

Hypothesis test, controlling for reverse halo ("spillover") effects. In one final hypothesis test, I statistically controlled for a possible reverse halo effect in participants' ratings of the beverage powders. Specifically, participants may have rated the counterfinal (vs. unifinal) beverage powders less positively on all qualities, including instrumentality to a focal goal, due to the counterfinal beverage powder's (negative)

⁵ Although the manipulation checks were successful, I conducted an internal analysis to retest the hypothesis, using as predictor variables the mean-centered measures (rather than manipulations) of a) alternative goal magnitude and b) perceived ecological validity of the cost heuristic. This internal analysis also failed to support my hypothesis. Only the main effect of alternative goal magnitude was significant ($p = .01$; all other $ps > .25$), but this result is uninformative with regard to my hypothesis.

detrimentality to the alternative goal. Indeed, participants who read about the counterfinal beverage powder ($M = 41.58$, $SD = 13.74$) rated it less positively on all qualities (i.e., tasty, good taste, refreshing, filling, smooth, healthy, likely to buy) on average than those who read about the unifinal beverage powder ($M = 44.91$, $SD = 14.24$), $t(345) = 2.22$, $p = .03$, Cohen's $d = 0.24$. Thus, retesting the hypothesis while statistically controlling for participants' ratings of the beverage powders on other qualities (besides tastiness) should tease out any reverse halo effects and allow the cost heuristic to emerge (if at all) in ratings of the beverage powders' tastiness (i.e., instrumentality to the focal goal).

Accordingly, I again conducted a 2 (alternative goal magnitude: high-pretzels vs. low-water) x 2 (ecological validity of cost heuristic article: high vs. low) x 2 (means type: counterfinal or unifinal) entirely between-subjects design ANOVA on ratings of the beverage powders' tastiness, but this time with the mean ratings of the beverage powders on all other qualities (excluding tastiness items) entered as a covariate. The only effects were a marginally significant main effect of means type ($F[1,337] = 3.24$, $p = .07$, partial $\eta^2 = .01$) and a significant effect of the covariate, general positivity scores ($F[1,337] = 78.31$, $p < .001$, partial $\eta^2 = .19$). The pattern of means revealed that participants who read about the counterfinal beverage powder ($M = 62.98$, $SE = 1.28$) rated it as marginally tastier than those who read about the unifinal beverage powder ($M = 59.66$, $SE = 1.31$), after controlling for mean ratings on other qualities of the beverage powders. No other interactions or main effects were significant ($ps > .15$). Although this finding did not support Hypothesis 1, it did provide marginal support for the general inference that counterfinal means are more instrumental than unifinal means to a focal goal.

Discussion

The results of Study 1 did not support Hypothesis 1, which predicted a three-way interaction among alternative goal magnitude, the perceived ecological validity of the cost heuristic, and the type of means with which participants were presented (counterfinal vs. unifinal). Instead, the data marginally supported a simpler version of the cost heuristic than I anticipated in my revised, more fine-grained hypotheses. Specifically, when I statistically controlled for a possible reverse halo effect in participants' ratings of the counterfinal means, participants perceived counterfinal (vs. unifinal) means as marginally more instrumental to the focal goal (i.e., tasty). Although this finding is supportive of a general cost heuristic that participants applied irrespective of the new, proposed moderators (alternative goal magnitude and the perceived ecological validity of the cost heuristic), the pattern was only marginally significant. This result should thus be interpreted with caution, and in Study 2 I attempted to provide additional support for either the general (unmoderated) cost heuristic that was weakly evinced in Study 1 (as well as my preliminary swine flu study), or the moderated version of the cost heuristic proposed in Hypothesis 1.

The manipulation and attention checks were all successful and the effect sizes quite large, suggesting that the null effects for the interaction hypothesis test were probably not due to ineffective or weak manipulations. However, range restriction on the alternative goal magnitude variable may have been an issue in this study; participants' mean scores on the alternative goal magnitude measure (thirst quenching) were within 3 points of the maximum on a 7-point scale, with standard deviations of approximately 1 scale point. Thus the null findings with regard to alternative goal magnitude may have

been due to limited range such that most scores were fairly high even in the “low” alternative goal magnitude condition.

Another possibility is that, although the manipulation check on the ecological validity variable suggested that its induction was effective, the observed differences across conditions might have been due to experimental demand rather than due to genuine differences in the perceived ecological validity of the cost heuristic. That is, the article manipulation may have been too obvious, and the manipulation check may not have reflected changes in participants’ true beliefs about the cost heuristic. In Studies 2 and 3, I measured the perceived ecological validity of the cost heuristic rather than manipulating it. Thus, if a faulty manipulation was responsible for the null findings in Study 1, then Studies 2 and 3 may yield results consistent with my hypotheses.

One other noteworthy finding appeared in the results of Study 1. Namely, the reliability of the perceived ecological validity of the cost heuristic measure was only marginally acceptable, but improved somewhat after dropping Item 2. In reviewing the item content, it seems that Item 2 is conceptually a bit different from the three other items that comprise the measure. Specifically, whereas the other three items explicitly ask whether costly means are especially effective for pursuing a goal, Item 2 asks whether costly and non-costly means are equally effective for pursuing a goal. Low scores on this item (which was reverse-scored) could indicate that participants believe that either costly *or* non-costly means are more effective. It could therefore indicate support for the cost heuristic *or* for its inverse; the item does not distinguish between the two. Therefore, Item 2 may prove problematic throughout the three studies in which I employed the cost heuristic measure, a possibility that I will explore in each subsequent study.

Study 2

In Study 2, I sought to provide an additional test of Hypothesis 1, which predicted that participants who believe in the cost heuristic will be more likely to apply it under high (vs. low) alternative goal magnitude, but that those who do not believe in the cost heuristic will not apply it regardless of alternative goal magnitude. Although both Studies 1 and 2 tested this hypothesis, Study 2 differed from and extended Study 1 in several ways. First, in Study 2, I measured the perceived ecological validity of the cost heuristic as a continuous predictor variable, rather than manipulating it as in Study 1. Second, I did not manipulate means type (counterfinal vs. unifinal) in Study 2, but rather asked all participants to rate only the counterfinal means' instrumentality to the focal goal. Therefore, in this study I predicted that the cost heuristic would manifest as differential ratings of counterfinal means' instrumentality across experimental conditions. Third, in this study I employed new focal and alternative goal domains of national security and governmental transparency.

Moreover, I attempted to demonstrate that the cost heuristic patterns predicted in Hypothesis 1 would be invariant to the specific contents of the focal and alternative goals. Specifically, in a pretesting session, I preselected participants who reported high focal goal magnitude in the domain of either a) national security or b) governmental transparency. Each participant's alternative goal was thus in the remaining domain, whichever was not pretested as their focal goal. In Study 2, I manipulated the magnitude of participants' alternative goal and measured the extent to which they believed the cost heuristic was ecologically valid. Participants then rated a counterfinal means' instrumentality to the focal goal. If all participants exhibited the cost heuristic under the

hypothesized conditions, regardless of the specific contents of their focal and alternative goals, this would bolster the argument that the cost heuristic is a general rule.

Importantly, in Study 2, I added an instruction that was intended to help attenuate or prevent reverse halo effects, such as those that may have occurred in Pilot Studies 1 and 2, as well as Study 1. Specifically, I asked participants in Study 2 to rate the counterfactual means' instrumentality to the focal goal without regard to its relationship with any other goals. This instruction was intended to make participants more conscious of their (possible) tendency to generalize the negative detrimental quality of counterfactual means to ratings of its instrumentality to a different goal. Although Study 1 afforded a statistical control of reverse halo effects that appeared at least partially successful, this new instruction in Study 2 provided a methodological control that could prevent reverse halo effects altogether.

Per Hypothesis 1, I predicted an interaction effect of alternative goal magnitude and the perceived ecological validity of the cost heuristic on ratings of counterfactual means' instrumentality to the focal goal. Specifically, I predicted that those who scored higher on the perceived ecological validity of the cost heuristic would rate counterfactual means as more instrumental to a focal goal under high (vs. low) alternative goal magnitude. By contrast, the counterfactual means instrumentality ratings of those who scored lower on the perceived ecological validity of the cost heuristic would not be impacted by alternative goal magnitude. I also predicted that this interaction would not be moderated by the specific contents of the focal goal.

Method

Participants. This study employed a 2 (alternative goal magnitude article: high vs. low) x 2 (alternative goal domain: national security vs. governmental transparency) entirely between-subjects design, with the perceived ecological validity of the cost heuristic measured as a third, continuous predictor variable. I recruited 258 University of Maryland undergraduate students to complete this study online in exchange for extra or partial course credit.

Procedure. In either an online mass testing survey or an online Part 1 survey (both of which also included filler questionnaires), participants were asked to rate their commitment to several goals, including among them national security and governmental transparency, using items 5a – 10a from the goal commitment questionnaire shown in Appendix A, excluding the certainty items (K. Fujita, personal communication, July 3, 2012). I preselected participants for this study on the basis of their scores on these measures. Participants who had a raw mean commitment score of 5 or higher on a 7-point scale for either focal goal domain (national security or governmental transparency) were invited to participate in the study. Whichever goal they scored 5 or above on was assigned to be their focal goal domain for Study 2; the other goal was assigned to be their alternative goal domain.⁶

Participants were told that the study was investigating political attitudes or beliefs. After providing informed consent⁷, participants were told that they were preselected to participate in this study because they indicated in pretesting that they were highly

⁶ Participants who scored 5 or higher on both goals during pretesting were invited to participate and then randomly assigned to one of the two focal goal domain conditions.

⁷ Participants who completed the pretest in mass testing gave consent for this study at this point, just before the essay, as mass testing requires separate consent as a different study altogether. Participants who completed the pretest for this study as Part 1 in a two-part study gave consent online before completing Part 1 for both parts.

committed to their focal goal domain (national security or governmental transparency). They were then asked to write an essay explaining why a specific focal goal within their pretested focal goal domain (national security or governmental transparency) is so important. The focal goal in the national security domain was “concealing top secret information”; in the governmental transparency domain, it was “exposing personal scandal among government officials”. This essay task was intended to make the focal goal salient. Participants were then randomly assigned to either the high or the low alternative goal magnitude condition, with the alternative goal (and thus the counterfactual means) differing depending on participants’ focal goal.

National security focal goal – Governmental transparency alternative goal. For participants whose focal goal was in the national security domain, the alternative goal was exposing personal scandal among government officials (in the governmental transparency domain). For these participants, the counterfactual means was *concealing* top secret records that implicate governmental officials in personal scandal. This means is instrumental to these participants’ focal goal (concealing top secret information) but detrimental to their alternative goal (exposing personal scandal among government officials).

Some of these participants were assigned to the high alternative goal magnitude condition, and were asked to read an article on the importance of exposing personal scandal among government officials. Others among these participants were assigned to the low alternative goal magnitude condition, and were asked to read an article stating that exposing personal scandals among government officials is not very important. Pilot testing showed that participants who read the high alternative goal magnitude article ($M =$

5.39, $SD = 1.78$) indeed rated the alternative goal as higher magnitude (using the goal commitment measure shown in Appendix A) than did those who read the low alternative goal magnitude article ($M = 3.29$, $SD = 1.39$), $p = .04$. Participants then completed some reading comprehension items, one of which served as an attention check on the alternative goal magnitude manipulation.

Governmental transparency focal goal – National security alternative goal. For participants whose focal goal was in the governmental transparency domain, the alternative goal was concealing top secret information (in the national security domain). For these participants, the counterfactual means was *leaking* top secret records that implicate government officials in personal scandal. This means is instrumental to these participants' focal goal (exposing personal scandal among government officials) but detrimental to their alternative goal (concealing top secret information).

Some of these participants were assigned to the high alternative goal magnitude condition, and were asked to read an article on the importance of concealing top secret information. Others among these participants were assigned to the low alternative goal magnitude condition, and were asked to read an article stating that concealing top secret information is not very important. Pilot testing showed that participants who read the high alternative goal magnitude article ($M = 6.33$, $SD = 0.85$) indeed rated the alternative goal as higher magnitude (using the goal commitment measure shown in Appendix A) than did those who read the low alternative goal magnitude article ($M = 4.42$, $SD = 1.64$), $p = .05$. Participants then completed some reading comprehension items, one of which served as an attention check on the alternative goal magnitude manipulation.

Dependent measures. Participants were then asked to complete a series of questionnaires, with most of the items intended to distract from the true nature and purpose of the study. The first of these questionnaires contained the target items concerning the instrumentality of counterfinal means to the focal goal (concealing top secret information or exposing personal scandal among government officials). These items were followed by numerous filler items concerning the instrumentality of various means to such political goals as LGBT rights, reducing illegal immigration problems, and reducing the government deficit. All means instrumentality ratings asked participants to use a slider bar to provide numerical probability estimates from 0% to 100% for each means' probability of achieving the listed focal goal. Participants were also instructed that they should make these focal goal instrumentality ratings for each means disregarding its helpfulness or harmfulness to other goals to which it might be related. This instruction was intended to methodologically prevent or attenuate any reverse halo effects for ratings of counterfinal means.

Next, participants completed the 4-item measure of the perceived ecological validity of the cost heuristic shown in Appendix B.

Participants then rated their commitment to the alternative, focal, and filler goals as a manipulation check on the alternative goal magnitude manipulation, using the same 6-item scale that was used for preselecting participants (see Appendix A).

Participants next completed a demographic questionnaire, including their birth year, year in school, political orientation, religious affiliation, and race, as well as a suspicion check. Finally, participants were fully debriefed on the nature and purpose of the study.

Results

I excluded 37 participants (14.34%) from subsequent analyses who met at least one of the following criteria: a) had duplicate IP addresses and initials; b) did not write anything for the essay prompt; or c) failed the attention check on the alternative goal magnitude manipulation. No participants guessed the study hypothesis, or that the study involved applying the cost heuristic to the behaviors rated in the study.

Similarly to Study 1, the measure of the perceived ecological validity of the cost heuristic had unacceptable reliability ($\alpha = .43$), but was improved to marginal acceptability with the removal of Item 2 ($\alpha = .65$). Thus in all subsequent analyses that involved this measure, I used the mean score on Items 1, 3, and 4.

Manipulation check. I conducted a manipulation check on the efficacy of the alternative goal magnitude manipulation within each of the alternative goal domain conditions. Specifically, I conducted a 2 (alternative goal domain) x 2 (alternative goal magnitude manipulation) ANOVA on participants' mean score on the 6-item alternative goal magnitude measure. The two-way interaction was significant, $F(1,217) = 5.10$, $p = .03$, partial $\eta^2 = .02$. The alternative goal magnitude manipulation was not effective in the national security alternative goal domain condition (where Hypothesis 1 was not supported). Participants in this condition did not differ in alternative goal magnitude scores across the high ($M = 4.20$, $SE = 0.18$) and low ($M = 3.91$, $SE = 0.19$) alternative goal magnitude conditions, $p = .26$. By contrast, the alternative goal magnitude manipulation was effective in the governmental transparency alternative goal domain condition (where Hypothesis 1 was supported). In this condition, those in the high ($M =$

3.86, $SE = 0.20$) vs. low ($M = 2.72$, $SE = 0.19$) alternative goal magnitude condition scored higher on alternative goal magnitude, $p < .001$.

This analysis also revealed two significant main effects (both of which were qualified by the above interaction) of goal domain ($F[1,217] = 16.83$, $p < .001$, partial $\eta^2 = .07$) and alternative goal magnitude ($F[1,217] = 14.43$, $p < .001$, partial $\eta^2 = .06$).

Hypothesis test. I conducted a 2 (alternative goal magnitude article: high vs. low) x 2 (alternative goal domain: concealing top secret information vs. exposing personal scandal among government officials) entirely between-subjects ANOVA, with mean-centered scores on the 3-item cost heuristic measure entered as a third, continuous predictor variable, on focal goal achievement probability ratings for the counterfactual means. The three-way interaction was significant, $F(1,207) = 4.49$, $p = .04$, partial $\eta^2 = .02$. I proceeded to test the simple effects of alternative goal magnitude as a function of the perceived ecological validity of the cost heuristic, within each goal domain.

Where exposing personal scandal among government officials was the alternative goal, the results are consistent with Hypothesis 1. Participants who scored high on the ecological validity measure (1 SD above the mean) rated the counterfactual means as providing a higher probability of focal goal achievement under high ($M = 60.15$, $SE = 5.35$) vs. low ($M = 40.75$, $SE = 4.92$) alternative goal magnitude, $p = .01$. Those who scored low on the ecological validity measure (1 SD below the mean) rated the counterfactual means as providing an equivalent probability of focal goal achievement under high ($M = 52.65$, $SE = 5.39$) and low ($M = 44.07$, $SE = 6.15$) alternative goal magnitude, $p = .30$. Figure 4 displays this pattern of results.

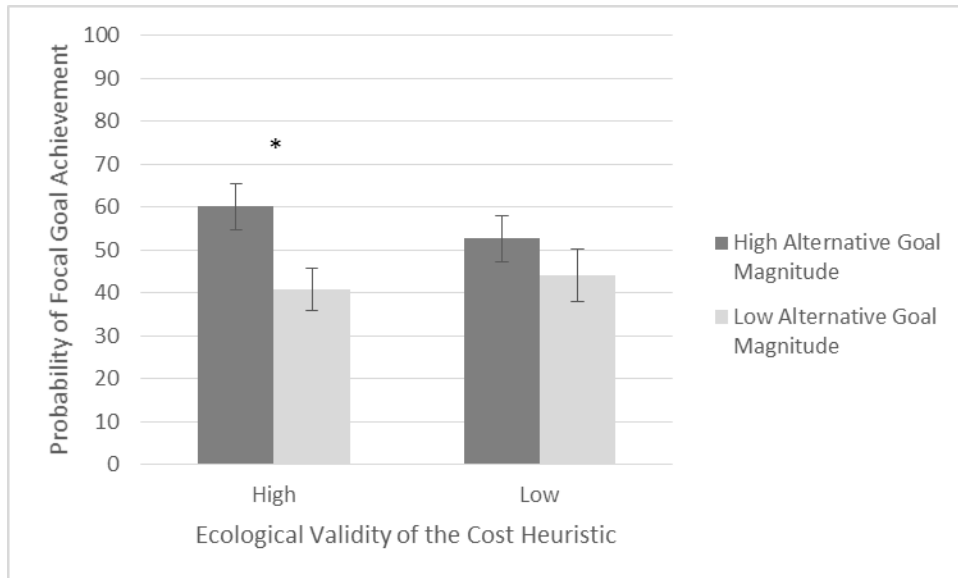


Figure 4. Ratings of counterfactual means' focal goal achievement probability as a function of alternative goal magnitude and the perceived ecological validity of the cost heuristic. Results displayed are within the alternative goal domain of “exposing personal scandal among government officials”.

Where concealing top secret information was the alternative goal, neither of the simple effects was significant, and the trends were not supportive of the hypothesis. Participants who scored high on the ecological validity measure (1 *SD* above the mean) rated the counterfactual means as providing equivalent probability of focal goal achievement in the high ($M = 69.38$, $SE = 4.76$) and low ($M = 79.53$, $SE = 5.68$) alternative goal magnitude conditions, $p = .17$. Likewise, those who scored low on the ecological validity measure (1 *SD* below the mean) rated the counterfactual means as providing equivalent probability of focal goal achievement in the high ($M = 86.31$, $SE = 5.19$) and low ($M = 75.51$, $SE = 4.46$) alternative goal magnitude conditions, $p = .12$.

A marginally significant interaction of domain and alternative goal magnitude also emerged ($F[1,207] = 3.43$, $p = .07$, partial $\eta^2 = .02$), as well as a significant main

effect of domain ($F[1,207] = 58.70, p < .001, \text{partial } \eta^2 = .22$) and a marginally significant main effect of alternative goal magnitude ($F[1,207] = 3.76, p = .05, \text{partial } \eta^2 = .02$). However, these lower-order effects were qualified by the three-way interaction. The remaining interaction and main effects were all non-significant ($ps \geq .26$).

The simple effects underlying the three-way interaction supported Hypothesis 1 in one domain, namely where exposing personal scandal among government officials was the alternative goal. However, both simple effects were non-significant where concealing top secret information was the alternative goal.

Discussion

The results of Study 2 provided partial support for Hypothesis 1, but only in one alternative goal domain. Specifically, manipulation checks revealed that the alternative goal magnitude manipulation was only effective in one goal domain (governmental transparency). In this domain, participants who perceived the cost heuristic as high in ecological validity rated counterfactual means as more instrumental to a focal goal under high (vs. low) alternative goal magnitude, just as predicted. In other words, when the *cue* underlying the cost heuristic (i.e., detrimentality to an alternative goal) was more salient due to greater alternative goal magnitude, those who believed in the cost heuristic rated counterfactual means as more effective. However, those who perceived the cost heuristic as low in ecological validity rated counterfactual means as equally instrumental to a focal goal across alternative goal magnitude conditions. These results were exactly as predicted in Hypothesis 1. However, in the other alternative goal domain (national security), neither the hypothesis test nor the manipulation check was successful.

The efficacy of the alternative goal magnitude manipulation in one domain (governmental transparency), but not the other (national security), is noteworthy and occurred despite successful pilot testing of the manipulations. Perhaps participants' concern with national security (as compared with their concern for governmental transparency) was relatively stable and resistant to manipulation, resulting in the null effects observed within this goal domain in Study 2. The null manipulation check for the national security alternative goal does not appear to be due to ceiling or floor effects, as participants' scores on the manipulation check for national security were near the scale midpoint under both high and low alternative goal magnitude.

In addition to the ineffective national security alternative goal magnitude manipulation, the findings of Study 2 could also be due to participants' preconceived notions about the counterfactual means they rated in this condition. In the national security alternative goal domain, participants may have had strong prior beliefs about the counterfactual means that overrode any inferences consistent with the cost heuristic hypothesis. Indeed, the counterfactual means in this goal domain was *leaking* top secret records that implicate government officials in personal scandal, whereas in the other goal domain it was *concealing* top secret records that implicate government officials in personal scandal. Perhaps participants had stronger notions about leaking top secret records than about concealing them, and these stronger beliefs overrode any cost heuristic inferences in the former condition.

Despite this possibility in the national security alternative goal domain, it is unlikely that any prior beliefs about the counterfactual means in the governmental transparency alternative goal condition would have produced the moderation pattern

predicted (and obtained) in Hypothesis 1. As such, it would be difficult to argue that the cost heuristic hypothesis is patently incorrect. Rather, it may be that the cost heuristic (as with any heuristic) is one of many factors that might impact the perceived instrumentality of means, and if strong prior beliefs about those means exist, they could override the cost heuristic. However, where one is lacking strong prior beliefs about a means, for instance when a novel counterfactual means to a goal is introduced, the cost heuristic may be more likely to emerge.

As an additional note, the full 4-item measure of perceived ecological validity of the cost heuristic was again unreliable in Study 2. By dropping Item 2, the reliability was improved to marginal acceptability, as in Study 1. The replication of this result further suggests that Item 2 was flawed and should be eliminated from the measure.

Study 3

Studies 1 and 2 provided tests of Hypothesis 1, which concerns the role of alternative goal magnitude in people's application of the cost heuristic. In Study 3, I tested Hypothesis 2, which concerns the role of cognitive resources in the application of the cost heuristic. In Study 3, I measured the perceived ecological validity of the cost heuristic as a continuous predictor variable, and I manipulated participants' cognitive resources using a depletion task. Specifically, participants engaged in either an easy or a difficult digit rehearsal task while completing their ratings of means' instrumentality to a focal goal. This manipulation of cognitive load has been used extensively in previous research (e.g., Gilbert & Hixon, 1991; Gilbert & Osborne, 1989; Van Dillen & Derks, 2012; Van Dillen & Koole, 2009).

I then asked participants to rate the instrumentality of both counterfinal and unifinal means (within-subjects) to a focal goal, across two vignettes that each contained different contents of means and goals. This study thus provides an additional test of the hypothesis that the cost heuristic is a general rule that people apply under the predicted conditions regardless of the specific contents of the focal and alternative goals. I also included in Study 3 a similar instruction as in Study 2 to prevent or attenuate reverse halo effects in participants' ratings of the means.

Per Hypothesis 2, cognitive resources should interact with the perceived ecological validity of the cost heuristic to impact the extent to which people exhibit it. Specifically, individuals who do not perceive the cost heuristic as ecologically valid should rate unifinal and counterfinal means as equally instrumental to a focal goal under low cognitive load, but counterfinal (vs. unifinal) means as more instrumental under high cognitive load. By contrast, those who do perceive the cost heuristic as ecologically valid should rate counterfinal (vs. unifinal) means as more instrumental to a focal goal, regardless of their level of cognitive load. This pattern should not be moderated by the specific contents of the goals in the two vignettes.

Method

Participants. This study employed a 2 (digit rehearsal task: difficult vs. easy) x 2 (means type: counterfinal or unifinal) x 2 (goal domain: war vs. charity) mixed design, with depletion task manipulated between-subjects and means type and goal content within-subjects, and the perceived ecological validity of the cost heuristic measured as a continuous predictor. I recruited 166 participants from undergraduate psychology courses to complete this two-part study in exchange for extra or partial course credit.

Procedure. Participants were told that the study was investigating their multitasking abilities. They completed Part 1 of the study online and then went to the lab at least one week later to participate in Part 2. After providing informed consent, participants completed the measure of the perceived ecological validity of the cost heuristic (see Appendix B) along with filler questionnaires in an online survey comprising Part 1 of the study. They also provided some identifying information so that we could match their Part 1 scores on the ecological validity measure to their data from Part 2 of the study.

Participants then went to the lab at least one week later to complete Part 2. Because the procedures involved memorizing a number string as a cognitive load manipulation, the researcher instructed participants to place all personal possessions in a closed cabinet or back corner of the room until the end of the study session. This was intended to discourage participants from recording the number instead of actively rehearsing it during the study. The experimenter also explicitly instructed participants that it is very important that they do not “cheat” by writing down the number somewhere, but instead actively rehearse it and try to remember it during the study. Participants were further instructed that they would have to report their number out loud to the researcher, which meant that attempts to record the number and read from a cheat sheet would be obvious to the experimenter. In addition, the study was conducted using Media Lab and Direct RT software, which takes over the whole computer screen during the study, so that participants could not record their number on the computer screen.

Participants first read a brief vignette that was intended to make the cost heuristic accessible; they were told this was a reading comprehension task. This article described

an undergraduate student (Jamie) with a variety of filler characteristics (e.g., major, relationship status). It also mentioned that Jamie has a few foundational beliefs: “for instance, Jamie believes that great sacrifices yield great rewards across many domains in life, so Jamie sometimes incurs costs to some personal goals in the belief that it will provide progress toward other personal goals.” This sentence was intended to make the cost heuristic an accessible rule to participants; there was no evaluative information provided as to the goodness-badness or accuracy-inaccuracy of Jamie’s belief in the cost heuristic. Participants then responded to several reading comprehension questions about Jamie, one of which served as an attention check on the salience of the cost heuristic.

Next, participants were randomly assigned to complete either an 8-digit (difficult) or 1-digit (easy) digit rehearsal task, which was intended to manipulate their cognitive resources to high vs. low levels, respectively. Participants in the difficult digit rehearsal task condition received instructions explaining that an 8-digit number string would appear on the next screen, and they would have 25 seconds to memorize it. The instructions further explained that they would need to remember this number string while they completed a series of questionnaires about their personality and some facts from the passage they just read, after which they would be required to accurately report the entire number string to the experimenter. Their goal was to be as accurate as possible on both tasks: the questionnaires and remembering the 8-digit number. Participants in the control condition received the same instructions, except that they were asked to memorize a 1-digit number string. When participants clicked to the next screen, a randomly generated 8- or 1-digit number string appeared for 25 s.

When the number string disappeared, participants were asked to read two short vignettes, each one describing two means (counterfinal and unifinal) to a shared focal goal. The two vignettes involved the domains of charity and war, and the order of the vignettes was counterbalanced across participants. In the charity domain, the focal goal was getting volunteers for Wounded Warriors, and the alternative goal was getting volunteers for a local food bank. In the war domain, the focal goal was killing terrorists, and the alternative goal was minimizing civilian casualties. The counterfinal means listed in each vignette was detrimental and the unifinal means unrelated to the alternative goal. The two vignettes are shown in Appendix C.

Importantly, each vignette noted that the cost of the counterfinal plan was new information to the organization, and that they were planning to consider it in upcoming meetings. This step helped to rule out an alternative explanation for any heightened instrumentality ratings on the counterfinal means in this study. Namely, if the counterfinal plans' costs were previously known to the organizations and yet they were still considering each counterfinal plan, then the organization must believe or know that the counterfinal plans would be particularly effective, enough so to negate or exceed the known costs and justify their continued consideration. This would suggest a different kind of inference than the cost heuristic that could also explain boosted instrumentality ratings for the counterfinal means. However, if the organization had not yet weighed the costs of the counterfinal plans because they just received this information, then participants' efficacy ratings would not be based on any presumed knowledge the organization had about the counterfinal plans' efficacy.

Following each vignette, participants completed a questionnaire allegedly testing their comprehension of the material in the vignette. This questionnaire included several filler items about the filler content in the vignette. It also contained two target items that asked participants to rate the instrumentality of each plan (counterfinal and unifinal) to the stated focal goal. In these items, participants were instructed to provide estimates of the probability that each plan would achieve the focal goal, using any number from 0% to 100%. As in Study 2, participants were instructed to consider only the probability of focal goal achievement in their ratings of each plan's instrumentality, regardless of its impact on the alternative goal. This instruction was again intended to prevent or attenuate reverse halo effects in ratings of the counterfinal plans. In this questionnaire, participants also rated the effect each plan would have on the alternative goal (e.g., increase, decrease, or no effect on number of volunteers at local food banks), as an attention check.

After reading both vignettes and completing the reading comprehension questionnaire that followed each, participants were instructed to go and get the researcher to proceed to the next part of the study. At this time, the researcher asked the participant to recite their number string aloud and recorded the number the participant reported.

The next questionnaire included measures of the importance participants placed on the focal and alternative goals in the two vignettes. Specifically, participants were asked to rate the following two items for each of the two goals in each of the two vignettes (8 items total) on a Likert scale from 1 (*Not at all*) to 7 (*Extremely*): "How important do you think it is to [goal]?" and "How committed are you to [goal]?" For

instance, for the war vignette, the focal goal items were “How important do you think it is to kill terrorists?” and “How committed are you to killing terrorists?”

Participants then completed a brief demographic questionnaire, including gender, race, year in school, and age, as well as a suspicion check. Finally, participants were fully debriefed on the nature and purpose of the study.

Results

I excluded from analyses 52 participants (31.33%) who did not correctly report 80% or more of their assigned digit string, who guessed that the study was testing the application of beliefs about sacrifices to the reading passages/questions, and/or who failed the attention checks.

Similarly to Studies 1 and 2, the measure of the perceived ecological validity of the cost heuristic had unacceptable reliability ($\alpha = .41$). In this study, the reliability was improved with the removal of Item 2 ($\alpha = .58$), but to a lesser extent than in Studies 1 and 2. Thus in all subsequent analyses that involved this measure, I used the mean score from Items 1, 3, and 4.

Manipulation check. I attempted to conduct an independent t test to examine the effect of the cognitive resources manipulation on the percentage of numbers reported correctly. However, all participants in the easy digit rehearsal condition reported their single-digit number correctly; therefore, there was no variability in this group and a t test was not possible. In the difficult digit rehearsal condition, by contrast, only 90% of participants reported their eight-digit number correctly. Thus, it seems based on a descriptive comparison that the cognitive resources manipulation was effective.

Hypothesis test. I conducted a 2 (digit rehearsal task: difficult vs. easy) x 2 (means type: counterfinal or unifinal) x 2 (goal domain: charity or war) mixed ANOVA on focal goal achievement probability ratings for each means, with mean-centered scores on the 3-item cost heuristic measure entered as a third, continuous predictor variable. Digit rehearsal task was a between-subjects variable, and means type and goal domain were both within-subjects variables. The outcome variable was participants' focal goal achievement probability rating for each means type. The hypothesis would have been supported by a significant three-way interaction of the ecological validity measure, digit rehearsal task (i.e., cognitive resources), and means type, with the same pattern occurring in both goal domains (charity and war). The results did not support this hypothesis.

The highest-order, significant interaction that emerged was the two-way interaction of means type and goal domain, $F(1,110) = 16.37, p < .001, \text{partial } \eta^2 = .13$. Simple effects tests revealed that within the charity goal domain, participants rated the unifinal means ($M = 62.86, SE = 2.25$) as providing a higher probability of focal goal achievement than the counterfinal means ($M = 57.82, SE = 2.35$), $p < .05$. By contrast, in the war goal domain, participants rated the counterfinal means ($M = 72.02, SE = 2.16$) as providing a higher probability of focal goal achievement than the unifinal means ($M = 63.73, SE = 2.45$), $p < .001$. Figure 5 displays this pattern of results.

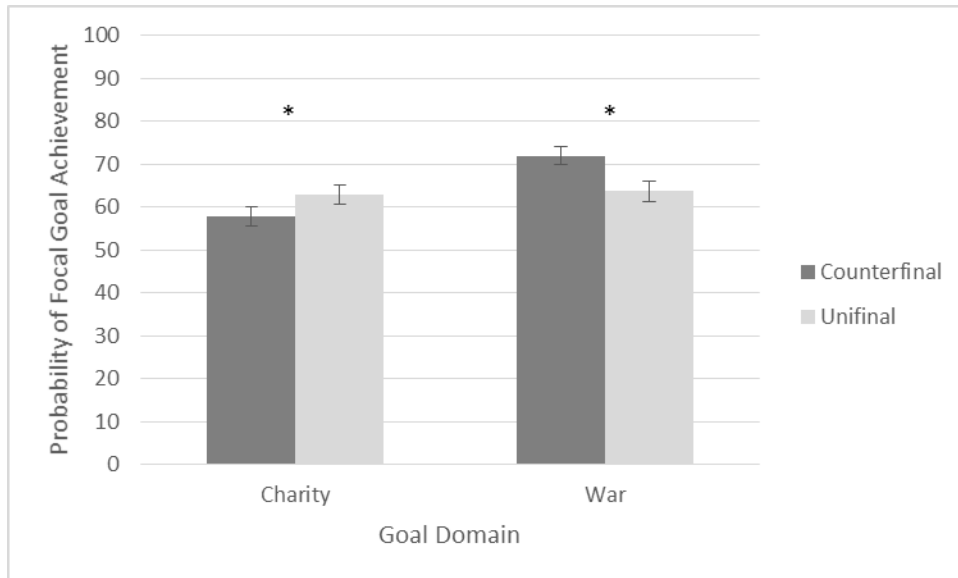


Figure 5. Ratings of focal goal achievement probability for counterfinal and unifinal means in the charity vs. war goal domains.

Two marginally significant two-way interactions also emerged. The first was the interaction of goal domain and digit rehearsal condition, $F(1,110) = 3.02$, $p = .09$, partial $\eta^2 = .03$. Both simple effects underlying this interaction were non-significant ($ps \geq .34$); thus I do not interpret this interaction further.

The other marginally significant interaction involved means type and the perceived ecological validity of the cost heuristic, $F(1,110) = 3.19$, $p = .08$, partial $\eta^2 = .03$. Simple effects tests revealed that those who scored high (1 *SD* above the mean) on the perceived ecological validity of the cost heuristic rated the counterfinal means ($M = 64.47$, $SE = 2.60$) as providing a marginally higher probability of focal goal achievement than the unifinal means ($M = 59.80$, $SE = 2.81$), $p = .06$. By contrast, those who scored low (1 *SD* below the mean) on the perceived ecological validity of the cost heuristic rated the counterfinal ($M = 65.37$, $SE = 2.60$) and unifinal means ($M = 66.78$, $SE = 2.81$) as

providing equivalent probability of focal goal achievement, $p = .56$. Figure 6 displays this pattern of results.

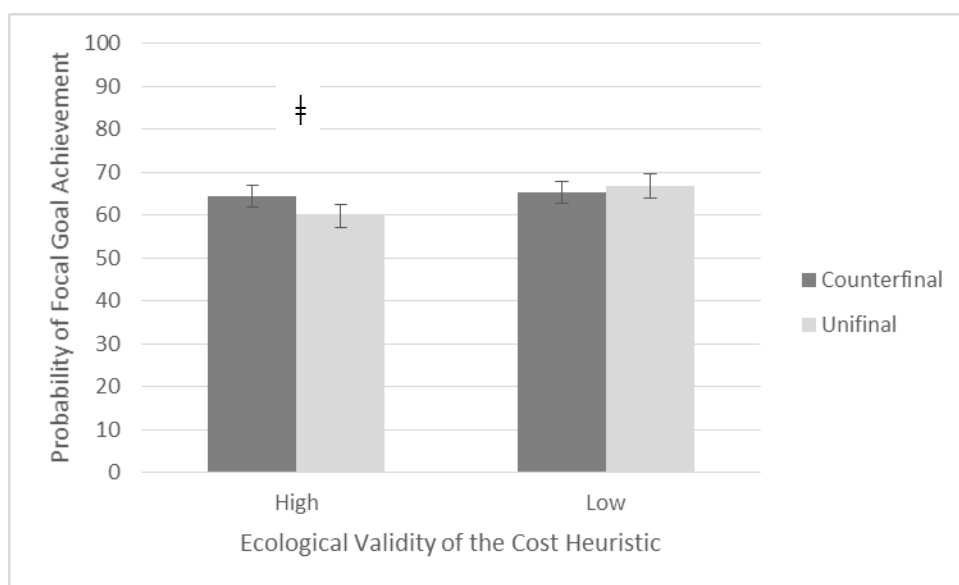


Figure 6. Ratings of focal goal achievement probability for counterfactual and unifinal means among those who scored high vs. low on the perceived ecological validity of the cost heuristic.

A significant main effect of goal domain also emerged, $F(1,110) = 14.81, p < .001$, partial $\eta^2 = .12$. However, this main effect was qualified by the two higher-order interactions of domain with means type and digit rehearsal condition. All other interactions and main effects were non-significant ($ps \geq .14$).

Exploratory analyses. I then conducted two exploratory analyses that tested Hypothesis 2 within each goal domain (charity and war), separately. Specifically, within each goal domain, I conducted a 2 (digit rehearsal task: difficult vs. easy) x 2 (means type: counterfactual or unifinal) mixed ANOVA on focal goal achievement probability ratings for each means, with mean-centered scores on the 3-item cost heuristic measure entered as a third, continuous predictor variable. Digit rehearsal task was a between-

subjects variable, and means type was a within-subjects variable. The outcome variable was participants' focal goal achievement probability rating for each means type.

War goal domain. In the war goal domain, the three-way interaction was marginally significant, $F(1,110) = 2.97, p = .09$, partial $\eta^2 = .03$. Simple effect tests showed that among participants who scored *high* (1 *SD* above the mean) on the ecological validity of the cost heuristic measure, those in the easy digit rehearsal condition rated counterfactual means ($M = 79.87, SE = 4.35$) as providing a higher probability of focal goal achievement than unifinal means ($M = 59.13, SE = 4.93$), $p < .001$. By contrast, those in the difficult digit rehearsal condition rated counterfactual ($M = 66.55, SE = 4.30$) and unifinal means ($M = 61.23, SE = 4.88$) as providing equivalent probability of focal goal achievement, $p = .23$. This pattern is depicted in Figure 7.

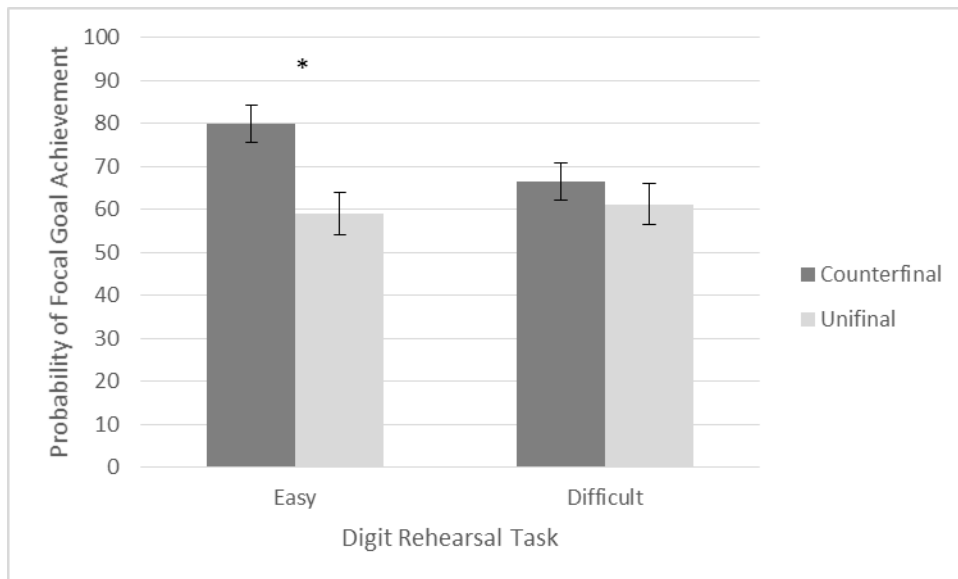


Figure 7. Ratings of focal goal achievement probability for counterfactual and unifinal means in the easy vs. difficult digit rehearsal task conditions. Results depicted are for the war goal domain among those who scored high on the perceived ecological validity of the cost heuristic.

Among participants in the war goal domain who scored *low* (1 *SD* below the mean) on the ecological validity of the cost heuristic measure, neither simple effect was significant. Participants rated counterfinal and unifinal means as providing equal probability of focal goal achievement in both the easy ($p = .41$) and difficult ($p = .44$) depletion task conditions. Hypothesis 2 was not supported in the war goal domain.

Several other interaction and main effects emerged in the war goal domain, but they were all qualified by the three-way interaction. These included a significant interaction of means type and the ecological validity measure ($F[1,110] = 4.54, p = .04$, partial $\eta^2 = .04$), a marginally significant interaction of means type and digit rehearsal condition ($F[1,110] = 3.04, p = .08$, partial $\eta^2 = .03$), and a significant main effect of means type ($F[1,110] = 13.91, p < .001$, partial $\eta^2 = .11$). No other main or interaction effects were significant ($ps \geq .44$).

Charity goal domain. I next conducted the same analysis in the charity goal domain. The only significant effect was a main effect of means type, $F(1,110) = 4.04, p < .05$, partial $\eta^2 = .04$. Participants rated unifinal means ($M = 62.86, SE = 2.25$) as providing a higher probability of focal goal achievement than counterfinal means ($M = 57.82, SE = 2.35$). All other interaction and main effects were non-significant ($ps \geq .15$). Hypothesis 2 was not supported in the charity goal domain; rather, the unifinal means was perceived as more instrumental to the focal goal than the counterfinal means.

Discussion

The results of Study 3 did not support Hypothesis 2 in either the primary hypothesis test or exploratory hypothesis tests conducted separately within each goal domain (charity and war). These null results do not appear to be due to an ineffective

cognitive resources manipulation. Indeed, participants only reported their number string at less than perfect recall when they had eight (vs. one) digits to rehearse. Despite the lack of support for Hypothesis 2, the results of Study 3 revealed several interesting patterns that involved differences in the perceived instrumentality of the counterfinal vs. unifinal means.

Perhaps the most interesting pattern of results from Study 3 emerged as part of the exploratory hypothesis test I conducted within each goal domain separately. Specifically, in the war goal domain, cognitive resources interacted (albeit marginally) with the perceived ecological validity of the cost heuristic to produce differential perceptions of counterfinal vs. unifinal means' instrumentality. However, the patterns underlying this interaction were inconsistent with Hypothesis 2. Instead, I found that the *only* group to rate counterfinal (vs. unifinal) means as more instrumental to a focal goal (thus applying the cost heuristic) was the high ecological validity – high cognitive resources group. Those who did *not* perceive the cost heuristic as ecologically valid did not apply it (regardless of their level of resources), and those who *did* perceive it as ecologically valid *only* applied it when they had ample cognitive resources (see Figure 7).

These results suggest that the vignettes' length or complexity may have obscured the cost heuristic for participants (see Appendix C). Perhaps the filler material in the vignettes made it too difficult, particularly for those under high cognitive load, to detect the critical cue on which the cost heuristic is based: detrimentality to an alternative goal. Absent this cue, the cost heuristic lacks the "if" part of the "if-then" rule, which would prevent participants from applying it. This explanation is consistent with the results. Only those who a) believed in the validity of the cost heuristic and b) had ample

cognitive resources actually applied the cost heuristic. For those under high cognitive load, detecting the detrimentality cue may have been too difficult amid all the filler material in the vignettes. And those who did not believe in the validity of the cost heuristic did not apply it even when they had ample resources, because they did not view it as a useful rule.

The lack of any moderation findings in the charity goal domain could also be explained by the difficulty of detecting the detrimentality cue that was buried in the vignette. Specifically, the charity vignette may have been even more difficult to comprehend than the war vignette, thus producing null results for the majority of effects. Indeed, the charity vignette involved one charity potentially poaching volunteers from another charity, a situation participants may have had more difficulty comprehending than the war vignette's simple juxtaposition of killing terrorists vs. killing civilians. If the charity vignette was so complex as to render even those with ample cognitive resources *and* belief in the cost heuristic unable to detect the detrimentality cue, they may have opted for unifinal means, as observed, based on prior beliefs about the means or perhaps some other unidentified factor.

Despite this possibility, it is worth noting that participants were excluded from the study if they failed attention checks on the detrimentality (or lack thereof) of the counterfinal (vs. unifinal) means in each of the vignettes. Thus if participants only remained in the sample for analysis if they *did* detect the detrimentality of the counterfinal (vs. unifinal) means, then the above explanation would not hold. However, it is possible that some participants remained in the sample for analysis due to guessing on the attention checks, and if these participants comprised a large proportion of the

sample for analysis, this could have skewed the results. Thus it is unclear whether the results could be explained by the difficulty of detecting the detrimentality cue and thus applying the cost heuristic; however this possibility cannot be ruled out.

Another possibility is that the cost heuristic is a more difficult rule to apply than previously anticipated, perhaps due to reverse halo effects' interference with the cost heuristic. I initially assumed that the cost heuristic would be an easy rule to apply because it was based on a single cue: detrimentality to an alternative goal. However, if this same cue could also produce reverse halo effects that would lead to exactly the opposite inference from the cost heuristic (i.e., *lower* instrumentality to the focal goal), then the cost heuristic could be more difficult to apply than I expected. Although I attempted to methodologically prevent reverse halo effects in this study, it is possible that the instruction was underutilized, particularly by participants under cognitive load. Thus, the cost heuristic could be a more difficult rule to apply than I initially anticipated.

Another interesting pattern (see Figure 6) showed a relation between the perceived ecological validity of the cost heuristic and its application. Participants who believed more strongly in the cost heuristic rated counterfinal (vs. unifinal) means as marginally more instrumental to a focal goal. By contrast, those who believed less strongly in the cost heuristic rated counterfinal and unifinal means as equally instrumental. Although this effect was not moderated by cognitive resources as predicted, the pattern nonetheless shows that the ecological validity measure was predictive of participants' use of the cost heuristic. The predictive ability of this measure is encouraging, particularly given its marginal reliability throughout Studies 1, 2, and 3.

A final noteworthy pattern in Study 3 (see Figure 5) revealed that participants rated counterfinal (vs. unifinal) means as more instrumental to a focal goal in the war goal domain, but precisely the opposite in the charity goal domain. One potential explanation for these findings is that, as in Study 2, participants may have had strong prior beliefs about the means in the charity and/or war domains. The means described in the vignettes (see Appendix C) were not explicitly named and were thus not specific acts with which participants could have had any prior experience (e.g., drone attacks). However, it is possible that participants' familiarity with the general content of the goal domains (particularly in the case of the war goal domain) might have led them to apply strong prior beliefs about, say, means that would kill terrorists but increase civilian casualties. It seems less likely that participants would have had prior beliefs about the type of means described in the charity domain, which involved a charity increasing its own volunteer rates by poaching or not poaching volunteers from another charity. However, this possibility cannot be ruled out given the data at hand.

Another possible explanation for this finding is that reverse halo effects occurred in the charity domain (where counterfinal means were rated as *less* instrumental than unifinal means), but not the war domain (where counterfinal means were rated as *more* instrumental than unifinal means), although it is not entirely clear why this would occur. I included an instruction intended to prevent or attenuate reverse halo effects in Study 3, but perhaps this instruction was lost on participants following the somewhat lengthy vignettes, particularly for those under high cognitive load. However, this would not explain why results consistent with a reverse halo effect occurred in one domain but not

the other. Thus it is unclear why the observed reversal pattern for the counterfinal vs. unifinal means occurred across the war and charity goal domains.

As a final note, in Study 3 the measure of the perceived ecological validity of the cost heuristic was once more unreliable using the full 4-item version. By dropping Item 2, the reliability was improved as in Studies 1 and 2, but to a lesser extent, and did not meet conventional thresholds for even marginally acceptable reliability. The replication of this result again suggests that Item 2 should be eliminated from the measure.

General Discussion

Across six studies, I obtained suggestive but inconclusive results concerning the existence of a cost heuristic and the conditions under which it may be particularly likely to emerge. The cost heuristic predicts that if a means is detrimental to alternative goal(s) (i.e., counterfinal), then people might perceive it as especially instrumental to a focal goal. Moreover, I predicted that people would be especially likely to apply the cost heuristic if they believe it is an ecologically valid rule, and particularly when the detrimentality cue on which the rule is based is more salient due to heightened alternative goal magnitude. I also predicted that people who do *not* perceive the cost heuristic as ecologically valid would only apply it if they lack the cognitive resources to search for a (subjectively) better rule. I obtained inconsistent evidence for these predictions of a cost heuristic and the conditions most conducive to its emergence. However, the results of these six studies afforded glimpses of support for the hypotheses, as well as other intriguing patterns that suggest the cost heuristic might exist in a different form and under more restricted conditions than I initially predicted.

Most encouragingly, in one goal domain in Study 2, the results were precisely consistent with my predictions about the moderating role of alternative goal magnitude and perceived ecological validity in the application of the cost heuristic. Participants who perceived the cost heuristic as ecologically valid rated counterfactual means as more instrumental under high (vs. low) alternative goal magnitude, likely because the detrimentality cue that underlies the cost heuristic was stronger. However, ratings of counterfactual means were unaffected by alternative goal magnitude among those who did not perceive the cost heuristic as ecologically valid. This finding, although it only emerged for one goal domain in one of the two studies that tested Hypothesis 1, suggested that the cost heuristic might be worth exploring further.

I also found some support for the notion that individual differences exist in the extent to which people believe the cost heuristic is an ecologically valid rule. Despite the marginal reliability of the ecological validity measure, it did interact with alternative goal magnitude in the hypothesized manner in one goal domain in Study 2. It also predicted participants' application of the cost heuristic in Study 3: participants who scored high on the measure rated counterfactual means as marginally more instrumental than unifinal means, whereas those who scored low rated the two means as equally instrumental. Thus the ecological validity measure I created for these studies seems to have some predictive utility.

Finally, in the preliminary swine flu study, as well as Study 1, I found some support for the general idea that counterfactual means would be perceived as more instrumental than other means types (e.g., multifinal, unifinal). In Study 1, however, I only obtained this result after controlling for a possible reverse halo effect, and even then

it was marginally significant. These rare glimpses of a general cost heuristic inference suggest that the more nuanced Hypotheses 1 and 2, which outline potential moderators of the cost heuristic's application, may be a more viable approach to detecting the cost heuristic, if it indeed exists.

Although the present research produced inconsistent, rare support for the cost heuristic hypothesis, it was highly productive in uncovering intriguing challenges and problems researchers might encounter in trying to document the cost heuristic. Questions raised by the present research also highlighted some areas for theoretical refinement and further exploration of the cost heuristic or similar mechanisms that could explain a variety of costly real-world behaviors. I turn now to a fuller discussion of these limitations and future directions for research.

Limitations and Future Directions

Reverse halo effects. One interesting problem in detecting the cost heuristic was suggested by the results of Pilot Studies 1 and 2, and I attempted to correct for this issue either statistically or methodologically in Studies 1, 2, and 3. Namely, it is possible that a reverse halo effect occurred in ratings of counterfinal means, such that negativity associated with a counterfinal means' detrimentality to alternative goal(s) is generalized to its other qualities, including ratings of its focal goal instrumentality. After statistically controlling for potential reverse halo effects in Study 1, I obtained marginal evidence that counterfinal means are perceived as more instrumental than unifinal means to a focal goal. Similarly, after methodologically controlling for possible reverse halo effects in Study 2, I obtained results in one goal domain that precisely matched my more complex hypothesis about the moderating role of alternative goal magnitude and perceived

ecological validity in application of the cost heuristic. In future research, it may be advisable to control for reverse halo effects when attempting to detect the cost heuristic.

However, it would also be worthwhile to consider and explore the effects of controlling for reverse halo effects, statistically or methodologically, on the ecological validity (in the methodological sense) of any results supporting the cost heuristic. If reverse halo effects occur in more naturalistic settings outside the laboratory, then they may negate any effects of the cost heuristic and produce a zero effect in the real world. The fact that people did apply the cost heuristic to an observable extent in Study 2 when instructed to avoid reverse halo effects suggests that the cost heuristic could be powerful enough to override any reverse halo effects in the real world. However, this possibility would need to be tested in more naturalistic settings.

Another possibility is that reverse halo effects could be mere observer effects, which occur only when participants know their ratings of counterfactual means are being scrutinized by scientists. That is, participants may feel pressured in the lab to rate counterfactual means in a “rational” manner, by rating a counterfactual means negatively on all qualities because it is detrimental to an alternative goal. Indeed, the cost heuristic is a somewhat counterintuitive rule, and so participants who exhibit demand characteristics may do so by rating a counterfactual means consistently with reverse halo effects rather than the less seemingly logical cost heuristic. However, absent any observer effects, participants might be more likely to exhibit the cost heuristic in the real world. This possibility could also be tested by manipulating whether participants expect that experimenters will actually see their ratings.

Difficulty of cost heuristic. Another set of interesting findings that emerged in Study 3 suggested that applying the cost heuristic in this study was more difficult than I expected. One possibility is that the vignettes I used to describe the counterfactual and unifinal means may have been too complex or lengthy, thus rendering it very difficult for participants to detect the detrimentality cue that serves as the basis of the cost heuristic. Another possibility is that the cost heuristic per se is a more difficult rule to apply than I expected, perhaps due to competing reverse halo effects that could negate the cost heuristic.

In future research, it would be advisable to calibrate the amount of cognitive resources needed to detect the detrimentality cue. For instance, researchers could make the detrimentality information more salient (e.g., by reducing filler information), thereby ensuring that the cue is accessible for use in the heuristic rule regardless of cognitive resources. Another solution could be to implement a two-stage experimental model, wherein participants are free of cognitive load when they first learn (i.e., read about) the counterfactual and unifinal means, and cognitive load is then manipulated later during the instrumentality rating phase of the study. The results of a similar study to Study 3, but which implements the suggested steps here, could more clearly demonstrate the ease or difficulty of implementing the cost heuristic.

Prior beliefs about means. In several studies, it is possible that participants had prior beliefs about the type of means I described and asked them to evaluate. Such strong, prior beliefs might have overridden any cost heuristic the participants might otherwise have applied in their ratings of the means. Although I did take steps to try to prevent prior beliefs from influencing participants' ratings of the means (e.g., by using

novel means, such as the beverage powders in Study 1 or nonspecific “plans” different organizations were considering in Study 3), this possibility cannot be entirely ruled out. Future researchers should attempt to make the means as novel as possible to participants so that they lack prior beliefs about the means that might override the cost heuristic. Similarly, researchers could attempt to standardize participants’ *a priori* beliefs about the means, although they would likely want to do so well in advance of the manipulations and ratings of the means’ instrumentality, so that suspicion would not be raised.

Alternative goal magnitude. Two related issues emerged in Studies 1 and 2 with regard to alternative goal magnitude. In Study 2, the manipulation of alternative goal magnitude was ineffective in one of the two goal domains, despite pilot testing suggesting it the manipulation was effective. In this goal domain, the hypothesis was not supported, possibly due to the manipulation failure – however, the hypothesis *was* supported in the other goal domain where the manipulation of alternative goal magnitude was successful. A similar limitation in Study 1 was range restriction on the alternative goal magnitude variable. Without truly low alternative goal magnitude to serve as an appropriate comparison group for high alternative goal magnitude, it may be difficult if not impossible to obtain results consistent with Hypothesis 1. This may be one reason for the failure of alternative goal magnitude to produce results consistent with Hypothesis 1 in Study 1, although it did in Study 2 where range restriction was not a problem. To address both of these issues, future research could involve more extensive pretesting of the alternative goals and their magnitude manipulations, so that researchers can obtain adequate and conceptually meaningful range in magnitude scores.

Ecological validity variable. Another set of related limitations involved the perceived ecological validity of the cost heuristic variable. One such limitation was the low reliability across all three studies of a new measure of the perceived ecological validity of the cost heuristic. Item 2 appeared to be the problematic item, as it could reflect either a belief in the cost heuristic or in its inverse (i.e., that non-costly means are more instrumental than costly means). Before employing this measure in future studies, researchers should strongly consider dropping Item 2 from the measure. They may also wish to sample a larger number of items tapping the cost heuristic construct, in order to increase the measure's reliability. Despite these limitations, I was able to obtain conceptually meaningful results with this measure in Studies 2 and 3, which suggests that it has some predictive utility.

Another limitation involving the ecological validity variable was the possibility that it is a more stable individual differences characteristic than I initially thought. In Study 1, pilot testing and manipulation checks both suggested that the manipulation of the ecological validity variable was successful. However, the possibility cannot be ruled out that participants' responses on these measures were due to demand characteristics, and that the manipulation did not impact participants' true beliefs about the cost heuristic. A subtler manipulation and/or measure of the cost heuristic could allow researchers to explore and calibrate the malleability of the ecological validity variable.

The finding of meaningful, predictive individual differences in the perceived ecological validity of the cost heuristic also suggests that future research into group or cultural differences in this variable could yield interesting results. Research has documented cultural differences in cognitive processes, heuristics, and biases (see Nisbett

& Norenzayan, 2002 for a review), and it is similarly possible that people from certain groups or cultures perceive the cost heuristic as more ecologically valid and are more likely to apply it than are others. For instance, it is possible that more politically conservative or libertarian individuals perceive the cost heuristic as highly ecologically valid, given the constellation of related beliefs such individuals tend to strongly emphasize or espouse (e.g., the value of hard work, personal responsibility, and sacrifice for one's country, religious beliefs, etc.).

Salience assumption. As a new direction, future research could also provide tests of one of the major assumptions about the cost heuristic in the present research. Namely, the salience of the detrimentality cue was presumed to underlie the effects of alternative goal magnitude on participants' application of the cost heuristic. However, none of the studies in the present research provided a direct test of this assumption. Future studies could test the salience of the detrimentality cue under high (vs. low) alternative goal magnitude, perhaps using a word-letter completion task or reaction time task that measures the association of words like "costly" or "harmful" with the counterfactual means.

Social proof. As noted throughout the present research, the results weakly and inconsistently supported the cost heuristic as proposed here. However, it is possible that another, slightly different mechanism is driving the cost heuristic than I proposed, and that I did not obtain consistent support for the cost heuristic because I controlled for this mechanism in at least one study (i.e., Study 3). Specifically, it could be that people are more likely to exhibit the cost heuristic (and possibly less likely to exhibit reverse halo effects) if they have information suggesting that at least some people do actually like the

counterfinal means, despite its costs. Such information might suggest to the rater that people who use the counterfinal means, despite its costs, must have some information about the counterfinal means that the rater does not.

Namely, a social proof mechanism (Cialdini, 2001) might be driving the cost heuristic, such that if at least some people think a counterfinal means is effective, or actually choose or use it, then these people must know or think the counterfinal means is effective enough to counteract or outweigh its costs. This possibility could easily be tested by providing participants with information that 50% of people in a surveyed sample chose the counterfinal means. Notably, I tried to rule out just such a mechanism in Study 3 by informing participants that the organizations in the vignettes were considering the counterfinal and unifinal means before they knew of the counterfinal means' costs. In other words, the organizations had just discovered the counterfinal means' costs and thus did not have any information about its special efficacy that would have led them to keep it in the means set for consideration. Thus a social proof mechanism should not have operated in Study 3, and if this mechanism is responsible for the cost heuristic, then the cost heuristic should not have occurred as strongly as in other studies where I did not make any targeted attempt to rule out the social proof mechanism. Indeed Study 3 produced some of the weakest support for the cost heuristic of all the studies in the present research. Future research could thus explore whether a social proof mechanism produces a stronger cost heuristic effect.

Degree vs. presence of cost. In the present research, the majority of the studies (c.f., Pilot Study 2) only explored the presence vs. absence of cost/detrimentality as a variable, by pitting a counterfinal means against a unifinal means. However, an

interesting direction for future research could be whether a greater *degree* of cost (i.e., more counterfactual means) renders a counterfactual means subject to the same cost heuristic effect as obtained for counterfactual vs. unifactual means. In Pilot Study 2, I tested this hypothesis by comparing means that were more vs. less counterfactual (i.e., more vs. less detrimental to the alternative goal), but I had very low statistical power and accordingly obtained weak results. Future research could provide stronger tests of this possibility.

A related question is whether more costly counterfactual means would yield a stronger cost heuristic effect than less costly counterfactual means. For instance, a drug to treat ED might be perceived as more effective if a possible side effect is a heart attack, rather than a mildly irritating, occasional heart flutter. Future research could test this possibility, as well, in a variety of goal domains.

Domain-specific or general. One other direction for future research would be continued investigation into the domain-specificity or generality of the cost heuristic. As noted in the introduction, the price-quality and effort-efficacy associations (Labroo & Kim, 2009; Shiv et al., 2005) have been extensively documented and seem quite robust. By contrast, the present research produced weak and inconsistent evidence of a cost heuristic in other goal domains. Perhaps the cost heuristic is so evident in the price and effort domains because these constitute simpler or more familiar metrics for judging a means' detrimentality than other goal domains (e.g., detrimentality to national security). It is also possible that the cost heuristic is more robust in the price and effort domains because people have so much experience with working and buying that the cost heuristic is very well-learned for these domains. For other goals that people encounter less routinely, the cost heuristic may be less robust.

It might also be worthwhile and interesting to explore the cost heuristic hypothesis in domains where counterfactual means have already been identified in the psychological literature. For instance, Baumeister and Scher's (1988) notion of tradeoffs is essentially identical to the counterfactual means discussed in the present research. They cite as specific examples of tradeoffs self-handicapping, substance abuse, neglect of personal health care, self-presentation efforts, and shyness. It would be interesting to test whether people who engage in these behaviors also tend to exhibit the cost heuristic in these and other domains. For instance, in the case of shyness, a person who perceives the cost heuristic as ecologically valid might rate behaving shyly as more effective for avoiding embarrassment or rejection (i.e., the focal goal) if the alternative goal (i.e., intimacy or friendship) is more salient.

If the cost heuristic is a more general heuristic or rule that applies across domains (albeit perhaps more robustly in some domains than others), it could unify the large but disconnected literature on a variety of costly behaviors, including for instance Baumeister and Scher's (1988) tradeoff behaviors, costly signaling behaviors (Sosis & Alcorta, 2003), the price-quality association (Shiv et al., 2005), and the effort-efficacy link (Labroo & Kim, 2009). The cost heuristic could also comprise an important component of an even broader model of the antecedents and consequences of costly behaviors, which might also include research on effort justification following engagement in a costly behavior (Aronson & Mills, 1959), as well as the augmentation effect (Kelley, 1971) whereby others infer that an actor who uses a costly means must be highly committed to whatever goal it serves.

Implications for choice. The present research also suggests new research avenues concerning when people will actually *choose* or use counterfactual means. The mere perception that counterfactual means are highly instrumental to a focal goal (i.e., the cost heuristic) is likely insufficient to make them appear rational, much less provoke one to use a counterfactual means. One possibility is a two-stage model of choice of counterfactual means. In Step 1, an individual would first infer the especial instrumentality of a counterfactual means to a focal goal, based on its detrimental relation to an alternative goal (or possibly the social proof mechanism outlined above). Once the especial instrumentality of the counterfactual means is learned in Step 1, the individual could then use this information in Step 2 (the choice stage) to determine which means to use in focal goal pursuit.

For instance, in Step 1 a person might first learn that a drug to treat ED has negative heart-related side effects, and infer based on the cost heuristic (or a social proof mechanism) that the drug must be especially effective for treating ED. However, in Step 2, the person may decide against using this ED drug because heart health is more important to him than treating ED (i.e., the relative magnitude of the alternative vs. focal goals is higher; see Klein & Kruglanski, 2013; Kruglanski, Klein, & Belanger, 2013). This highlights an interesting irony: Although greater alternative goal magnitude may lead one to perceive counterfactual means as highly instrumental in Step 1 (as hypothesized and shown in Study 2), it may prevent one from *choosing* counterfactual means in Step 2. Future research could continue to test this and related ideas, such as when a counterfactual means might be perceived as more or less extreme (Klein & Kruglanski, 2013).

Conclusion

Across six studies, I obtained suggestive but inconsistent evidence of a cost heuristic and the conditions most conducive to its emergence. Although this evidence is by no means conclusive, it does make it difficult to conclude that the cost heuristic simply does not exist. Previous research, including glimpses in the present research, suggests that across a variety of domains and goal contents, people often do perceive costly means as especially effective. The present research suggests that although researchers have not yet established the best paradigm to elicit the cost heuristic, improvements and refinements to both the theory and methodology could yield more promising and interesting results. An understanding of the reasons that people engage in counterfactual means for goal pursuit could elucidate a host of costly real-world phenomena, from eating disordered behaviors to hunger strikes to suicide attacks. The present research thus provides preliminary steps toward an organizing framework to help unify the myriad costly or extreme behaviors people undertake every day.

Appendix A

Please respond to the following items by choosing the number that reflects what you think.

*Note: Items 2 and 3 were reverse-scored.

1) Ignoring all the negative aspects, how positive is it to maintain a healthy diet?

1	2	3	4	5	6	7
Not at all positive						Extremely positive

2) Ignoring all the positive aspects, how negative is it to maintain a healthy diet?

1	2	3	4	5	6	7
Not at all negative						Extremely negative

3) How conflicted do you feel about maintaining a healthy diet?

1	2	3	4	5	6	7
Not at all conflicted						Extremely conflicted

4) Overall, how would you rate your feelings toward maintaining a healthy diet?

1	2	3	4	5	6	7
Negative			Mixed			Positive

5a) How important is maintaining a healthy diet to you personally?

1	2	3	4	5	6	7
Not at all important						Extremely important

b) How certain are you about this rating?

1	2	3	4	5	6	7
Not certain						Very certain

6a) How deeply do you care about maintaining a healthy diet?

1	2	3	4	5	6	7
Not deeply						Very deeply

b) How certain are you about this rating?

1	2	3	4	5	6	7
Not certain						Very certain

7a) How concerned are you about maintaining a healthy diet?

1	2	3	4	5	6	7
Not concerned						Very concerned

b) How certain are you about this rating?

1	2	3	4	5	6	7
Not certain						Very certain

8a) How committed are you to the goal of maintaining a healthy diet?

1	2	3	4	5	6	7
Not committed						Very committed

b) How certain are you of this rating?

1	2	3	4	5	6	7
Not certain						Very certain

9a) How would you evaluate the goal of maintaining a healthy diet?

1	2	3	4	5	6	7
Not at all positive						Extremely positive

b) How certain are you of this evaluation?

1	2	3	4	5	6	7
Not certain						Very certain

10a) How personally relevant is the goal of maintaining a healthy diet?

1	2	3	4	5	6	7
Does not affect me						Affects me a great deal

b) How certain are you of this rating?

1	2	3	4	5	6	7
Not certain						Very certain

Appendix B

Instructions: This questionnaire has been designed to investigate people's beliefs about different actions for pursuing their goals. There are no right or wrong answers. We are interested in your ideas.

Using the scale below, please indicate the extent to which you agree or disagree with each of the following statements by writing the number that corresponds to your opinion in the space next to each statement.

1	2	3	4	5	6
Strongly disagree	Disagree	Mostly disagree	Mostly agree	Agree	Strongly agree

1. The most effective actions for pursuing a goal are actions that are costly to some other goal.
2. When you're pursuing a goal, the costliest action will give you the same chance of achieving the goal as a non-costly action would.
3. Of all the actions you could use to pursue a goal, the one that requires the most sacrifice will be the most effective.
4. A path that requires great sacrifice will not necessarily provide great progress toward a goal.

Appendix C

(Charity:) A local charity, Wounded Warriors, that provides services to wounded veterans at Bethesda Medical Center is considering the top contenders among several advertising campaigns to attract new volunteers. Their advisory board, which consists entirely of seasoned volunteers and experts on veterans' affairs, plans to vote on the plans at their next open meeting. The president of the organization has announced that the board will take questions and comments prior to their deliberations in a "town hall," to be held at the Bethesda American Legion next Friday at 7:00pm. The two projects they are considering are Campaign Enlist and Campaign Mobilize, each of which would be at least somewhat successful at attracting new volunteers for the Wounded Warriors. The primary difference between the two, which the organization just discovered and plans to consider at the town hall on Friday, is their effect on volunteer numbers at local food banks. Campaign Enlist would reduce the number of volunteers at local food banks, whereas Campaign Mobilize would have no impact on the number of volunteers at local food banks. The board will meet on Friday night to decide, in light of this new information, which of the two programs will be more effective at attracting new volunteers to Wounded Warriors. Fox Five DC, your choice for local news, will have updates for you on the board's decision and its implications in the coming weeks.

(War:) In the so-called "War on Terror," the U.S. military and intelligence agencies use a variety of methods for attacking and killing known terrorists, but they are constantly looking for new and more effective ways of accomplishing

that end. Just this weekend, sources close to top officials at the Pentagon leaked news of an extensive program of research at DoD laboratories around the country, all to test newer and better ways to kill terrorists on foreign soil. Two of the most promising methods to emerge from this study are known as Alpha attack, more formally labeled in redacted Pentagon documents as Plan 01-0971246, and Charlie attack, formally known as Plan 01-0971485. However, the DoD has just released some new information from their study about these two attack methods. Alpha attacks would kill more civilians than the currently used combat methods, whereas Charlie attacks would maintain the current rate of civilian casualties in combat. A DoD panel plans to meet with the research team soon to evaluate the two plans in light of this new information. Attempts to contact DoD and intelligence officials have been met with “No comment,” but stay tuned in the event that more leaks occur to Fox Five DC, your source for the most current news.

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