ABSTRACT

Title of dissertation: MORE USEFUL, OR NOT SO BAD? EVALUATING THE EFFECTS OF INTERVENTIONS TO REDUCE PERCEIVED COST AND INCREASE UTILITY VALUE WITH COLLEGE PHYSICS STUDENTS

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In the present study I developed and evaluated the effects of two interventions designed to target students' motivation to learn in an introductory college physics course. One intervention was designed to improve students' perceptions of utility value and the other was designed to reduce students' perceptions of cost. Utility value and cost both are central constructs from Eccles and colleagues' expectancy-value theory of motivation (Eccles-Parsons et al., 1983). Students (N = 148) were randomly assigned to receive the cost intervention, the utility value intervention, or one of two control conditions. Compared to a survey control condition, neither intervention impacted overall students' motivation, measured at 3 time points over the semester, or their course outcomes. In moderation analyses, neither intervention impacted any students' perceptions of utility value. However, both interventions impacted some students' perceptions of cost, competence-related beliefs, and course outcomes positively while impacting these variables for other students negatively. The cost intervention benefitted consistently and

in different ways students who had low baseline competence-related beliefs, low prior achievement, strong malleable beliefs about intelligence, or who were female. However, the intervention showed consistent undermining effects on motivation and/or achievement for students with strong fixed beliefs about intelligence. The utility value intervention benefitted consistently the course outcomes of students who had low baseline competence-related beliefs, low prior achievement, or who were female. The intervention showed less consistent undermining effects on motivation for students with strong fixed beliefs about intelligence, high baseline competence-related beliefs, or high prior achievement. Prior researchers have shown that utility value interventions improve course outcomes for some students who are at risk for underachievement. The present study extends prior work by showing that utility value interventions benefit similar students in college physics courses. It also demonstrates that a cost intervention is a viable way to impact at-risk students' physics course outcomes. Future researchers should consider carefully moderating variables and how to mitigate potential undermining effects for some students when implementing future expectancy-value-theory-based interventions in college physics courses.

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by

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Chapter 1: Introduction

Statement of Problem

During the next few decades, the United States will need more professionals who have skills and knowledge in the domains of science, technology, engineering, and mathematics (STEM; National Science Foundation, 2014; U.S. Congress Joint Economic Committee, 2012). Unfortunately, despite years of effort to increase participation, too few students complete college or graduate degrees in many STEM fields, particularly engineering and computer science (Olson & Riordan, 2012; U.S. Congress Joint Economic Committee, 2012; Xue & Larson, 2015). It is important to encourage more students to take courses and pursue college majors in these fields, both in order to fuel economic growth and because these are important domains for scientific innovation (National Science Board, 2015). Many political groups and corporations have recognized the importance of this issue and have designated funding and other resources to address it at the college and K-12 levels.

College is one critical point at which many students decide no longer to pursue STEM coursework (Chen, 2013; Crisp, Nora, & Taggert, 2009). The National Center for Education Statistics reported that 48% of students who intended to pursue a bachelor's degree in a STEM field, and 69% of students who intended to pursue an associate's degree in one, dropped out or switched majors before doing so (Chen, 2013). Based on this and other information, the President's Council of Advisors on Science and Technology has suggested that increasing retention of college students in these majors by even ten percent would much alleviate the projected shortfall of qualified STEM professionals in the U.S. (Olson & Riordan, 2012).

Students' poor performance in introductory college STEM courses is one of the strongest predictors of their attrition from STEM majors (Chen, 2013; Crisp et al., 2009; Rask, 2010; Strenta, Elliott, Adair, Matier, & Scott, 1994). Furthermore, researchers have reported that many students leave STEM majors because they find their introductory STEM course material to be boring compared to the material they have in other areas (Seymour & Hewitt, 1997; Strenta et al., 1994). Other students often do not believe they are competent to complete successfully their work in these introductory courses (Lent et al., 2003; Strenta et al., 1994). Some educational policy organizations such as the President's Council of Advisors on Science and Technology (Olson & Riordan, 2012) have recommended that professors include more active, hands-on learning in introductory college STEM courses. They posit that changing the curriculum in these and other ways will make students more engaged in the courses, perform better in them, and take more related courses in the future (Olson & Riordan, 2012).

Another way to address students' retention in STEM fields is to attempt to improve their motivation for introductory STEM courses. As noted above, many students leave STEM majors for motivational reasons, such as perceiving that introductory course material is difficult or boring. Furthermore, many researchers have shown that college students with less adaptive motivation for STEM courses often have lower achievement in those courses and participate and engage less with their course material (e.g., Acee & Weinstein, 2010; Musu-Gillette, Wigfield, Eccles, & Harring, 2015; Perez, Cromley, & Kaplan, 2014; see Chapter 2 for full discussion). Targeting students' motivation in introductory STEM courses may be an effective way to ensure that students participate and perform well in these courses. This could make them more likely to choose or remain in STEM majors. I focused on this approach in the present study.

Defining Motivation

Currently, many researchers view students' motivation as resulting from certain beliefs, values, and goals that they have. Motivation ultimately influences these students' actions in achievement and other settings (see Eccles & Wigfield, 2002, for review). One theoretical model describing beliefs, values, and goals is the Eccles-Parsons et al. (1983) Expectancy-Value Theory (EEVT); this theory provided the basis for the present study. According to EEVT researchers, students' motivation to pursue different achievement tasks is determined most directly by two constructs. First are students' expectancies for success for a given task. Second is the extent to which students value an achievement task. Eccles-Parsons et al. (1983) posited that task value has three components: students' inherent enjoyment of a task (i.e., intrinsic value), beliefs about whether the task is important to one's sense of self (i.e., attainment value), and beliefs about whether the task is useful (i.e., utility value).

Eccles-Parsons et al. (1983) also wrote that students might perceive cost for different achievement tasks, which could make them less motivated to complete those tasks. They described three dimensions of cost: the cost of effort exerted on a task, the loss of valued alternatives students have as a result of choosing a certain task, and the psychological cost of task failure. In recent years, EEVT researchers have refined how these dimensions are defined and have suggested additional dimensions of cost (Flake, Barron, Hulleman, McCoach, & Welsh 2015; Wigfield & Eccles, 2000). I adopted their broader understanding of cost in the present study (see Chapter 2 for full discussion).

Researchers have shown in many different studies that students' expectancies, other competence-related beliefs defined in the EEVT model, task value, and cost predict their achievement in STEM courses, participation and engagement in those courses, and intentions to take more courses in STEM fields or major in those fields (see Barron & Hulleman, 2015; Wigfield, Tonks, & Klauda, 2016, for reviews). Students' expectancies for success, other competence-related beliefs, and the components of task value typically predict these outcomes positively, whereas cost predicts these outcomes negatively.

I chose the Eccles-Parsons et al. (1983) EEVT framework to guide the present study because it models the antecedents of students' motivational beliefs, task value, and perceived cost. Thus EEVT is useful for understanding how educational interventions might affect motivational constructs and academic outcomes. Another reason for choosing this model is that many researchers have successfully improved students' STEM outcomes after implementing motivation interventions aimed at improving their utility value. I discuss this work next.

EEVT-Based Intervention Research and Its Limitations

Over the last several years many researchers have targeted one or more of the constructs from EEVT in interventions that have improved students' STEM course participation and achievement (see Rosenzweig & Wigfield, 2016, for review). Most of this work has targeted students' perceptions of utility value for a particular course. Researchers often target utility value because it is thought to be more amenable to external manipulation than other constructs in EEVT (Hulleman, 2007; Hulleman, Godes, Hendricks, & Harackiewicz, 2010). The most common type of utility value intervention asks students to make connections between the material in one of their courses and their own lives. Researchers posit that utility value interventions are effective because they encourage students to generate ideas about how their course material is relevant to them. This helps them connect more to the material, which can increase their perceptions of utility (Hulleman et al., 2010). To date, utility value interventions focused on relevance have improved college students' course achievement in various STEM fields including biology and mathematics (Harackiewicz, Canning, Tibbetts, Priniski, & Hyde, 2016; see Tibbetts, Harackiewicz, Priniski, & Canning, 2016, for review).

Utility value intervention findings are promising, but they are only one way of attempting to increase students' course taking, achievement, and participation in STEM fields. Few researchers have developed or evaluated the effects of interventions that target any other constructs from EEVT. Thus, there is little work that reports on the effects of any other EEVT-based interventions. Cost is a worthwhile construct to target in intervention work, for several reasons. Students' perceived cost of engaging in academic tasks negatively predicts their STEM achievement and course-taking intentions (Conley, 2012; Perez et al., 2014). An intervention focused on reducing the cost students experience in STEM courses might be equally effective as a utility value intervention at improving students' achievement and participation

Another important issue with respect to motivation interventions is that all students do not always benefit equally well from them. This can limit the generalizability of interventions' effects (Rosenzweig & Wigfield, 2016). Many researchers have found that moderating variables constrain the effects of utility value interventions, with the most frequent moderators being students' prior levels of expectancies or other competence-related beliefs and prior levels of achievement (Harackiewicz et al., 2016;

Hulleman et al., 2010; Hulleman & Harackiewicz, 2009; Hulleman, Kosovich, Barron, & Daniel, 2016; see Rosenzweig & Wigfield, 2016, for review; see Chapter 2 for full discussion). Researchers have not reached clear conclusions about how each of these moderating variables is likely to affect the results of utility value interventions. Because most college students report experiencing cost in STEM courses, many students might benefit from a cost intervention, whereas fewer students might benefit from support for utility value. Alternatively, some students might benefit more from a cost intervention but different students benefit more from a utility value intervention.

Purpose of the Proposed Studies, Research Questions, and Hypotheses

To extend the EEVT-based intervention literature, I developed and tested an intervention designed to reduce students' perceptions of cost in an introductory college physics course. I also developed and tested an intervention designed to raise students' utility value for the physics course. I compared each of the two intervention conditions to two types of control conditions to determine whether the interventions showed effects on students' motivation, physics course achievement, physics course participation, or future physics course-taking patterns. In one control condition, students only completed surveys. In the other, students summarized what they were learning in physics. I included both of these control conditions in order to understand whether interventions providing motivation support showed similar effects when they were compared to a condition in which students completed no activity versus a condition in which students completed an activity providing cognitive support (see Chapter 3 for discussion).

I tested the cost and utility value interventions in introductory physics because it is a usually a required course for students who will major in the natural sciences, engineering, or the computer sciences. These are fields with high attrition rates during college (Chen, 2013). Thus students' performance and participation in introductory physics are likely to be key determinants of whether they pursue STEM majors in fields where more qualified professionals are needed (Seymour & Hewitt, 1997). Few researchers have addressed students' motivation for introductory college physics specifically. However, many researchers have demonstrated that college students' expectancies, task value, and cost predict their performance in mathematics, engineering, and natural science courses, and these are similar to physics (e.g., Musu-Gillette et al., 2015; Nagy, Trautwein, Baumert, Köller, & Garrett, 2006; Perez et al., 2014; see Chapter 2). Thus EEVT-based interventions administered in introductory physics seemed likely to improve students' performance in physics and their likelihood of continuing to take engineering, natural science, or computer science courses in the future.

Next, I outline broadly my research questions and hypotheses. I return to them in more detail in Chapter 3, after describing my methodology and analysis strategy. I note that all of my hypotheses were based on using the survey control condition as a control group. I did not make specific hypotheses regarding how the intervention conditions would compare to the summary condition as a control group, because prior utility value intervention research has reported that this condition can improve students' achievement in some circumstances (Rosenzweig et al., 2017b).

My first research question was:

1. How did cost or utility value interventions impact the following focal motivational constructs at three time points during the semester, compared to either a survey control or summary condition?

a. **Utility value in physics**

b. **Three dimensions of cost (task effort, outside effort, and emotional cost) and overall cost in physics**

To answer this question, I administered cost and utility value intervention conditions, and two control conditions, to college physics students. I used a 4-condition (cost intervention, utility value intervention, survey control condition, summary condition) between-subjects experimental design. I measured students' self-reported utility value and cost at three time points: immediately after the intervention, one month later after they completed an intervention refresher activity, and at the end of the semester. The immediate post-intervention measurements allowed me to explore whether the interventions affected the motivational constructs that they intended to target at each session. The end-of-semester measurement allowed me to measure whether the interventions affected any motivational constructs after some time had passed.

Based on EEVT and previous research (e.g., Gaspard, Dicke, Flunger, Brisson, et al., 2015; Harackiewicz et al., 2014; Rosenzweig et al., 2017a; Walton & Cohen, 2007, 2011), I hypothesized that at all three time points, students who received the utility value intervention would report higher utility value compared to students in the survey control condition at all three time points. However, they would not report lower perceived cost. In contrast, I hypothesized that students who received the cost intervention would report lower cost compared to students in the survey control condition. However, they would not report higher utility value.

My second research question was:

2. How did cost or utility value interventions impact the following non-focal motivational beliefs and task value components at each of three time points during the semester, compared to either a survey control or summary condition?

a. **Competence-related beliefs in physics**
b. **Intrinsic value in physics**
c. **Attainment value in physics**

The interventions administered in the present study were both grounded in EEVT. Therefore it was possible that they might affect students' expectancies for success, other competence-related beliefs, attainment value, or interest value. Furthermore, the intervention designed to target cost was newly developed for this study, so it was important to explore whether it would impact other motivational beliefs or components of task value in EEVT. Previous utility value intervention researcher groups have found that their interventions changed components of task value or related constructs. Gaspard, Dicke, Flunger, Brisson, et al. (2015) found that students' intrinsic value and attainment value in math increased following one of the two utility value interventions they administered, and Hulleman and Harackiewicz (2009) found that students' interest in science increased after their intervention. Researchers also have demonstrated that students' perceptions of competence can be impacted by utility value interventions, either positively (Hulleman et al., 2016) or as a function of the intervention methodology and students' initial competence-related beliefs (Canning & Harackiewicz, 2015; see Chapter 2 for discussion). Because there was little information in the literature about these possibilities I did not make direct hypotheses for any of these analyses.

My third research question was:

3. How did cost or utility value interventions impact the following measures of students' achievement in physics, compared to either a survey control or summary condition?

a. **Average quiz scores**

b. **Scores on each exam taken during the semester and average exam scores**

c. **Final course grades**

This question allowed me to evaluate how the different conditions affected three student achievement outcomes in physics. Based on findings from previous research (e.g., Harackiewicz et al., 2016; Hulleman et al., 2010; Hulleman & Harackiewicz, 2009; Walton & Cohen, 2007, 2011), I hypothesized that students who received the cost or utility value interventions would earn higher exam scores, quiz scores, and letter grades in physics compared to students in the survey control condition.

My fourth and fifth research questions were:

4. How did cost or utility value interventions impact the following measures of students' course participation in physics, compared to either a survey control or summary condition?

a. **Average amount of time per assignment students spent on homework**

b. **Students' attendance rates in course discussion sections during the semester**

5. How did cost or utility value interventions impact the following measures of students' course-taking patterns during the semester following the intervention, compared to either a survey control or summary condition?

a. **Whether or not students took the next course in the physics sequence**

b. How many STEM courses students took

EEVT theorists state that the components of task value and the dimensions of cost impact students' persistence in courses and intentions or choices to take certain courses along with their academic performance (e.g., Battle & Wigfield, 2003; Durik, Vida, & Eccles, 2006; Eccles, 1987; Eccles-Parsons et al., 1983; Meece, Wigfield, & Eccles, 1990; Perez et al., 2014). Thus the interventions implemented in the present study could have affected the outcomes just mentioned in addition to students' course achievement.

I hypothesized that students who received either the cost or the utility value interventions would spend more time on homework, have higher attendance rates in discussion sections, and take more STEM courses compared to students in the survey control condition. I did not make specific hypotheses regarding the outcome of students taking the next course in the physics sequence because this was a required course for many students; thus it was unclear how much students' motivation would affect their decisions regarding whether to take the course.

My sixth research question was:

6. Were the results for Research Questions 1-5 moderated by the following student-level characteristics, previous achievement, or motivational constructs?

a. Students' baseline competence-related beliefs in physics?

b. Students' prior achievement in physics?

c. Students' belonging uncertainty in physics?

d. Students' beliefs about whether intelligence is malleable or fixed?

e. Gender (female versus male)

f. **Ethnicity (African American versus European American, Asian American versus European American)**

Answering this research question could provide important information to the field about how generalizable cost and utility value interventions' effects were for different types of students. I did not make specific hypotheses about how any particular moderating variable was likely to affect results, because extant work on moderating variables in utility value intervention research has produced mixed effects. In Chapter 2, I discuss further each of these moderating variables and my rationale for including them.

Although I did not have specific hypotheses about how any moderators would impact results, I acknowledged that some moderators were more likely to have an impact than others. In particular, students' baseline competence-related beliefs and prior achievement frequently have moderated the effects of past utility value interventions (e.g., Hulleman & Harackiewicz, 2009; Hulleman et al., 2010; 2016). Thus, I considered baseline competence-related beliefs and prior achievement to be theoretical moderators that were more likely to impact results than were the other moderators. Gender and ethnicity have been tested as moderators in some utility value interventions (e.g., Gaspard, Dicke, Flunger, Brisson, et al., 2015; Harackiewicz et al., 2016), but their moderating effects on these interventions have been less consistent; students' beliefs about the malleability of intelligence and perceptions of belonging uncertainty have never been tested as moderators in utility value intervention research. I thus considered the other moderators to be exploratory. When presenting the results of the utility value intervention, I discuss separately theoretical and exploratory moderators. I considered all moderators to be exploratory when evaluating results of the cost intervention.

Exploratory mediation analyses. Motivation intervention researchers assume that intervention practices change motivational constructs, which then change students' achievement or other academic outcomes. Most researchers measure whether intervention versus control conditions differed on motivational and/or achievement outcomes. However, fewer researchers have modeled all of these links together in one model (Rosenzweig & Wigfield, 2016). It is important that researchers model together the effects of one or more intervention conditions versus a control condition, subsequent changes to any motivational constructs that are targeted by an intervention, and later changes to academic outcomes (Rosenzweig & Wigfield, 2016). Without evaluating this type of model, researchers cannot determine whether improved motivation caused differences in students' outcomes, or whether some other aspect of an intervention did so (Rosenzweig & Wigfield, 2016). For example, perhaps a utility value intervention improved achievement because students were able to write more about a particular subject in the intervention versus the control condition, not because they found more utility value in that subject.

In the present study I assessed whether the constructs of utility value or perceived cost mediated the relationships between receiving either of the interventions and two academic outcomes that I measured at the end of the semester. I also examined as a mediator students' competence-related beliefs, because, as I noted above, prior research has suggested that utility value interventions might impact competence-related beliefs in addition to utility value (Canning & Harackiewicz, 2015; Hulleman et al., 2016). Consistent with my predicted sample size, I chose to conduct a set of exploratory analyses within the regression framework to provide insight onto mediating effects in this study. My research questions with respect to this analysis were:

E1. Did perceived cost measured at three time points mediate the relationship between receiving the utility value intervention compared to the survey control condition, or receiving the cost intervention compared to the survey control condition, and the following outcomes measured at the end of the semester?

a. **Students' final exam scores**
b. **The number of STEM courses in which students enrolled during the semester following the intervention**

E2. Did perceived utility value measured at three time points mediate the relationship between receiving the utility value intervention compared to the survey control condition, or receiving the cost intervention compared to the survey control condition, and the following outcomes measured at the end of the semester?

a. **Students' final exam scores**
b. **The number of STEM courses in which students enrolled during the semester following the intervention**

E3. Did perceived competence-related beliefs measured at three time points mediate the relationship between receiving the utility value intervention compared to the survey control condition, or receiving the cost intervention compared to the survey control condition, and the following outcomes measured at the end of the semester?

a. **Students' final exam scores**
b. **The number of STEM courses in which students enrolled during the semester following the intervention**

I hypothesized that cost would significantly mediate the effect of receiving the cost intervention versus the survey control condition on each outcome, but utility value would not. Also, utility value would significantly mediate the effect of receiving the utility value intervention versus the survey control condition on each outcome, but cost would not. I did not make specific hypotheses regarding competence-related beliefs as a mediating variable because I did not know whether either intervention would impact competence-related beliefs.

Contributions

This study was expected to contribute to the field in several important ways. The study was the one of the first to explore whether an intervention could be developed and implemented to reduce college students' perceived cost in a STEM field. As I discuss in Chapter 2, some researchers (e.g., Cohen, Garcia, Apfel, & Master, 2006; Cohen, Garcia, Purdie-Vaughns, Apfel, & Brzutsoski, 2009; Walton & Cohen, 2007, 2011) have targeted constructs similar to perceived cost in intervention work. However, only Cromley, Perez, and colleagues (T. Perez, personal communication, January 7, 2016) targeted cost directly, and they have not yet published findings regarding whether their intervention actually changed students' perceptions of cost. The present study also is one of the first to evaluate whether a cost reduction intervention improves students' academic outcomes. Researchers and educational practitioners could build on the results of this initial intervention study to consider whether it is possible to target students' perceived cost in order to improve their achievement or course-taking in introductory college STEM courses.

Second, this study expands upon extant literature by evaluating the effects of a utility value intervention in college physics. As discussed in more detail in Chapter 2, no researchers have evaluated utility value interventions in college physics and it is not clear whether the effects of these interventions observed in other educational settings extend to college physics courses. College physics is an important attrition point with respect to pursuing STEM careers and majors, so it is critical to explore what intervention techniques can improve these students' motivation and performance in this course.

Third, this study implemented cost and utility value interventions in the same educational context. Few prior researchers have compared directly the effects of utility value interventions to interventions targeting other motivational constructs within or outside EEVT (Harackiewicz et al., 2016). This study allowed me to explore whether cost interventions impacted students but utility value interventions did not, or vice versa. Results provide information to the field regarding which interventions seem to be more or less effective when administered with students enrolled in introductory STEM courses.

Fourth, I planned to evaluate possible moderators of interventions' effects to explore whether all students responded similarly to cost and utility value interventions. These results addressed whether one type of intervention was more or less susceptible to the effects of moderating variables than was another. It also provided information regarding which students might benefit or not benefit from different EEVT-based interventions administered in college STEM courses. Although many previous researchers have assessed moderating variables, the influence of specific moderating variables are not clear based on existing research. Thus much more work needs to be done on this topic (Rosenzweig & Wigfield, 2016; see Chapter 2 for further discussion).

Definition of Terms

Motivation. I adopted the definition used by Eccles and Wigfield (2002), stating that motivation stems from the beliefs, values, and goals that relate to action. In this study I utilized the Eccles and colleagues Expectancy-Value Theory of motivation as a guiding framework (EEVT; Eccles-Parsons et al., 1983). This theory specifically focuses on students' expectations for success and their task value as primary influences on their motivation.

Expectancies for success on a task. This term refers to how well students believe they will do on upcoming tasks in the future (Eccles-Parsons et al., 1983; Wigfield & Eccles, 2000).

Ability beliefs. This term refers to how competent students perceive they are to complete a certain task in the present (Eccles-Parsons et al., 1983; Wigfield & Eccles, 2000).

Competence-related beliefs. This is an umbrella term referring to students' beliefs in their competence to complete a given task. As measured in the present study, this term encompasses students' expectancies for success on a task and their ability beliefs. I discuss this construct further in Chapter 2. Competence-related beliefs served as both a moderating variable (when measured at baseline) and an outcome variable (when measured at the end of Sessions 1, 2, and 3) in this study. For clarity, whenever I refer to competence-related beliefs as a moderator and not as an outcome in this document, I use the term "baseline competence-related beliefs."

Task value. This construct refers to students' perceptions of how much they are interested in a task (intrinsic value), find a task to be useful (utility value), or feel that a task is important to them (attainment value; Eccles, 2005).

Cost. Eccles (2005) defined cost as anything a student perceives that they must give up to pursue a task, or the effort they perceive they must put into task completion. Eccles-Parsons et al. (1983) originally posited three specific dimensions of cost: the effort one must put forth on a task (effort cost), the psychological ramifications of failure on a task (psychological cost), or the alternative valued activities students must give up to complete a task (loss of valued alternatives cost). Since then, Wigfield and Eccles (2000) expanded the dimension of psychological cost to be called emotional cost, which refers to any negative emotional or psychological experiences students might have while completing a task. Additionally, Flake et al. (2015) have posited an additional dimension of cost, which is the effort one must put forth on other tasks that interferes with a given task (outside effort cost). I discuss the definition and measurement of cost further in Chapter 2.

Educational intervention. I adopted the definition used by Lazowski and Hulleman (2015), which is "a manipulation implemented by an external agent (i.e., teacher, researcher) that was intended to change students' cognitions, emotions, and/or behaviors" (p. 5).

Course achievement. This term refers to any quantitative indicator of students' performance in a course, including their test and quiz scores and their course grades.

Course participation. This term refers to any manifestation of students' effort in a course, including the extent to which students take part in classroom activities, complete required assignments, and spend time working on a course.

Course-taking patterns. This term refers to students' choices to take more courses in a particular field or to take certain courses in that field.

Chapter 2: Literature Review

As outlined in Chapter 1, the United States needs more qualified STEM professionals in order to remain competitive in the global economy. Unfortunately, too many U.S. college students drop out of STEM majors and thus are not qualified to pursue careers requiring STEM skills (Chen, 2013). As I discuss below, aspects of these students' achievement motivation, including their perceptions of the utility value for a course and of the cost associated with the course, influence their STEM achievement and course-taking. It is worthwhile for researchers to design educational interventions that aim to improve these motivational constructs. In this dissertation study, I compared how interventions targeting college students' perceived cost and utility value affected their motivation, achievement, and course-taking in physics. As I discuss below, I focused on utility value because many intervention researchers have targeted this construct. I focused on perceived cost because it is a particularly promising construct for intervention researchers to target (Barron & Hulleman, 2015).

In this chapter I summarize theory and research that is relevant to the present study. I first describe Eccles-Parsons and colleagues' (1983) Expectancy-Value Theory (EEVT), which provided the framework for understanding students' motivation in this study. In particular, I define utility value and perceived cost and explain how they relate to students' achievement and course-taking in STEM fields. Second, I describe research on EEVT-based interventions in STEM fields, with a focus on interventions targeting utility value and perceived cost. I outline limitations of extant intervention literature and how the present study addressed those. Third, I discuss recommendations regarding how best to design and measure the effects of motivation interventions. Fourth, I describe how the present study was expected to contribute to extant work.

Eccles and Colleagues' Modern Expectancy-Value Theory (EEVT): An Overview

As described in Chapter 1, modern expectancy-value theory as developed by Eccles-Parsons and colleagues (1983) explains how students' motivational beliefs and values and a variety of other influences impact their motivation to pursue achievement tasks and their performance on them (see Figure 1; also see Eccles, 2005; Wigfield & Eccles, 2000; Wigfield et al., 2016). Eccles-Parsons et al. (1983) originally created the EEVT model to explore why women were less likely to pursue math and science careers than men, but researchers have since used it to explain motivation and academic outcomes in many achievement domains. EEVT theorists posit that students' motivation to complete an academic task is determined most directly by their beliefs about their expectancies for success on that task and the facets of the task that make them want to or not want to complete it (i.e., their task value).

EEVT is based on the expectancy-value theory of motivation devised by Atkinson (1957). He posited that a given individual is likely to be motivated to pursue a given task as a result of his probability of succeeding at it (expectancy), and the incentive he receives to do well on it (value). He posited that expectancy was inversely related to value, such that easy tasks were not as valuable as more difficult tasks; individuals' motivation was highest when both expectancy and value were at a moderate level. In their work on EEVT, Eccles and colleagues expanded and refined Atkinson's theory (Eccles, 2005; Eccles-Parsons et al., 1983; Eccles & Wigfield, 2002). The authors defined the constructs that constituted motivation in broader ways. They also posited that expectancies and task value were positively related to one another rather than inversely related. Finally, they more carefully described the antecedents and consequences of different motivational constructs.

One major tenet of EEVT is that expectancies and task value are subjective, or based on each individual's perceptions of a given task. To some, a colorful photo on a physics textbook might cause them to be interested and value the text; to others this photo may be perceived as uninteresting. Similarly, two students might be likely to earn the same score on a physics exam, but one might believe that this score indicates she will do well on this type of exam in the future, whereas the other might believe it indicates he will do poorly. To measure these subjective perceptions, researchers must ask people to report their expectancies and task value rather than use some objectively defined incentive amount or probability of success.

Eccles-Parsons and colleagues (1983) stated that "the overall value of any specific task is a function of three major components" (p. 89). These components are intrinsic value, attainment value, and utility value (see Eccles, 2005, for detailed discussion). *Intrinsic value* concerns how much a student enjoys what he or she gains from completing a task, or how much the student will enjoy doing the task itself. *Attainment value* is how much a student finds the task to be personally important or meaningful to her sense of self. *Utility value* is how useful a particular task is with respect to a student's future plans or personal goals. Eccles and Wigfield (1995) demonstrated that the three components of task value related positively to one another but formed distinct factors.

Students' expectancies for success refer to how well they believe they will do on achievement tasks in the future (Eccles-Parsons et al., 1983). As Figure 1 demonstrates, students' self-concepts of their abilities to complete certain tasks in the present (i.e., their ability beliefs) affect their expectancies. Although expectancies and ability beliefs are conceptually distinct, Eccles and Wigfield (1995) found that items measuring these beliefs did not factor separately. Thus EEVT researchers often measure ability beliefs and expectancies for success together as a single construct. I took this approach in the present study and adopted the umbrella term "competence-related beliefs" to refer to that construct. Sometimes researchers also measure self-efficacy as an indicator of students' competence-related beliefs (see Bandura, 1997, for review of this construct). This is because, although self-efficacy is conceptually distinct from ability beliefs and expectancies, the constructs overlap empirically (e.g., Bong & Skaalvik, 2003). I did not take this approach in the current study, but I reference some researchers that have done so.

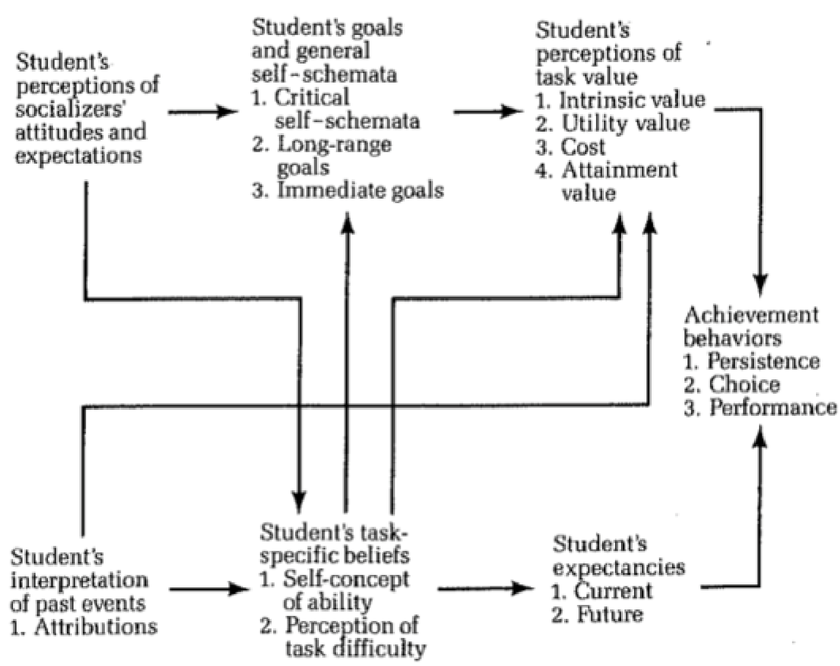

Figure 1. Eccles-Parsons et al.'s (1983) original expectancy-value model of motivation

Eccles-Parsons et al. (1983) also described the construct of perceived cost. Although Figure 1 shows cost as a component of task value, Eccles-Parsons et al. (1983) wrote about this construct as an influence on task value. They noted that students consider a cost-benefit ratio when deciding whether or not to value a particular activity. Broadly, perceived cost refers to anything a student must give up to do a particular task as well as any effort a student must put into task completion (Eccles, 2005). I discuss this construct below in a separate section.

EEVT researchers have posited and shown that competence-related beliefs and task value are positively related to one another, so students who believe that they can do well on a task often also value it more (Eccles & Wigfield, 1995; Jacobs, Lanza, Osgood, Eccles, & Wigfield, 2002; Meece et al., 1990; Wigfield et al., 1997). Conversely, students who believe they will do poorly on a task that they perceive as important may begin to devalue the task to protect their self-worth (Eccles & Wigfield, 2002). Researchers taking both variable-centered and person-centered research approaches to studying individuals' competence-related beliefs and task value have confirmed that most students who have high competence-related beliefs also often tend to have high utility, attainment, and/or intrinsic value, and vice versa (e.g., Conley, 2012; Eccles & Wigfield, 1995; Simpkins & Davis-Kean, 2005). Eccles-Parsons and her colleagues (1983) proposed that expectancies and task value are influenced by different factors and influence many types of academic outcomes (Figure 1). I discuss the academic consequences of students' task value and expectancies in the next section. In terms of antecedents, the most proximal influences on individuals' expectancies for success and task value are their goals, self-schemas, ability beliefs, affective memories, and perceptions about a task. As noted above, many researchers measure together ability beliefs and expectancies, so in this dissertation I refer to the two constructs together as "competence-related beliefs." Goals, schemas, ability beliefs, memories, and perceptions are affected by students' interpretations of previous experiences, and by their broader beliefs and stereotypes about certain academic domains. Cultural norms, socializers' beliefs and values, students' differential levels of ability for different tasks, and other experiences students have had all affect their beliefs and interpretations (Eccles; 2005; Eccles-Parsons et al., 1983; Wigfield, Rosenzweig, & Eccles, in press; Wigfield et al., 2016). The impact of socializers on students' expectancies and values was not directly related to the goals of this study, so I do not discuss it further in this chapter.

A final tenet of EEVT is that adolescents' competence-related beliefs and task value are domain-specific (e.g., Wigfield et al., 2016; Wolters & Pintrich, 1998). Students who believe they are competent at reading may not have the same belief about physics, so EEVT researchers typically assess motivation for one academic domain rather than assessing students' overall competence-related beliefs or task value for school. In this chapter I focus primarily on research that has been conducted on students' competence-related beliefs and components of task value within STEM domains.

I chose EEVT as the theoretical model to guide the present study because it provided a clear description of how students are motivated to learn in a given academic domain. It also provided information about the school and home antecedents of competence-related beliefs and task value, meaning that there was space within the framework to conceptualize how educational interventions might affect these motivational constructs.

How Competence-Related Beliefs and Task Value Predict STEM Outcomes

A very large body of research has shown that individuals' competence-related beliefs and attainment, utility, and intrinsic value positively predict their achievement, course-taking, and other academic outcomes. This is true for male and female students, in elementary school through college, and across many academic domains including STEM (e.g., Durik, Vida, & Eccles, 2006; Musu-Gillette et al., 2015; Pintrich & DeGroot, 1990; Simpkins, Davis-Kean, & Eccles, 2006; see Eccles & Wigfield, 2002; Wigfield et al., 2016, for reviews). These motivational constructs each predict many outcomes, but competence-related beliefs tend to predict more strongly and directly students' achievement outcomes (such as test and quiz scores), whereas attainment, utility, and intrinsic value tend to predict more strongly and directly students' course-taking choices and intentions (e.g., Durik et al., 2006; Eccles, 1987; Eccles-Parsons et al., 1983; Meece et al., 1990). Because individuals' competence-related beliefs and task value relate positively to each other they also have indirect effects on students' academic outcomes. For example, Meece et al. (1990) found that middle and high school students' value of math did not directly predict their grades. However, their task value predicted their expectancies for success in math, which predicted grades.

As noted in Chapter 1, college students' competence-related beliefs and task value specifically affect whether they choose to pursue or leave STEM majors. Musu-Gillette et al. (2015) found that high school students whose levels of interest, utility value, or competence-related beliefs in math remained high through twelfth grade were more likely to pursue a math-intensive college major (also see Wang, 2013). Larson, Wu, Bailey, Borgen, and Gasser (2010) and Malgwi, Howe, and Burnaby (2005) found that college students with higher interest or competence-related beliefs in a particular domain were more likely to major in that domain, including STEM. Similarly, Seymour and Hewitt (1997) interviewed students who left STEM majors and reported that these students' top two reasons for leaving were (a) loss of interest in science and (b) growing interest in other fields (also see Strenta et al., 1994). Finally, Lent et al. (2003) showed that engineering students' competence-related beliefs influenced their amount of persistence in engineering majors.

Perceived Cost as a Critical Influence on STEM Outcomes

In this section I discuss the motivational construct of perceived cost in more detail. Only in the past five to ten years have researchers begun to explore more fully perceived cost in different academic domains. Wigfield and Cambria (2010) noted that perceived cost had been under-studied in the field and should be addressed more. Since then, researchers have begun to expand the definition of cost and develop better ways to measure it, to examine how different students experience different dimensions of cost, and to assess how cost affects students' course-taking, participation or persistence, and achievement in STEM fields. I describe research on each of these topics next (also see Barron & Hulleman, 2015; Flake et al., 2015).

To review the cost literature, I conducted a search for the terms "cost", "motivation", and "education" in the PsycINFO and Google Scholar databases and read through relevant results. I also contacted several researchers studying cost currently and asked them to send up-to-date conference submissions and published papers on the topic. Finally, I searched through the reference lists of review articles written about perceived cost to find relevant literature.

Defining and measuring cost. Eccles-Parsons et al. (1983) described the construct of cost in the chapter introducing EEVT, but they did not explicitly define the term cost. Instead, they noted that students experienced cost and weighed it against benefits to decide whether or not to value an activity. The authors listed three specific dimensions on which students might experience cost: (a) the effort students needed to put forth on a task, (b) the valued alternatives students might give up as a result of choosing to do a task, and (c) the psychological consequences of failure on the task. In subsequent writings, Eccles, Wigfield, and colleagues expanded the notion of psychological cost to incorporate anxiety and emotional experiences that are associated with a task, calling this construct "emotional cost" (Wigfield & Eccles, 2000).

Wigfield and Eccles (1992) defined cost broadly as the perceived negative consequences associated with completing a task. Eccles (2005) provided a more specific definition of cost that encompassed its different dimensions: "What the individual must give up to do a task … as well as the anticipated effort one will need to put into task completion" (p. 113). In writing this definition, she posited that students likely experience many dimensions of cost (also see Flake et al., 2015; Wigfield et al., in press). This is because, at any given time, there are many different factors that might detract from an individual's experience completing a task (e.g., while writing a paper at a coffee shop, a graduate student might experience anxiety about choosing the correct words for her paper, use up time and energy that could be spent with friends, or spend too much money on coffee).

Consistent with the original writings of Eccles-Parsons et al. (1983), researchers most often have described three types of cost: effort cost, emotional cost, and loss of valued alternatives cost. *Effort cost* refers to the amount of effort required by an individual to complete a task; I further differentiate this aspect of cost below. *Emotional cost* refers to the perceived negative emotional or psychological consequences of pursuing a task (e.g., after working for hours on a physics assignment, a student might be frustrated) or of success or failure on that task (e.g., if a student fails a physics test, he might experience guilt). *Loss of valued alternatives cost* refers to students' perceptions of what they cannot do because they are completing the task at hand (e.g., a student studying for a physics exam cannot complete a chemistry assignment at the same time).

Several researchers have developed measures to assess these dimensions of cost (see Wigfield et al., in press, for review). Conley (2012) and Trautwein et al. (2012) both created two-item measures of students' cost perceptions, focusing on effort and loss of valued alternatives cost, and Battle and Wigfield (2003) developed a longer measure that assessed all three types of cost. These researchers confirmed that their cost items were empirically distinct from the items measuring other components of task value (also see Jiang, Rosenzweig, & Gaspard, 2017; Gaspard et al., 2017; Safavian & Conley, 2016). In later research, Perez et al. (2014) adapted Battle and Wigfield's (2003) measure to assess separately the effort, emotional, and loss of valued alternatives dimensions of cost experienced by college students majoring in STEM fields. Gaspard, Dicke, Flunger, Schreier, et al. (2015) also developed a measure to assess these three dimensions of cost, which they validated with German high school students. These researchers used factor analysis to confirm that each of the dimensions of cost was independent from one another as well as from the other constructs in EEVT.

Flake et al. (2015) built on this work by conducting focus groups with college students about their experiences of cost, and by reviewing literature from fields such as behavioral economics to explore dimensions of cost. They determined that effort cost could be further subdivided into two dimensions. *Task effort cost* refers to students' perceptions of the amount of effort that they must put forth to complete a task (e.g., in physics class, an assignment might take students many hours to complete). *Outside effort cost* refers to perceptions of the effort required by other activities, which might impede the student from engaging in the task at hand (e.g., if a biology assignment requires a lot of work, a student is less able to put effort into a physics assignment). The authors developed and validated a measure that assessed the two types of effort cost, emotional cost, and loss of valued alternatives cost. They confirmed that each dimension was independent using factor analysis.

Flake and colleagues also differentiated between objective and subjective cost. Flake et al. (2015) noted that students' *subjective perceptions* of cost are more important than the actual cost experiences that they have in a STEM course. Two students might both work for six hours on a physics homework assignment, but only one student might believe that six hours is "too much" effort, and only that student experiences cost. This implies that intervention researchers can target students' perceptions of their experiences in order to reduce cost rather than changing the actual experiences that induce cost, such as course requirements. Flake et al.'s (2015) measure of cost is worded in such a way to assess whether students experience each dimension of cost as "too much."

In my dissertation study, I adopted Flake et al.'s (2015) conceptualization and measure of cost. It represented the most thorough and up-to-date examination of the construct and its dimensions: task effort, outside effort, loss of valued alternatives, and emotional. As noted by Eccles (2005), Wigfield et al. (in press), Johnson and Safavian (2016), and others, these are not the only dimensions of cost that might impact students' experiences in STEM courses. Other dimensions of cost include financial cost, ego cost, cost based on cognitive interference from thinking about activities one has previously turned down, and cost of needing to do well academically to please others (Hofer & Fries, 2016; Johnson & Safavian, 2016; Wigfield et al., in press). In this study I focused only on the dimensions of cost as they were described and measured by Flake et al. (2015), because they are the most well-understood to date.

Where do researchers conceptualize cost within EEVT? Eccles-Parsons et al. (1983) wrote about cost as a potential mediator of task value, and listed cost under the construct of task value in the figure depicting the model (see Figure 1). However, they also wrote that individuals consider a cost/benefit ratio when deciding whether or not to value an activity. This suggests that the authors might have actually considered cost to be a moderator of task value (Barron & Hulleman, 2015; Wigfield et al., in press). In later writings, Eccles, Wigfield, and their colleagues included cost as one of the components of task value and did not describe it as a moderator (e.g., Eccles, 2005; Wigfield & Eccles, 1992; Wigfield & Eccles, 2000; Wigfield et al., 2016). Recently, researchers have revisited this topic and provided some evidence that cost might be an independent construct in EEVT, separate from competence-related beliefs and from the components of task value (Barron & Hulleman, 2015; Jiang et al., 2017). This is similar to how Eccles-Parsons et al. (1983) originally wrote about the construct.

Supporting the distinction between cost and the other components of task value, Conley (2012), Perez et al. (2014), and Perez, Wormington, Barger, Schwartz-Bloom, and Linnenbrink-Garcia (2013) found that many students in STEM classes who had high utility, intrinsic, or attainment value also perceived high levels of cost. These researchers used person-centered analyses to create profiles of students based on their competence-related beliefs, utility, intrinsic, and attainment value, and perceived cost. Conley (2012) conducted these analyses with middle school math students, and Perez et al. (2013, 2014) did so with college students majoring in STEM fields. Both researchers found a group of students who had high competence-related beliefs and affirming value as well as high perceived cost. Previous research has suggested that students' competence-related beliefs and utility, attainment, and intrinsic value relate strongly and positively to one another within academic domains (Eccles & Wigfield, 1995; Jacobs et al., 2002; Meece et al., 1990; Wigfield et al., 1997). Cost seems to break this pattern; many students who have otherwise positive motivation still can perceive high cost to their STEM coursework.

Because of the evidence suggesting that cost impacts students in a different way than competence-related beliefs and the other components of task value, I conceptualized cost as an independent construct from task value in the present study. However, I note that independent does not mean uncorrelated. There are interrelationships between cost, value, and competence-related beliefs, similar to how there are interrelationships between competence-related beliefs and the components of task value. These interrelationships typically are negative. For example, if a student perceives that a task is too effortful he may determine that the effort is not worth it for him and lower the extent to which he values the task, or he may feel less competent to complete the task (Barron & Hulleman, 2015; Eccles, 2005). Many researchers have shown negative correlations between overall cost or specific dimensions of cost and one or more of the components of task value and/or competence-related beliefs (e.g., Flake et al., 2015; Gaspard et al., 2017; Perez et al., 2014; Safavian & Conley, 2016).

There are also some situations in which the relationship between cost and task value might be positive, however. Johnson & Safavian (2016) reported that in focus groups, many college students reported that having high stress or frustration helped them to work harder in the course, and putting forth effort helped teach them about time management. Rosenzweig, Jiang, and Wigfield (2017) also found that college and high school students' perceptions of emotional cost predicted positively their perceptions of attainment value in math and science courses (also see Safavian & Conley, 2016). They concluded that when tasks were more valuable to students, perceptions of emotional cost might increase because the stakes for failure at the task were higher. This work is recent and clear conclusions cannot be drawn about potential positive interrelationships between cost and value at this point. However, these findings demonstrate that cost should be considered independent from, yet related in complex ways to, task value and competence-related beliefs.

How cost impacts students' experiences and outcomes in STEM courses. Many students experience cost when they are taking STEM courses. In pilot work for this project (see Chapter 3 for more information), I surveyed college students in biology and physics and found that 89.8% of them reported experiencing at least one dimension of cost (task effort, outside effort, emotional, or loss of valued alternatives). Furthermore, among STEM subjects, students' perceptions of cost in physics tend to be particularly high: Gaspard et al. (2017) found that students' perceptions of cost during grades 5-12 were highest in physics compared to biology and math. In that study, perceptions of cost also increased over the course of students' educational trajectories, with stronger increases for female versus male students.

Experiencing cost in STEM courses has been shown to influence negatively students' achievement. Trautwein et al. (2012) found that German middle school students' perceptions of cost predicted negatively their math achievement on an international standardized test. They measured cost using a composite score of items assessing the dimensions of task effort and loss of valued alternatives. When they added competence-related beliefs to their model, cost no longer predicted achievement. However, it interacted with competence-related beliefs; competence-related beliefs predicted achievement more strongly as students' perceptions of cost decreased. Furthermore, Safavian, Conley, and Karabenick (2013) found that middle school students' perceptions of cost predicted their achievement on standardized math exams, even after controlling for utility, intrinsic, and attainment value and for competence-related beliefs.

Battle and Wigfield (2003) and Perez et al. (2014) found that perceived cost predicted negatively students' course-taking intentions. Battle and Wigfield (2003) administered a composite measure of cost, with items assessing effort, emotional, and loss of valued alternatives cost, to college women in STEM and non-STEM fields. Higher cost related to lower intentions to attend graduate school, even after controlling for attainment, intrinsic, and utility value. Perez et al. (2014) explored how each of three dimensions of cost uniquely predicted students' course-taking intentions, also controlling for competence-related beliefs and the components of task value. They adapted Battle and Wigfield's (2003) measure for use with male and female STEM majors. They found that perceptions of effort cost most strongly predicted intentions to leave a STEM major, and loss of valued alternatives cost predicted these intentions to a lesser degree.

Other work suggests that cost may be a *particularly* influential predictor of STEM outcomes. Conley (2012) found that students who showed patterns of motivational constructs characterized by high levels of perceived cost had worse math achievement than students with patterns that were characterized by low cost. Similarly, Perez et al. (2013) found that college students with high levels of competence-related beliefs and attainment, utility, and intrinsic value, but low cost, had greater intentions to pursue a science-related career than did students with high levels of the other constructs and high cost. Both researchers argued that cost might have been a key construct differentiating the groups of students that earned better versus worse STEM outcomes. More recently, Jiang et al. (2017) demonstrated that, compared to competence-related beliefs and a composite score of the three components of task value, cost was the strongest predictor of outcomes related to maladaptive academic functioning among middle and high school math students. These included adoption of avoidance goals, procrastination, disorganization, avoidance intentions, and negative classroom affect.

Finally, there is some recent evidence that student-level characteristics may differentially affect the relations between cost and course outcomes. Only one study has addressed this topic: Perez et al. (2016) reported that students with low self-efficacy showed a negative relationship between opportunity cost and course achievement, whereas students with high self-efficacy showed higher course achievement as their perceived opportunity cost increased. This finding raises the possibility that cost may impact course outcomes differently for different types of students. Such a possibility is supported by research from other branches of psychology. For example, Freitas, Liberman, and Higgens (2002) found that individuals who were predominantly concerned about potential losses in their environment and pursued tasks with vigilant cognitive strategies (i.e., they were prevention focused) performed better on a math task when they needed to resist distractions during the task compared to students who were predominantly concerned about potential gains in their environment (i.e., they were promotion focused). This was found to be as a result of prevention-focused individuals enjoying the task more when they resisted distractions during it than when they did not. Resisting distractions is related to loss of valued alternatives cost, so results suggest that individual differences may determine how strongly cost relates to course outcomes for some students.

Summary of cost research and possibility for intervention. Cost research has progressed to the point where researchers understand and can measure well four dimensions: task effort, outside effort, emotional, and loss of valued alternatives. Furthermore, researchers have demonstrated that many students report experiencing one or more dimensions of cost in their STEM classes, including those who are otherwise adaptively motivated to learn. Finally, researchers have shown that perceptions of cost tend to predict negatively students' STEM achievement and course-taking intentions, and cost sometimes differentiates between students who show higher or lower levels of these outcomes. Although findings regarding cost are complex, in general research suggests that to improve STEM outcomes it would be useful to try and reduce students' perceptions of cost. However, as I discuss in the next section, almost no researchers have tested this possibility to date using educational interventions.

EEVT-Based Interventions as Tools for Improving STEM Outcomes

Recently, researchers have shown that educational interventions focused on increasing motivational constructs from EEVT can improve students' performance and participation in STEM courses (Rosenzweig & Wigfield, 2016). I review work on this topic next, beginning with interventions that have targeted more than one motivational construct from EEVT simultaneously. Next I review interventions that only targeted utility value. These constitute the majority of intervention studies within EEVT to date. I then review interventions that have targeted constructs related to students' perceived cost. Finally, I discuss the limitations of the extant motivation intervention work within EEVT.

To find relevant motivation intervention literature in STEM, I conducted a systematic review using the APA PsycINFO and GoogleScholar platforms. First, I used PsycINFO to find articles that met three criteria: (a) at least one keyword was about motivation, either general ('motivation') or specific to constructs from a particular theory of motivation ('self-efficacy' and 'self efficacy', 'self-concept' and 'self concept', 'intrinsic motivation', 'interest', 'value', 'avoidance', 'mastery', 'goal'), (b) at least one keyword was about education ('education', 'school', 'academic', 'achievement'), and (c) at least one word anywhere in the text was about interventions or change ('intervention', 'experiment', 'quasi-experiment' and 'quasi experiment', 'enhancing', 'improving', 'increasing'). Second, I searched for articles that included any of the motivation keywords listed above, at least one STEM-subject-specific keyword ('STEM', 'science', 'math', 'technology', 'engineering'), and the term 'intervention' anywhere in the text.

Third, I hand-searched a number of review articles that have been written about motivation in the classroom, as well as every empirical study found using the searches outlined above that met criteria for inclusion in the review. Fourth, I conducted supplementary searches for work by specific researchers who had been frequently cited in motivation intervention literature. These steps returned over 3,000 results, each of which I reviewed for relevance to the topics discussed here. Here I discuss only the work related to EEVT, but see Rosenzweig and Wigfield (2016) for complete results of this literature review.

Interventions targeting more than one construct from EEVT. Acee and Weinstein (2010) and Weisgram and Bigler (2006a, 2006b, 2007) targeted more than one motivational construct from EEVT simultaneously in educational interventions. In four studies, Weisgram and Bigler targeted adolescent girls' competence-related beliefs and utility value for science and engineering. In one study (2006a) girls attended workshops in which they completed hands-on science activities to improve their competence-related beliefs, and they received information about science careers to improve their perceptions that science course material was useful for careers. Attendees showed higher competence-related beliefs and utility value for science compared to a comparison group of girls and boys who did not attend the workshop. However, the authors did not find pre-post workshop increases in these constructs after another, similar workshop, and in another study (2007) they found that girls only showed higher levels of these constructs if they received information about gender discrimination in science careers *and* a workshop. In a fourth study (2006b), girls who attended a one-week computer science camp that included hands-on activities and mentoring showed higher utility value for computer science, but not higher competence-related beliefs, than did girls and boys who attended a different camp. Overall results are mixed, but they suggest that in some circumstances a researcher can improve two or more motivational constructs from EEVT using an educational intervention.

Further supporting this point, Acee and Weinstein (2010) administered an intervention targeting utility, intrinsic, and attainment value to two sections of college statistics students. Students completed a 100-minute session in which they did activities designed to increase these components of task value. For example, to target attainment value students read a passage about why it was important to think about how the material they were learning was personally meaningful and then brainstormed skills that they could develop as a result of learning statistics. Students who received the intervention earned higher scores on a composite measure of the three components of value, and they were more likely than control condition students to access a supplemental website about statistics. One of the two course sections receiving the intervention also earned higher statistics test scores than students who did not receive it.

Interventions targeting utility value. The majority of EEVT-based STEM interventions have solely targeted students' perceptions of utility value. One might wonder why so many researchers have specifically targeted utility value instead of other EEVT constructs. Researchers have argued that the somewhat extrinsic nature of utility value makes it the most amenable target for motivation interventions (Hulleman, 2007; Hulleman et al., 2010). No researchers have designed interventions that solely target attainment value or intrinsic value, although some have targeted similar constructs within other theoretical frameworks. For example, Renninger et al. (2014) conducted interventions grounded in interest theory to target middle school students' interest in science, and interest is conceptually related to intrinsic value. I did not review that type of work here because it is not directly pertinent to the goals of the current study.

Hulleman and Harackiewicz (2009) published the first intervention that directly targeted students' utility value in a STEM course. The authors asked high school students to write one to eight brief essays about how what they were learning in science class related to their lives, or to summarize what they were learning in science. The goal of the treatment was to help students find relevance in what they were learning, which would make their course material seem more connected to them and thus more useful. Perceiving more utility value could make students more interested and engaged in what they were learning and ultimately help them achieve better. The authors found that treatment group students' interest and achievement in science did increase compared to control group students. However, these effects were limited to students who began the intervention with low competence-related beliefs in science. In other studies this research group has found positive effects of utility value interventions on utility value and performance in college psychology courses (Hulleman et al., 2010; Hulleman et al., 2016).

Four other research groups also have tested utility value interventions in STEM courses. Gaspard, Dicke, Flunger, Brisson, et al. (2015) gave German high school students an essay-based utility value relevance intervention similar to that used by Hulleman and Harackiewicz (2009). The intervention improved students' perceptions of utility value for their math classes compared to a waiting control condition, as well as their achievement in math (Brisson et al., 2017). However, the researchers found stronger positive effects after administering an intervention that asked students to evaluate quotations from other students about how math was related to their lives. The quotation-based intervention (but not the essay-based one) also improved students' perceptions of intrinsic and attainment value. Neither intervention changed students' perceptions of cost. The authors argued that the quotation-based intervention may be a more effective means of improving utility value than an essay-based intervention, possibly because the task is more pleasant for students to complete and provides more scaffolding.

Rosenzweig et al. (2017a) also compared quotation-based to essay-based utility value relevance interventions, with online high school math students. They compared these conditions to a condition in which students completed surveys, one in which students read quotations from other students and then wrote an essay about how math connected to their lives, and one in which students summarized what they were learning. They found that students in the quotations and evaluation condition reported higher utility value than did students in the do-nothing and summary conditions. Students in the other conditions reported utility value scores that were in between the other conditions' scores. Similar to Gaspard, Dicke, Flunger, Brisson, et al. (2015), the authors concluded that quotation evaluation is a particularly effective method to improve students' utility value. One additional finding from this study was that the summary condition improved some students' course grades more than did the utility value intervention condition, despite not impacting those students' utility value (Rosenzweig et al., 2017b). I return to these topics when discussing the design of the present study.

Harackiewicz et al. (2016) compared in college biology a utility value relevance intervention to another type of motivation intervention, called "values affirmation."

Values affirmation interventions are intended to buffer against students' experiences of identity threat (see Cohen & Sherman, 2014, for review). Values in these interventions are not task-based value from EEVT. Rather, they refer to broader personal values that represent meaningful topics for a particular individual (e.g., family, friends; see Rokeach, 1973). In values affirmation, students write brief essays about a topic that they value, such as family or friends; students in a control condition write about a topic that they believe someone else values (see Cohen & Sherman, 2014, for a review; see below for further discussion). Harackiewicz et al. (2016) did not find any effects of the values affirmation intervention on its own, or combined with the utility value intervention. However, students who received the utility value intervention earned higher course grades than students who did not receive it. In the utility value intervention, students wrote three essays about how biology course material was related to their lives. Effects were stronger for students who started with lower GPAs, and for first-generation college students who were members of underrepresented minority ethnic groups.

Finally, Harackiewicz, Rozek, Hulleman, and Hyde (2012) conducted a different type of utility value intervention, targeting *parents'* perceptions of utility value for their children. The authors mailed parents resources over two years about why it was important for their children to take math and science courses. The authors posited that the information would give parents more utility value for math and science that they could transmit to their children. This was shown to be true: Adolescents whose parents were in the treatment group reported higher utility value for math and science at the end of high school compared to a control group. This effect was mediated by parents' increased utility value for their children's STEM course-taking and by increased conversations between students and parents about STEM course-taking. Low-achieving female and high-achieving male students in the treatment group also took more advanced science and math courses than similar control group students (see Rozek, Hyde, Svoboda, Hulleman, & Harackiewicz, 2015).

Most of the studies just described impacted students' self-reported utility value and also sometimes impacted other motivational constructs and/or course outcomes. However, in one study (Hulleman et al., 2016, Study 2), a utility value intervention impacted competence-related beliefs and performance, but not utility value. In this study, students showed higher performance after receiving a utility value intervention, but these effects were explained by increases to students' perceptions of confidence in the course, not utility value. Results suggest that some interventions designed to target utility value may impact competence-related beliefs instead of, or in addition to, utility value. The authors noted that this intervention likely did not impact utility value because the instructor in the course increased all students' perceptions of value. Thus there may have been a ceiling effect on the intervention's ability to produce additional benefits to value.

Much laboratory research also has evaluated utility value interventions (Brown, Smith, Thoman, Allen, & Muragishi, 2015; Canning & Harackiewicz, 2015; Durik & Harackiewicz, 2007; Durik et al., 2014; Hulleman et al., 2010; Shechter, Durik, Miyamoto, & Harackiewicz, 2011). Rather than systematically describing each of these studies, I review relevant aspects of this body of work in the next section and in the Intervention Design and Measurement section.

Moderating variables in utility value interventions. In many utility value intervention studies, all students did not benefit equally. Researchers doing utility value intervention research have found moderating variables that in some circumstances affected their results. The four moderating variables that researchers have assessed most often are students' gender, ethnicity, baseline competence-related beliefs, and prior achievement. I discuss each in turn.

Gender is the moderator that researchers have assessed most often in STEM motivation interventions. Researchers often assess gender as a moderator because there are gender gaps in students' pursuit of certain STEM fields that have been attributed to differences in task value or competence-related beliefs (Eccles, 2007; Eccles, 2009). The results of studies testing gender as a moderator of motivation interventions vary widely. Gaspard, Dicke, Flunger, Brisson, et al. (2015) found that female students perceived higher intrinsic value and daily life utility value after a utility value intervention than male students did. However, in Harackiewicz et al.'s (2012) parental utility value intervention, only low-achieving boys and high-achieving girls took more advanced science and math courses compared to control (Rozek et al., 2015). Hulleman et al. (2016) also found that only low-achieving male students showed higher achievement after receiving a utility value intervention; high-achieving male students and all female students did not show achievement differences by condition. Furthermore, many utility value intervention researchers reported that gender did not moderate the results of their interventions (STEM studies: Harackiewicz et al., 2016; Hulleman & Harackiewicz, 2009; Laboratory studies: Brown et al., 2015; Durik & Harackiewicz, 2007; Durik et al., 2014; Hulleman et al., 2010; Shechter et al., 2011).

One should interpret with caution the large number of laboratory studies that failed to find gender effects. Rosenzweig and Wigfield (2016) posited that gender might only moderate interventions' results in STEM domains that are associated with beliefs that males typically will do better than females (e.g., engineering, advanced mathematics). Female students receive strong societal messages from caregivers and teachers that they are not well-suited for these types of STEM fields from a young age. Thus females often disidentify with and devalue these fields, choosing instead to pursue careers in other STEM or non-STEM domains (Eccles, 2007; Eccles, 2009). Laboratory utility value interventions have utilized mostly mental math paradigms, which are not associated with strong gender beliefs. Thus these interventions are not likely to be moderated by gender, but interventions in other STEM domains, such as physics, might be.

It is not clear whether males or females typically benefit more when gender moderates results of interventions. Utility value interventions might benefit females more than males because female students are in need of more motivation support for their task value in certain STEM domains. However, Rozek et al. (2015) argued that females might not benefit from interventions *as much as male students*, because a brief intervention could not change peoples' attitudes in the face of strong societal beliefs about gender and STEM. Researchers have not devoted sufficient attention to interpreting these inconsistent conclusions to date.

Ethnicity has been assessed as a moderator in two of the interventions discussed above. Similar to gender, results to date are not conclusive. Hulleman and Harackiewicz (2009) found no moderating effects of ethnicity in a utility value intervention with high school science students. However, Harackiewicz et al. (2016) found a three-way interaction with ethnicity and socioeconomic status in an intervention with college biology students. Students who were members of underrepresented minority groups (i.e., African American and Hispanic) benefitted more from a utility value intervention than did students who were not. Harackiewicz et al. (2016) argued that African American and Hispanic students often feel strong communal goals that are not perceived as consistent with learning advanced science. Thus they benefitted strongly from an opportunity to make course content congruent with their own personal goals by writing about how course material related to their lives.

Students' competence-related beliefs have moderated the effects of many utility value interventions, but again results to date are complex. Hulleman et al., (2010, Study 1) and Hulleman and Harackiewicz (2009) found that only high school students with *low* competence-related beliefs benefitted from a utility value intervention asking them to write about how what they were learning related to their lives. These authors argued that students with high baseline competence-related beliefs might have already been interested in and engaged with their courses, so they did not benefit from receiving additional information about the courses' utility. In the intervention, students with low competence-related beliefs were able to think and write about a connection that they might not have otherwise made to math, which benefitted them.

In contrast, Durik et al. (2014) found in laboratory work that only college students with *high* perceived competence in math showed higher interest and performance after a utility value intervention that showed students examples of how what they were learning related to their lives. The authors argued that students with high baseline competence-related beliefs benefitted from reading these examples because they saw that others also found math to be useful. This likely reinforced their belief that competence in math was important. In contrast, students with low competence-related beliefs who were told why math was useful may have reacted aversely to the examples because they were reminded of the importance of a field in which they did not feel competent. Without being asked to generate their own examples, they could not as easily connect themselves to the math they were learning.

Canning and Harackiewicz (2015) and Rosenzweig et al. (2017a) found that students with high and low perceived competence in math benefited equally well when they were asked to generate *and* read examples of utility value. According to Canning and Harackiewicz (2015), this approach reinforced the utility of math for students with high competence-related beliefs, and it gave students with lower competence-related beliefs a chance to generate their own ideas about how to connect to math. It therefore seems that competence-related beliefs moderate the effects of utility value interventions differently depending on the interventions' designs. I return to this topic in the Intervention Design and Measurement section. Furthermore, Harackiewicz et al. (2016) did not find moderation effects of competence-related beliefs in a study asking college students to generate connections between biology and their lives. Thus competence-related beliefs might not moderate the effects of all utility value interventions.

Finally, researchers have explored whether students' *actual* competence, in terms of prior achievement, moderated the effects of utility value interventions. Results are again not entirely consistent across studies. Hulleman et al. (2010, Study 2) and Harackiewicz et al. (2016) both found that college students who started an intervention with lower prior achievement benefitted more after receiving utility value interventions asking them to write about how what they were learning related to their lives.

Harackiewicz et al. (2016) also found that prior performance was a stronger moderator of interventions' results than were students' competence-related beliefs. However, Shechter et al. (Study 1) found no moderation effects of prior achievement after a laboratory intervention with college students.

Students' competence-related beliefs and prior achievement are the two variables that have most consistently moderated the results of utility value interventions. However. each of these moderating variables has affected the results of some interventions and not others, and moderating variables affected students in different directions across different studies. Additionally, other moderators sometimes have impacted the results of interventions, such as students' gender. Differences in methodologies and in populations tested make it difficult to draw overall conclusions about any particular moderating variable at this point. As I discuss below, this is a limitation of extant work.

Interventions targeting students' perceptions of cost. Most EEVT-based intervention work to date has targeted students' perceptions of utility value in STEM fields. Only Cromley, Perez, and colleagues (T. Perez, personal communication, January 7, 2016) have explicitly targeted students' perceptions of cost in an educational intervention. These researchers conducted a study in which college biology students viewed videos of other students discussing their experiences of cost in college science classes and why the effort they put into those classes was "worth it." The videos were intended to reduce students' perceptions of the effort cost associated with their biology classes. This intervention also incorporated some practices that might raise utility value by encouraging students to perceive their cost experiences as worthwhile. Results from the cost reduction component of this study have not been published to date. However, because the intervention targeted both cost and utility value, it will be difficult to determine whether changes to cost or to utility value are responsible for improving any outcomes. Additionally, the present study addressed utility value and cost in separate intervention conditions, so my results are distinct from the findings of this work.

No other studies within EEVT have targeted students' perceptions of cost. However, there are interventions in which researchers have targeted constructs similar to perceived cost. Specifically, social psychological intervention researchers have targeted constructs similar to emotional cost by attempting to reduce students' feelings of identity threat in their STEM courses. Identity threat refers to an individual's self-view being questioned or threatened (Sherman et al., 2013). This threat is a particular concern for students who belong to groups that are typically negatively stereotyped with respect to STEM achievement (e.g., African American students, women; Shnabel, Purdie-Vaughns, Cook, Garcia, & Cohen, 2013; Spencer, Steele, & Quinn, 1999). Identity threat experiences can cause students to feel uncertain about whether they "belong" in a particular course (Cohen & Sherman, 2014; Shnabel et al., 2013). Identity threat and belonging uncertainty relate to emotional cost because they address students' negative beliefs and emotions regarding their psychological experiences in a class. Thus interventions that reduce students' perceived identity threat potentially could be adapted to reduce students' perceptions of cost.

Some researchers have attempted to remedy experiences of identity threat by emphasizing students' positive characteristics. For example, the values affirmation interventions discussed in the previous section (e.g., Brady et al., 2016; Cohen et al., 2006, 2009; Harackiewicz et al., 2012, 2016; Miyake et al., 2010) aimed to reduce students' experiences of identity threat by affirming a positive value that students held. Researchers conducting these studies have shown positive effects, but these studies specifically targeted the emotional cost experienced by students in stigmatized groups. It is unclear how this type of intervention easily could be altered to reduce students' perceptions of other types of cost (e.g., task effort cost), or whether it would reduce all students' perceptions of cost rather than only those students who are members of stigmatized groups. Additionally, some of these studies found that students in non-stigmatized groups actually showed lower achievement if they received values affirmation versus a control condition (Brady et al., 2016; Miyake et al., 2010).

In other studies, Walton and Cohen (2007, 2011) targeted identity threat by encouraging students to *re-attribute* their feelings of belonging uncertainty. In a laboratory session, they asked African American and European American college students to read statistics and quotations from previous college students about fitting in in college; the information emphasized that (a) all students worry about social belonging at the beginning of college, and (b) these doubts lessen over time. Participants were asked to write their own quotation to this effect and were videotaped reading it. In the 2007 study, African American treatment group students rated their sense of academic and social fit more strongly, believed they had a higher potential to succeed in college, and intended to take a larger proportion of advanced college courses in the treatment condition versus a control group. They also were buffered from allowing daily adversity to lower their self-reported sense of "fit" in college for one week following the intervention, and they showed a larger change in GPA over the next year. In the 2011 study, African American treatment group students showed higher GPAs, better self-reported health, and reported that they visited the doctor fewer times during the three-year period post-intervention, compared to a control group.

The authors (2007) posited that their intervention was effective because it encouraged African American students to view belonging uncertainty not as a fixed characteristic associated with their identity (e.g., people like me do not belong in this class), but as a temporary and universal experience. This explanation is derived from attribution theory (Weiner, 1985; see Graham & Williams, 2009, for review), which posits that students' explanations for their successes and failures on tasks (i.e., their attributions) influence their later motivation, emotional responses, and behavioral choices. There are several dimensions of attributions that students make about the causes of their academic successes or failures. Two of these dimensions are (a) whether these causes were stable or temporary, and (b) whether these causes were internal or external. If students believe failure is due to internal, stable causes, this can be debilitating for later effort and performance because students believe they caused the failure and that this will continue to occur. Students who make this attribution are less likely to persist on challenging tasks or achieve well on them in the future (Graham & Williams, 2009).

Walton and Cohen's (2007, 2011) interventions aimed to prevent students from attributing their belonging uncertainty to stable, internal causes. They wanted students to re-interpret doubts about belonging in college as a challenge that got better over time (i.e., the doubts were temporary). They also wanted students to believe that having doubts about belonging was a normal part of college that many students experienced (i.e., the doubts were caused externally by college, not internally by students' own traits). The authors argued that by reframing their beliefs about belonging, students were able to cope better with challenging academic tasks moving forward and show higher persistence and achievement. In 2011, the authors empirically demonstrated that the intervention's effect on achievement was mediated by students' reduced perceptions that daily adversity was reflective of their own internal faults.

In two studies, interventions focused on belonging and attributions differentially improved outcomes for female students. Good, Aronson, and Inzlicht (2003) demonstrated that female students in high school math courses earned higher math test scores if they received messages from mentors regarding how their academic difficulties in seventh grade were due to the new environment instead of low ability. Additionally, Walton, Logel, Peach, Spencer, and Zanna (2015) did a similar social-belonging intervention to Walton and Cohen's (2007; 2011) studies and found that female students in demanding engineering courses showed higher grades during the year following the intervention if they received the intervention compared to a control condition.

Walton and Cohen's (2007, 2011) interventions provide a useful guide for understanding how to reduce students' perceptions of cost for two reasons. First, attribution-focused interventions fit well within the EEVT model. EEVT includes interpretations of experience as one factor that influences students' competence-related beliefs and task value (see Figure 1). The original Eccles-Parsons et al. (1983) figure showed interpretations affecting cost as part of effects on task value. In the present study, I considered cost to be a separate construct from task value, so I posited a direct link between attributions and students' perceived cost. Therefore, if students believed that any cost experiences they have in a course were temporary and not caused by their own low ability, this could lower their perceptions of these costs. Students might then show higher persistence and achievement in the course.

Second, unlike the values affirmation studies Walton and Cohen's (2007, 2011) work is adaptable to all dimensions of cost and to many groups of students. Students might be more negatively affected by any of the dimensions of cost if they believe those cost perceptions are stable and induced by their own shortcomings. Thus encouraging them to attribute any dimensions of cost as temporary and external could be beneficial. Furthermore, as noted earlier many college students experience cost in STEM courses (Perez et al., 2014). Targeting attributions about cost thus might impact the motivation of a wide variety of students, not just students who are members of stigmatized groups.

Moderating variables in cost interventions. Little intervention work has targeted cost directly, so it is unclear which variables are likely to moderate these interventions' effects. Walton and Cohen (2007, 2011) found that only African American but not European American college students showed higher achievement, motivation, and/or course-taking intentions after their interventions. Walton and Cohen (2007) argued that European American students did not benefit from this intervention because they already felt like they belonged in a course, whereas African-American students began the intervention with a low level of certainty regarding whether they belonged as a function of negative stereotypes regarding the typical academic performance of the group to which they belonged. African American students thus have a higher likelihood to make maladaptive attributions regarding their course challenges being reflective of low ability. For similar reasons, Good et al (2003) and Walton et al. (2015) both found that their interventions benefitted female students more than male students. Thus it is possible that

African American students, female students, or other students who have high initial belonging uncertainty regarding physics might benefit more than European American students, male students, or those with lower belonging uncertainty from interventions that target cost.

Another moderator that might impact cost interventions is students' beliefs about the malleability of intelligence (Dweck, 2000; Dweck & Master, 2009). Researchers have posited that students can believe that intelligence as a fixed, unchangeable construct, or instead they may believe that intelligence is more malleable and can change over time (see Dweck and Master, 2009, for review). If students have fixed beliefs about intelligence, meaning that they believe intelligence cannot change, they perceive course challenges to be indications of their own low ability (see Dweck & Master, 2009, for review). Course challenges are related to perceptions of cost, so students with fixed beliefs about intelligence may be more likely to perceive course challenges as stable factors that will not change, compared to students who have a strong belief that intelligence is malleable. Research shows that students pay more attention to information that aligns with their predominant theory of intelligence, and they show biased cognitive processing in order to discount information that counteracts their beliefs about intelligence (Plaks, Dweck, & Grant, 2005; Plaks, Stroessner, Dweck, & Sherman, 2001). Because of these factors, students with a stronger fixed belief about intelligence might not internalize readily an intervention message that they can re-attribute their beliefs about their course challenges. They might then benefit less from a cost reduction intervention. Conversely, students who believe intelligence is malleable may be particularly likely to internalize a message that their course challenges can change over time, because that message is consistent with their beliefs that their abilities can change.

Limitations of extant EEVT interventions and rationale for the present study. Interventions focused on enhancing students' utility value have been effective in improving STEM interest, course-taking, and achievement. However, the fact that most motivation interventions using an EEVT framework have only focused on utility value is a limitation of this work. Utility value interventions might be very effective ways to target students' STEM motivation and outcomes, but it is likely that this type of intervention is only one of many possible ways to do so. It would be useful to administer both a utility value intervention and an intervention targeting another construct from EEVT in the same context. In particular, the research outlined above suggests that an intervention targeting perceived cost could be developed and would be likely to influence students' STEM outcomes. In this dissertation I developed and tested a utility value intervention and a cost intervention and compared them to two different control conditions. There are several reasons why such a study was important to conduct, given the current state of knowledge in the field that I just outlined.

First, this study was one of the first to evaluate whether it was possible to improve students' STEM outcomes by targeting their perceptions of cost in a physics course. Cost relates strongly to students' outcomes in STEM courses (e.g., Conley, 2012; Perez et al., 2014). Thus it was worth exploring whether it was possible to target this motivational construct using an intervention and cause changes to students' STEM achievement, course participation, and later course-taking patterns. I also planned to collect information about whether students' responses to the intervention or their perceptions of cost changed over time. This could provide even more information to the field regarding when and how might be the best times to intervene to reduce students' perceptions of cost in introductory college physics courses.

Second, this study provided the first test of whether a utility value intervention would impact students' motivation and course outcomes in introductory college physics, a critical attrition point with respect to students pursuing STEM majors and careers. No prior studies have tested utility value interventions in this context. Many researchers have argued that the effects of motivation interventions might depend on elements of the course context (Kaplan, Katz, & Flum, 2012; Rosenzweig & Wigfield, 2016). In this study, many students in college physics courses might already have chosen to pursue a math-intensive major or career, and thus they may already have been aware of how useful their physics course was. This could cause utility value interventions not to show as strong of effects as they had shown in prior studies.

Third, the study assessed in the same context a utility value intervention and an intervention targeting cost. Cost has been found to distinguish groups of students who have better or worse achievement in STEM fields, even when accounting for those students' utility value (e.g., Conley, 2012). This result implies that cost might be uniquely linked with students' STEM outcomes and that a cost intervention might impact students even in educational contexts where a utility value intervention does not.

Fourth, the study explored the impact of several moderating variables on cost and utility value interventions. It is important to evaluate moderating variables, because researchers want to use the results of motivation interventions to make educational policy and practice recommendations (e.g., Lazowski & Hulleman, 2015). This is difficult to do without an understanding of how different students are likely to respond to different types of motivation interventions. There are many different moderators that have been shown to influence the results of utility value interventions. However, researchers have not drawn systematic conclusions regarding any of these moderators, and work on moderating variables for motivation interventions in general is limited. These factors limit researchers' abilities to predict for whom and in what circumstances different interventions will work best (Rosenzweig & Wigfield, 2016). This analysis also was important because it could provide insight on whether the cost intervention was affected by the same moderating variables as was the utility value intervention.

Summary. Research has explored how constructs from EEVT can be targeted through educational interventions. The majority of these studies have targeted utility value. Results are promising, but it is not clear whether other interventions also might be effective ways to improve students' STEM achievement and course-taking patterns. Perceived cost has not been targeted in many previous interventions, but evidence suggests that it may be possible to reduce students' perceived cost by changing their attributions about cost to be more temporary and external. Thus a cost intervention may be equally or more effective than a utility value intervention. It also may impact certain students differently than a utility value intervention, as a function of moderating variables. These possibilities were worth exploring in order to understand the different ways that researchers can improve college students' outcomes in introductory college physics courses.

Recommendations for Intervention Design and Measurement

Interventions targeting cost and/or utility value will be ineffective unless researchers design them so that they are likely to change the motivational constructs of interest. Furthermore, promising interventions may appear to fail if researchers do not appropriately measure their effects. In this section I review recommendations from past research regarding intervention design and measurement, two critical elements of understanding and interpreting interventions' results (Rosenzweig & Wigfield, 2016). In the last section of the chapter, I address how the present study adhered to these recommendations.

Aligning intervention design with motivation theory. Rosenzweig and Wigfield (2016) stated that the lack of alignment between motivation interventions and motivation theory is one of the major limitations of extant research. They argued that without clear alignment with theory it is difficult to understand why and how particular interventions were effective or ineffective. If such an intervention was ineffective, perhaps a particular construct cannot easily be changed via intervention, but the intervention also might not have targeted the appropriate psychological process to change that construct. If interventions were effective, it would be difficult to determine what psychological processes were targeted in the intervention to cause its success. They recommended that researchers (a) clearly define and measure all motivational constructs of interest according to theory, (b) clearly explicate what parts of an intervention might target those constructs and how they might effect change in them, and (c) clearly explicate how an intervention changing motivation would be expected to change broader academic outcomes. The present dissertation and most of the studies reviewed in this chapter were grounded within EEVT, but it was still critical that I articulate a theory of change outlining how each intervention condition would be likely to target a given construct within EEVT.

Designing interventions to effect maximum change in motivation. Once researchers have articulated the theoretical constructs and processes to be targeted by an intervention, they should design intervention practices that are likely to change those constructs in lasting ways. Many educational researchers have made recommendations about what are some good practices to change students' attitudes, emotions, or beliefs in general, and a few have addressed good ways to target students' utility value specifically. None have addressed perceived cost.

Changing attitudes, beliefs, and emotions. Yeager and Walton (2011) conducted a systematic review of educational interventions that targeted students' beliefs or emotions including motivational constructs, which they called "social-psychological" interventions. I summarize their conclusions here because they are a good synthesis of current recommendations for targeting these types of constructs in intervention work. The authors argued that social-psychological interventions are not a panacea or quick fix for broader educational problems. Instead, these interventions can be very successful supplements to broader educational reform efforts *if they are designed carefully and correctly*. They recommended that researchers conducting these types of intervention studies meet three criteria.

First, researchers should target recursive psychological processes in interventions. Recursive processes are those that occur for students again and again while they are learning, such as frequently-occurring beliefs (e.g., When am I going to use this information?) or experiences (e.g., I get frustrated when I think about an upcoming exam). These processes often build on themselves, which can heavily influence achievement and other academic outcomes (e.g., When I am frustrated about my exam I do poorly, which makes me even more frustrated for the next one; Cohen et al., 2009). Yeager and Walton (2011) argued that interventions tapping into recursive processes can show effects long beyond what students experience during the intervention itself, so long as students bring the message of the intervention to mind each time they have a particular experience or belief.

Second, researchers need to target the psychological process of interest in a particular educational context with accuracy. Yeager and Walton (2011) argued that students' motivational experiences are subjective and differ from one context to the next. Even small changes in how an intervention is administered can change the meaning of a particular intervention practice for students in a particular setting (also see Kaplan et al., 2012). For example, if a teacher wants to target her students' utility value she might ask the students to write a brief essay about how course material is relevant to their lives. This might work well in a class where students feel competent to learn their course material. However, this essay prompt may remind students of their low competence in a class where they struggle to understand the course. This might lower students' competence-related beliefs rather than increase their utility value. Yeager and Walton (2011) recommended that researchers carefully develop intervention materials so that they are meaningful within a particular context. One way to do this is to design interventions in cycles based on feedback from people who are familiar with the intervention context.

Finally, Yeager and Walton (2011) recommended that interventions use "persuasive yet stealthy" methods to change students' beliefs (p. 268). Researchers cannot simply ask students to read a message about the importance of utility value and then expect those students to show higher value. Students need to process the information in an intervention *actively* and *deeply* in order for their attitudes to change (Aronson, 1999; Lewin, 1952; Schwartz & Martin, 2004). A particular method the authors recommend is "saying is believing", in which students are asked to generate a particular message and advocate it to others. This makes students much more likely to internalize and endorse the message (see Aronson, 1999). Researchers also should be stealthy. Adolescent students have a strong need for autonomy, and they are unlikely to internalize any attempt to change their beliefs if they perceive it as controlling. Students also may feel that they are being singled out for "help" in overt interventions, and they might react defensively or negatively. In order to avoid these reactions, researchers should not overtly advocate a particular motivational message to students or tell them the purpose of an intervention directly.

One important topic that Yeager and Walton (2011) do not discuss, but which is important for intervention work, is whether students are more likely to change their beliefs and values if they read messages presented by particular types of sources (e.g., experts, peers, people they trust). Students tend to rely more heavily on source information to judge the quality of a message when they do not process the message deeply (Petty & Cacioppo, 1986). Based on the advice given by Yeager and Walton (2011), I planned for students to process the examples and content from the present study deeply and thus I did not think it was likely that participating students would rely heavily on information about the source of the content presented. However, I acknowledged that it was possible not all students would process the information deeply in this study. Thus, I took several steps to increase the likelihood that students would respond positively to the source of content presented in the intervention. In Chapter 3, I discuss relevant aspects of the work regarding information source and how I used it to inform my intervention design.

Changing utility value. To date, researchers have addressed two topics regarding what are the best methods to target utility value. First, they have compared different types of utility value intervention tasks. As noted above, Gaspard, Dicke, Flunger, Brisson, et al. (2015) and Rosenzweig et al. (2017a) found that asking students to read and respond to quotations from other students improved utility value more than asking them to write essays about utility value. Essay-writing tasks benefit students in several ways. Students have a chance to generate their own connections to an intervention, which reduces potential pushback or beliefs that they cannot do something that is important to them. Essay-writing also allows students to process information deeply.

However, quotation evaluation tasks that include some writing afford these same opportunities as well as provide additional benefits to students. These have been outlined by Gaspard, Dicke, Flunger, Brisson, et al. (2015) and Rosenzweig et al. (2017a). Students receive increased scaffolding on a quotation evaluation task compared to a writing task, so it is easier to complete. Students are also provided with examples of a breadth of ways in which they can make their own connections to utility value when they read quotations, which might give them ideas about how to do so in their own lives. Furthermore, students tend to dislike essay writing, so an evaluation task might be more enjoyable for them. Finally, students with different baseline levels of competence-related beliefs sometimes respond differently to different types of utility value interventions. By providing examples of utility through quotations and asking students to generate their own thoughts, a researcher can maximize her chance that students with different levels of competence-related beliefs will respond positively to the intervention.

The second topic that has been studied with respect to utility value intervention design is what types of utility value examples are best for students to read if interventions include examples or quotations. In college laboratory studies, researchers have found that interventions should include examples of utility value related to students' everyday life and to communal goals. Canning and Harackiewicz (2015, Study 3) found that students with low competence-related beliefs showed more utility value and interest after seeing examples of why a new mental math technique was useful for everyday activities, compared to when they saw examples relating the technique to everyday activities and future careers. Students with high competence-related beliefs benefitted equally from seeing either type of example. Brown et al. (2015) compared utility value information about everyday life to information about how biomedical research could be useful to help others or work with others. They found that students who read the "communal" utility value information had more positive feelings about biomedical research and reported greater intentions to pursue a biomedical research career than students who read about more general utility or about personal utility. The authors argued that these examples benefitted students because they typically perceive STEM activities as being personally useful, but not necessarily as useful for meeting communal goals.

Changing perceived cost. Almost no researchers have targeted students' perceived cost in intervention work. However, as I discussed above Walton and Cohen (2007, 2011) seemed to target a similar construct to emotional cost, by asking students to read quotations about other students' belonging uncertainty. I consider their interventions to be the best available model for designing an intervention to reduce students' perceived cost. This intervention is theoretically sound and shows a clear theory of change that is relevant for reducing perceived cost. The methodology of the intervention also aligns with the recommendations made by Yeager and Walton for social-psychological intervention design (2011; i.e., it incorporates a saying-is-believing activity and includes stealthy delivery of materials). Finally, the structure of their intervention is similar to that of the utility value intervention, because students read examples from other students and then write about those examples.

Choosing intervention timing. It is important to consider how long an intervention should last, and how many sessions of an intervention should be administered over that time period. Unfortunately, few motivation researchers have written about or justified their choice of intervention timing (Rosenzweig & Wigfield, 2016). Thus there exist no clear recommendations on either point. What is clear is that interventions *do not* need to have a long duration to work effectively or to show lasting effects. Lazowski and Hulleman (2015), Rosenzweig and Wigfield (2016), and Yeager and Walton (2011) all concluded that many brief interventions showed strong and lasting effects on motivation and academic achievement. Yeager and Walton (2011) argued that interventions' effects were likely determined much more by whether they targeted meaningful psychological processes for students and whether they were carefully designed to fit a particular context than by their length.

Previous utility value and cost interventions have been effective after only brief implementation. Gaspard, Dicke, Flunger, Brisson, et al. (2015) found positive effects on students' perceived task value after one session lasting between one and two hours and two short refresher doses. Harackiewicz et al. (2016) and Hulleman and Harackiewicz (2009) found effects on motivation and achievement after administering one through eight 10 - 15 minute essay-writing sessions over several months. In terms of cost, Walton and Cohen's (2007, 2011) belonging-uncertainty-targeting interventions showed effects for one to three years, and each was implemented in a single laboratory session.

Appropriately measuring effects of an intervention. Many motivation interventions improved some motivational constructs or achievement outcomes but not others, or they only showed effects for a certain type of student (Rosenzweig & Wigfield, 2016). Researchers should anticipate these potential results and ensure that they measure the effects of their interventions appropriately. A first step in this process is articulating a clear theoretical framework and theory of change for an intervention. Doing so ensures that researchers measure the specific motivational constructs that their intervention is likely to improve. For example, an intervention targeting students' utility value will not necessarily improve students' intrinsic value. If researchers did not know which theoretical construct was targeted by their intervention, they might measure intrinsic value as an outcome instead of utility value and reach the false conclusion that their intervention was not effective. Clear theoretical foundations are also important so that researchers measure the correct academic outcomes that are improved by an intervention.

For example, a utility value intervention might improve students' course-taking intentions but not their achievement. Researchers only measuring achievement as an outcome would again reach the false conclusion that the intervention was ineffective.

Even when researchers measure the appropriate motivation and outcome variables in a STEM intervention, many student-level variables might moderate results (Rosenzweig & Wigfield, 2016). Researchers should measure potential moderating variables so that they can test for these types of limited benefits of an intervention. With respect to the present study, it was worthwhile to measure gender, ethnicity, baseline competence-related beliefs, and prior achievement as likely moderators of utility value interventions. It was worthwhile to measure ethnicity, belonging uncertainty, and beliefs about the malleability of intelligence as potential moderators of cost interventions.

Summary. Researchers should ensure that their motivation intervention studies have a clear theoretical framework and theory of change. They should implement activities that are likely to effect change in students' motivation, such as providing persuasive messages, targeting recursive psychological processes, and ensuring that interventions are meaningful to the particular contexts in which they are implemented. Researchers specifically targeting utility value or cost both are likely to benefit from using quotation-based interventions in which students have sufficient scaffolding to complete the tasks. Researchers do not need to implement long-lasting interventions to effect meaningful change, but little work has addressed intervention timing overall. Finally, researchers should measure motivation, outcome, and moderating variables carefully so they do not fail to observe all of the potential effects of their interventions.

The Present Study

In the present study, I compared four experimental conditions in a sample of college physics students: (a) a utility value intervention condition, (b) a cost intervention condition, (c) a condition that summarized what they were learning, and (d) a control condition that did not receive an intervention. I conclude this review by briefly describing how the two intervention conditions in the present study aligned with the recommendations just noted.

Utility value intervention condition. In the utility value intervention condition, students were asked to read four quotations from their peers about how what they were learning in physics class related to their lives. Students rated each quotation on two dimensions, then ranked the quotations according to which was the most relevant to them. Next, they provided a brief commentary about why they ranked their top-ranked quotation the way that they did. Finally, they wrote their own examples of how utility value related to their lives, ostensibly for a future student in physics to read. This condition was expected to target utility value by encouraging students to think about why what they are learning in physics was relevant to their lives. As Hulleman and Harackiewicz (2009) and others have noted, by believing that course material is relevant, students might feel more connected with material and perceive more utility value in it. Having more utility value would improve students' participation in the course. Ultimately, this could improve achievement in that course and likelihood of students taking other STEM courses in the future.

Cost intervention condition. The cost intervention condition also asked students to read quotations from fellow students about physics class. The quotations in the cost intervention emphasized how students experienced different types of cost in physics and then described how those costs affected students temporarily, or were something that was common to many students. Students completed the same activities to evaluate the quotations as did students in the utility value intervention condition, except the examples they read and wrote about were related to overcoming challenges in physics class, not relating course material to their lives. By reading these examples and writing about their meaning, participants were expected to think about how their cost experiences were temporary and externally caused, rather than reflective of their own low ability in the course or being something that would continue to affect them indefinitely. This was expected to lower students' cost. Lower perceived cost could in turn improve students' course-taking patterns and course achievement.

General intervention information. The two intervention conditions in this study adhered to the recommendations that I outlined in the previous section. They targeted recursive processes, specifically thoughts regarding how what students were learning was expected to be useful, and thoughts about how much effort or frustration a course would entail. By asking students to rank and evaluate quotations, the purpose of the interventions was not salient. Furthermore, students were encouraged to process deeply and actively each of the quotations they read by ranking and evaluating them, and by writing their own example. To ensure that the meaning of the intervention was accurate, I extensively pilot-tested intervention materials (see Chapter 3). Finally, I measured motivation and achievement at several time points using well-validated measures that aligned with theory, and I tested for potential moderators that have been suggested in previous research: baseline competence-related beliefs, prior achievement, belonging uncertainty, beliefs about the malleability of intelligence, gender, and ethnicity.

Chapter 3: Methods

Design

In this study I tested the effects of utility value and cost interventions with college physics students. I utilized a between-subjects experimental design with four conditions (cost intervention, utility value intervention, summary, survey control). Students were assigned to these conditions using a two-step process: They first were randomly assigned to the cost intervention condition, the utility value intervention condition, or a control condition. Next, students in the control condition were randomly assigned to either the survey control condition or the summary condition. The purpose of using two-step random assignment was to ensure that there was a sufficiently large sample in the cost and utility value intervention conditions to detect effects, while still being able to evaluate whether these interventions showed effects compared to two different types of control conditions.

Participants

As mentioned in Chapters 1 and 2, college students were the population of interest in this study because there are relations between these students' motivation in their college STEM courses and their achievement in those courses, as well as their later pursuit of STEM majors (e.g., Lent et al., 2003; Musu-Gilette et al., 2015; Perez et al., 2014; Seymour & Hewitt, 1997; Strenta et al., 1994). I therefore sampled college students who were enrolled an introductory physics course (Physics 161, Mechanics and Particle Dynamics) at the University of Maryland, College Park. I used this method of non-probability sampling to recruit participants because a particular professor expressed interest in administering the intervention. The professor received a $100 gift card to the University of Maryland bookstore in exchange for participating in the study.

Physics 161 is the first in a three-semester set of physics courses that students take who are pursuing engineering, natural science (excluding the life sciences), or computer science degrees at the University of Maryland. This course is not restricted to students from particular fields, but only certain students typically take it because it requires prerequisites of calculus and chemistry. There is also a separate physics course geared towards students who want to pursue medicine or biological sciences. The anticipated enrollment in Physics 161 for Fall, 2016, based on data from prior semesters, was 250 students. I anticipated that this sample would have the following demographic characteristics based on pilot data from Fall, 2015: 70% male; 85% freshmen or sophomores; 40% European American; 15% African American; 23% Asian American.

One hundred and seventy-nine students actually enrolled in Physics 161 during Fall, 2017. I collected data from 162 of these students and received permission to analyze the data from 148 of them (82.7% of the class; see Chapter 4 for more information). Consenting students were 72.1% male, 53.7% European American, 28.6% Asian or Asian American, 8.2% African American, 4.1% Hispanic or Latino, 4.1% Middle Eastern, and 1.4% other ethnicities. Students' average age was 19.94 (SD = 1.48); 49.0% of students were freshman, 39.5% were sophomores, 8.8% were juniors, and 1.4% were seniors. Most students (86.4%) were required to take Physics 161 for their majors or intended majors. Related to that point, almost all (94.6%) of the students were enrolled in or intended to pursue majors in the university's school of engineering (69.4%) or the school of computer, mathematical, and natural sciences (25.2%).

Participants were recruited to the study by the course professor, who agreed to embed the study activities into his course curriculum for the semester. Students completed the intervention or control activities on three occasions as part of their weekly online homework assignments for the course. Students could opt in or out of releasing their data from these activities (see Chapter 4 for the numbers of students who chose to do this). Students who completed the intervention or control activities received a small amount of homework credit in exchange for participating, but this credit was not contingent on whether or not they opted in or out of releasing their data.

I conducted power analyses to determine whether my sample size would be appropriate to answer the research questions of interest in this study, using G*Power 3.1 (Faul, Erdfelder, Lang, & Buchner, 2007). I based these analyses on Research Questions 1- 6, which focused on the effects of the interventions, and with potential moderators of those effects. I conducted a sensitivity analysis to explore power, because I knew my sample sizes and wanted to determine what detectable effect sizes would be. As I discuss later in this chapter, to answer Research Questions 1 – 5, I evaluated the main effects of the intervention conditions on various outcomes using multiple linear regression with three terms. For Research Question 6, I tested for moderation effects of six variables on this three-term model. This required me to add additional terms to the models used for Questions 1 – 5 representing the moderating variable(s) and the interaction of a given moderating variable with the three terms representing the experimental conditions. I originally planned to add all six potential moderating variables to each model as covariates (8 terms per model) and test the moderating variables' interactions with the conditions one at a time (3 terms per model, except in the case of models testing ethnicity as a moderator, which would include 9 terms). This would result in most models having 14 terms, and models testing ethnicity as a moderator having 20 terms.

Before collecting data, I conducted an a priori power analysis to determine the size of effects I could observe using this model and my projected sample size. Historically, approximately 95% of students typically completed each homework assignment for this course. Thus I estimated that I would obtain data from approximately 240 students at each time point of the intervention. I estimated that the total number of students who would release their data at all three time points was 200. I conducted my power analysis within the *t* test family, because I was interested in detecting the effects of single regression coefficient. I determined that with 3 regression terms, a two-tailed test, n = 200, power = 0.8, and $\alpha = 0.05$, I could detect effects at a Cohen's $f^2 = 0.0396325$. With 14 and 20 terms and the same parameters, I would detect effects at a Cohen's $f^2 = 0.0396558$ and $f^2 = 0.0396697$, respectively. These values were all slightly above the value typically interpreted as a small effect (0.02; Cohen, 1988) and well below the value that typically suggests a medium effect (0.15; Cohen, 1988). Thus I determined that I would be able to detect at least small to medium effects using a sample size of 200.

Fewer students actually enrolled in the course than I anticipated, so I re-ran this power analysis with my achieved sample size of students after collecting data. I used a sample size of 132, which represented the smallest number of students providing data at any point in the study (see Chapter 4 for details). Based on the same parameters noted above, I could detect effects at $f^2 = 0.0604525$ and $f^2 = 0.0605069$ for models with 14 and 20 regression coefficients, respectively. This was still within the range to detect small to medium effects, but the threshold for detecting effects was much higher than it was with a sample size of 200. In order to maximize my power with this smaller sample, I chose not to include all six moderating variables in all of my interaction models as covariates. Instead, I included each moderating variable only in the models for which it was the moderator of interest. This reduced the number of terms utilized in the models, so that I could detect effects at f^2 =.0603957 (in most models) and f^2 = .0604700 (in models testing ethnicity as a moderating variable).

Materials

Intervention conditions. I administered two intervention conditions (cost and utility value) and two control conditions (survey control and summary) in this study. Following the recommendations outlined in Chapter 2, I conducted much pilot work to ensure that the materials for each condition were theoretically meaningful and relevant to students in Physics 161. In this section, I briefly outline the process used to develop the materials for each condition. Then I describe the final versions of each condition.

Intervention development. To develop the intervention conditions for the present study, I adapted materials that have been used in prior research and collected feedback from students enrolled in Physics 161 during the 2015-2016 and 2016-2017 academic years.

I choose to administer a quotation evaluation task for the cost intervention as well as for the utility value intervention. As outlined in Chapter 2, previous research suggests that the best way to target utility value in intervention work is to have students read and evaluate quotations (Gaspard, Dicke, Flunger, Brisson, et al., 2015; Rosenzweig et al., 2017a). This task benefits students with both high and low competence-related beliefs, it encourages active processing of the intervention content, it provides scaffolding to help students generate examples of utility, and it is relatively enjoyable. Yeager and Walton (2011) and others have argued that students completing any type of intervention will benefit from generating ideas, completing a task with scaffolding, and processing information deeply and actively. Furthermore, Walton and Cohen’s (2007, 2011) interventions targeting a construct similar to cost also asked participating students to read quotations from fellow students.

I began developing materials by creating the quotations that students would read in the cost and utility value intervention conditions. I did not conduct extensive pilot work to develop the ranking and evaluation task for these quotations; instead, I adapted closely the structure of materials used in prior research (Gaspard, Dicke, Flunger, Brisson, et al., 2015). To create the quotations, I first wrote sample quotations that (a) described students’ experiences of cost in physics and how those experiences got better over time, for the cost intervention, or (b) described how students related their course material in physics to their lives, for the utility value intervention. Prior researchers have suggested that it is best to use examples of relevance for communal goals and for everyday life activities, not solely examples of relevance for careers, in utility value interventions (Brown et al., 2015; Canning & Harackiewicz, 2015). Thus I created pool of eight utility value quotations that addressed how physics can help one understand phenomena in the world around them, how physics can be useful to help others, and how physics could be useful for career paths that were frequently pursued among students in Physics 161.

Few researchers have assessed students’ experiences of cost in STEM courses. Therefore, I was unsure what experiences college physics students would perceive as costly, or which kinds of quotations students would find compelling. To study these topics, I conducted a survey-based pilot study with 187 students enrolled in Physics 161 during Fall, 2015. This included open-ended questions as well as quantitative and demographic questionnaires. As part of this study, I asked students to note whether they experienced each type of cost (task effort, outside effort, emotional, or loss of valued alternatives) and to describe those experiences. Students also evaluated some preliminary quotations that I had developed. Students rated the extent to which they had experienced something similar to the example depicted in each quotation, and they made suggestions to improve each quotation.

The results of the pilot work suggested that 89.8% of Physics 161 students reported experiencing at least one type of cost, and 48.6% reported experiencing two or more types of cost simultaneously. Further, students' open-ended responses suggested that many reported a set of common experiences that were associated with high cost. Students often noted the weekly homework for Physics class as a source of effort cost, because it was time consuming. They also noted that studying for exams was a source of effort cost, and some noted it as a source of emotional cost as well because they experienced anxiety. Students cited frustration over working hard for little payoff as another source of emotional cost. For outside effort cost, many students reported that physics itself was not always particularly challenging, but managing the workload for physics along with the workload in other courses was. Students did not report experiencing loss of valued alternatives cost as often as the other sources. One final piece of feedback was that students did not believe their cost experiences "got better" over time, as the preliminary quotations had suggested. Many students noted instead that they found ways to manage actively the costs that affected them. I utilized this feedback to revise my initial pool of cost quotations. The revised quotations focused on those topics that students emphasized consistently as being costly. Furthermore, instead of noting that cost "got better" I stated in the quotations that cost lessened over time because students better knew what to expect, because they realized this was a normal part of college, or because they reminded themselves that effort required to study or complete an activity was only temporary.

In Spring, 2016 I administered revised cost and utility value quotations to two focus groups of approximately five students each who were enrolled in Physics 161. Participants gave feedback on the content of quotations. I also received feedback on the quotations from two experts in motivation intervention research and theory. This made it more likely that the quotations would target cost and utility value rather than other psychological constructs. Based on this feedback, I revised the quotations. Then, I recruited two additional focus groups of approximately six students each to provide feedback with which I revised the quotations further.

Later in Spring, 2016 the faculty committee advising me on this dissertation study recommended that I revise the quotations again so that they would (a) emphasize the re-attribution of cost experiences more clearly, (b) emphasize controllable aspects of cost reduction more clearly, and (c) mitigate the likelihood that students would respond negatively to the content of any quotations. I addressed these concerns to create a final version of each quotation, which I revised in consultation with an expert in motivation theory and intervention research.

The committee also recommended changes to the framing of the intervention activities. In both conditions, and at both sessions, I revised materials so that instructions asked students for their feedback regarding their experiences of cost or utility value at different points in the semester, rather than telling students directly that they were participating in an intervention targeting their motivation. This choice was intended to improve students' perceptions of autonomy with respect to the intervention and to further reduce any anxiety they might experience if they did not agree with the students featured in the quotations.

The next step in developing intervention materials was to choose how many quotations to include in each condition. I chose to include four quotations in the first session of the cost and utility value intervention conditions for several reasons. First, this number is similar to previous quotation-based utility value interventions, which have utilized four to six quotations (Gaspard, Dicke, Flunger, Brisson, et al., 2015; Rosenzweig et al., 2017). Second, with four quotations there would not be too much text for students to read. However, students would still be exposed to multiple different examples from which they could glean ideas about how physics related to their lives or how to perceive less cost. Finally, students in the focus groups showed consistent positive responses to the four quotations I selected in both the cost and utility value intervention conditions. They had mixed reactions to some of the other quotations. In particular, students expressed mixed reactions to the quotations targeting loss of valued alternatives cost. Because students did not report experiencing this type of cost often in the pilot study, I omitted examples targeting this dimension of cost from the cost intervention.

I also determined what would be the "source" of the quotations students would read in the two motivation intervention conditions. There is mixed research regarding whether and how source characteristics of messages influence individuals' likelihoods of internalizing those messages (Petty & Cacioppo, 1986). Petty and Cacioppo (1986) proposed an elaboration likelihood model of message persuasion to address these discrepancies, stating that students can make decisions regarding the persuasiveness of a message based on thorough analysis of its content, or based on less cognitively-taxing analysis of peripheral cues regarding the message, such as evaluating the appeal of the message source (see O'Keefe, 2013, for review). The availability of cognitive resources, students' motivation to process information, and the nature of the message being presented all impact the likelihood that students will engage in elaborate processing of information (see O'Keefe, 2013, for review). In the present study, students were asked to think about the quotations actively and deeply, received an incentive, and reflected on information in a way that was unlikely to challenge their existing attitudes directly. Thus I concluded that it was likely students would rely more on the central content of the intervention message than on the source of the information being presented to respond to the intervention.

Nonetheless, I wanted to ensure that the quotations had a maximally persuasive source in case students did rely on source information at all. Generally, sources who are more expert, more similar to message recipients, and more trustworthy tend to be more persuasive (Pornpitakpan, 2004; Tormala & Clarkson, 2007; Wilson & Sherrell, 1993). Thus I tried to make the source of the quotations in the cost and utility value intervention conditions credible, similar to students receiving the intervention, and relative experts in the content being discussed. I told students that the quotations came from slightly older students (ages 19 or 20) whose ages were compatible with them having taken the course in the previous year, and whose majors were frequently represented in Physics 161 (e.g., Mechanical Engineering). The older nature of the students and similar majors would make them seem like experts, but the students would still be similar to the Physics 161 students who were completing the study.

After developing the Session 1 interventions for the cost and utility value intervention conditions, I conducted pilot work to develop materials for the refresher dose of the intervention and control activities. The goal of the refresher was to remind students of the messages that they received in the initial intervention or control activity and to encourage them once again to process those messages actively and deeply. I adapted the structure of the refresher task based on a task administered by Gaspard, Dicke, Flunger, Brisson, et al. (2015) in prior utility value intervention work. However, I added material to their task that was geared towards reminding students more fully of the messages of the activities that they completed during Session 1. In particular, I included two quotations in the cost and utility value intervention conditions. In the utility value intervention condition, students re-read two of the quotations from Session 1 regarding how other students related physics to their lives.

In the cost intervention condition, I wrote two new quotations for students to read in the refresher condition. I did this because in pilot work, many students reported that sources of cost such as homework effort increased over a given semester. I did not want students to re-read a message about cost decreasing but then be frustrated that their own cost experiences actually had increased since the prior session. I tried to reduce any negative emotional consequences of reading quotations to the maximum extent possible. Thus, during summer, 2016, I met with a researcher who had conducted a motivation intervention focused on values affirmation that undermined outcomes for some students (Brady et al., 2016). Although that study utilized a different type of intervention than did the present study, I discussed what procedures to take during pilot work to try and mitigate similar undermining effects of intervention materials. Considering her recommendations, I wrote drafts of two quotations that focused specifically on cost that occurred midway through the semester and sought out feedback on them in Fall, 2016 with two focus groups of approximately six students each, who had taken Physics 161 during the previous semester. Participants also talked with me about how their cost experiences changed over the course of a semester. I also asked them very specifically about what negative reactions they had to any parts of either quotation and tried to reduce those reactions through revision to the quotations. Finally, I sought out feedback from a undergraduate and graduate students, and two experts in motivation theory and intervention research, to finalize the text of the quotations.

Utility value intervention condition. The full text of the utility value intervention condition can be found in Appendix A and took students on average 5.65 minutes to complete (range = 0.63 to 21.27 min, *SD* = 3.77). Students read that I was studying experiences in physics classes. I stated that I had interviewed other Physics 161 students and I wanted feedback from participants regarding what those students had said. I emphasized that the participating students were the best possible source of information about their own experiences in Physics 161and that is why I valued their feedback. Then, participating students read four quotations from male and female students describing ways in which physics related to their lives. The quotations expressed the following: (a) engineers used physics to innovate machines and help the environment; (b) computer scientists used physics to make video games more realistic; (c) physics was helpful to understand blood pressure readings; and (d) people thought about physics when watching action movies to see if scenes were plausible.

After reading the quotations, students ranked and evaluated them. First, students rank ordered the quotations from the most to least related to their lives. Students also evaluated each quotation according to two dimensions: whether they had a similar experience, and whether they found the quotation to be interesting. Next, students briefly noted what about the top-ranked quotation caused them to rank it highest. Finally, students generated their own examples of how what they were learning in physics related to their lives.

Session 1: Cost intervention condition. The full text of the cost intervention condition can be found in Appendix B and took students on average 5.34 minutes to complete (range = 0.35 to 18.21 min, SD = 3.89). Students read that I was conducting a survey about students' challenges in their physics class. I stated that I had interviewed other Physics 161 students about this topic and I wanted feedback from participants regarding what those students had said. I emphasized that the participating students were the best possible source of information about their own experiences in Physics 161and that is why I valued their feedback.

Then, participating students read four quotations from male and female students describing how they had addressed various experiences of cost in physics. The quotations discussed the following: (a) physics homework was effortful, but once students knew what to expect it did not feel like as much effort; (b) studying for exams was effortful, but students reminded themselves that it was temporary and that made it feel less so; (c) juggling physics with other courses was effortful, but others were going through the same thing and over time students realized they might have overestimated how much effort this took; (d) working very hard and not receiving a good grade was frustrating, but the frustration was temporary and other students were going through the same experience. Participating students ranked and evaluated these quotations using the same task as in the utility value intervention condition.

Session 1: Control conditions. I included two different control conditions in this study in order to assess whether the cost and utility value interventions were effective compared to receiving no activities, or compared to receiving a cognitively engaging activity that did not provide motivation support. I wanted to explore whether students might have performed better in the intervention conditions because they spent time writing about their course material, rather than because they actually experienced changes to their motivation. If the summary activity improved students' course outcomes in the same way that the two intervention activities did, that would suggest that spending more time thinking about physics class was responsible for the effects of the intervention, rather than receiving motivation support for physics class.

Students in the survey control condition responded to the same baseline and post-test motivation surveys that the other groups received. However, they did not complete any other activities. Students in the summary condition completed a task that took on average 7.18 minutes to complete (range = 0.86 to 26.43 min, SD = 6.46). The full text of this activity can be found in Appendix C. I adapted this activity to be consistent with summary tasks used in prior utility value intervention research (Hulleman et al., 2010; Hulleman & Harackiewicz, 2009; Rosenzweig et al., 2017a). Students read that physics class could be challenging, but there were strategies that could help them learn physics better and I wanted their feedback on one such strategy. Next, they described a topic that they were currently learning in physics. They were told that they should try to write about a topic that was challenging for them. They described the calculations they would need to do to complete a problem associated with that topic. Finally, they wrote a problem and its solution related to the topic.

Session 2: Refresher activity. I administered a refresher activity to students one month after they completed the initial intervention or control activities. The goal of the refresher was to remind students of the messages that they received during the initial intervention or control activity and to encourage them once again to process those messages actively and deeply.

The full text of the refresher task can be found in Appendix D. I did not measure how long students took on the refresher task, but it was designed to take approximately 5-10 minutes. In the survey control condition, students completed a questionnaire measuring their motivation but did not complete other activities. In the other conditions, students wrote down what they remembered about the task they completed in Session 1. The purpose of this was to encourage students to remember the intervention or summary condition's message again. Also, quizzing students has been shown to improve memory for the tested information (Roediger & Karpicke, 2006). Students were then reminded of the purpose of the task (e.g., "Just to remind you, last time we asked you to think about overcoming challenges you experienced in physics").

After this, activities differed by condition. In the utility value intervention condition, students re-read the two quotations that were ranked most favorably during Session 1. Then, they noted whether their thinking about how physics related to their lives had changed since they completed the first activity and wrote about why it did or did not change. Finally, they wrote a brief essay explaining how physics related to their lives. I told students that I wanted them to write about the same general topic from Session 1 so I could study how students thought about this topic at different points in the semester. Students were told that they could write about the same information that they wrote about in Session 1 or something new. These instructions aimed to maximize student autonomy and engagement with the activity.

In the cost intervention condition, students read two new quotations regarding how other students dealt with mid-semester challenges in Physics 161. As noted above, these quotations were specific to students' cost experiences later in the semester. In both quotations, the students reflected that their cost experiences were manageable during the remainder of the semester and that they knew other students experienced the same challenges. After reading the two quotations, students in the cost intervention condition wrote about whether either or both of the statements were similar to their own experiences. They also wrote which statement they liked more, and why. Finally, they were asked to write a brief essay explaining how they overcame a challenge in physics. Similar to the utility value intervention condition, students were told their feedback was the best source of information about students' experiences in Physics 161 at multiple points in the semester, and that they could write about the same information as they wrote about in Session 1 or something new.

Students in the Summary condition reported whether or not they remembered what topic they summarized during the previous session. Students who did remember the topic they summarized were asked to write why they had chosen that topic, and to discuss whether their thinking about the topic had changed since the first session. Students who did not remember which topic they wrote about were asked to choose a topic that they remember learning about before their first physics exam and to write about whether their thinking regarding that topic had changed over the course of the semester.

Measures. The complete list of items administered in this study is located in Appendix E. It is worth noting here that competence-related beliefs served as both a moderating variable (when measured at baseline) and an outcome variable (when measured at the end of Sessions 1, 2, and 3) in this study. For clarity, whenever I refer to competence-related beliefs as a moderator and not as an outcome, I use the term "baseline competence-related beliefs."

Pre-intervention motivation questionnaire. Students completed a 10-item questionnaire before beginning Session 1 of the intervention, intended to measure three motivational constructs that might (based on the research reviewed in Chapter 2) moderate the effects of the different experimental conditions. All items on the questionnaire were randomized.

Baseline competence-related beliefs. I assessed students' beliefs related to their competence to learn physics using a questionnaire adapted from previous EEVT research that has been well-validated in the field (Eccles & Wigfield, 1995). This questionnaire contained five items, three of which assessed students' current ability beliefs (sample item: "How good in Physics are you?") and two of which assessed students' future expectations for success (sample item: "How well do you expect to do in Physics this semester?"). Students responded to these items using a 7-point Likert scale with anchor terms that differed for each question (see Appendix E). I created an average score based on these responses across the five items (α = .93).

Belonging uncertainty in physics. Students completed two items assessing their belonging uncertainty in physics class. These items were adapted from Harackiewicz et al. (2016; sample item: "When something bad happens, I feel that maybe I don't belong at the University of Maryland.") Students responded to items using a 6-point Likert scale with anchors ranging from *Strongly Disagree* to *Strongly Agree*. I created an average score across the two items (α = .75).

Beliefs about the malleability of intelligence. Students completed three items assessing their beliefs regarding whether intelligence could change over time or was a stable, unchanging entity. These items were adapted from Dweck, Chiu, and Hong (1995; sample item: "You can learn new things, but you can't really change your basic intelligence."). Students responded to these items using a 6- point Likert scale with anchors ranging from *Strongly Disagree* to *Strongly Agree.* Higher scores meant that students had a stronger belief that intelligence was fixed, and lower scores meant that students held a stronger belief that intelligence was malleable. I created an average score across the three items (α = .89).

Prior achievement. I measured students' prior physics achievement using their scores on the first exam that they took in Physics 161. Students took two midterms and a final exam in Physics 161, evenly spaced across the semester. The first course exam occurred one week before students received the first session of this study. I chose exam scores as an indicator of prior achievement rather than overall or STEM GPAs for two reasons. First, many of the students were college freshmen and did not yet have a college GPA. Second, overall or STEM GPA does not necessarily reflect physics-specific achievement, which is the focus of the present study.

Post-intervention motivation questionnaire. Students completed a post-intervention questionnaire assessing competence-related beliefs, the three components of task value, and perceived cost. This questionnaire was administered once after the initial intervention or control activity, again after the refresher activity, and a third time at the end of the semester (see Procedure). All items were randomized.

Utility value. I measured students' perceptions of utility value for learning physics using the Eccles and Wigfield (1995) questionnaire described above. This was assessed using two items (sample item: "In general, how useful is what you learn in Physics?"). Students responded to these items using a 7-point Likert scale with anchor terms differing depending on the question (see Appendix E). I computed an average score across the items (Session 1: α = .86; Session 2: α = .86; Session 3: α = .90).

Perceptions of cost. I assessed students' perceptions of cost in physics using a questionnaire developed and validated by Flake et al. (2015) with college students. This questionnaire assessed three dimensions of cost (task effort, outside effort, and emotional cost) with 4-6 items for each dimension (sample item for task effort cost: "This class demands too much of my time."). Students responded to all items using 7-point Likert scales ranging from *Strongly Disagree* to *Strongly Agree.* I computed an average score across all items to represent total cost at each time point, as well as an average score for each dimension of cost at each time point (for effort cost: Session 1: α = .94; Session 2: α

= .95; Session 3: α = .93; for outside effort cost: Session 1: α = .92; Session 2: α = .95; Session 3: α = .91; for emotional cost: Session 1: α = .95; Session 2: α = .95; Session 3: α = .94 ; for total cost: Session 1: α = .96; Session 2: α = .98; Session 3: α = .96). The Flake et al. (2015) scale also included items to assess loss of valued alternatives cost. However, I did not utilize those items in the present study because the cost intervention did not target this dimension of cost.

Competence-related beliefs. I assessed students' competence-related beliefs in physics at post-test, using the same five items that I used at pre-test (Eccles & Wigfield, 1995; see Appendix E). I computed an average score across the five items at each time point (Session 1: α = .94; Session 2: α = .93; Session 3: α = .94).

Intrinsic and attainment value. I assessed students' perceptions of how important physics was to their sense of self, and of how interesting physics was, using four items from the Eccles and Wigfield (1995) questionnaire described above. There were two items for each component of value (sample item for intrinsic value: "How much do you like doing Physics?"). Students responded to these items using 7-point Likert scales with anchor terms differing depending on the question (see Appendix E). I computed an average score for each component of value at each time point (for intrinsic value, Session 1: α = .81; Session 2: α = .76; Session 3: α = .86; for attainment value, Session 1: α = .81; Session 2: α = .83; Session 3: α = .90).

Post-intervention physics outcomes. I collected data regarding students' academic outcomes in physics class in different ways.

Physics achievement. I collected three outcomes related to physics achievement. First, I measured students' average scores on the quizzes they took in Physics 161.

Students completed approximately 8 quizzes throughout the semester. Three of those quizzes occurred prior to the first intervention session, so I computed an average score for each student across the five quizzes that occurred following Session 1 of the intervention. Second, I measured students' exam scores. I computed an average score across the two exams students took after the intervention, as well as a separate score for each exam. Exam and quiz scores were averages of proportions of points earned and scores ranged from 0 – 1. Third, I collected students' final percentage grades in the course. These were based on in-class exercise participation, homework participation, quiz scores, and exam scores and were scored on a scale from 0 – 100.

Physics course participation. I measured two outcomes related to students' participation in physics class. First, I collected students' attendance rates in their course discussion sections. Teaching assistants for the course took attendance on one to four randomly-selected occasions over the course of the semester. I computed an attendance rate for each student by dividing the total number of sections they were marked as present by the number of sections for which their teaching assistant had taken attendance.

Second, I measured the average amount of time per assignment (in minutes) that students spent logged into the website that contained their homework assignments. This homework website logged students out once they had been inactive for approximately 20 minutes. Physics 161 students spent approximately 136.63 minutes per assignment (SD = 53.97) logged into this website in the present study. Students completed 12 homework assignments over the course of the semester, 6 of which occurred after the first intervention session. I computed an average score for each student based on the average time they took to complete the 6 assignments that occurred after the intervention.

Physics and STEM course-taking patterns. I collected data regarding the courses in which students were enrolled during the Spring, 2017, semester from the University of Maryland registrar's office in mid-February, 2017. The date on which I collected data was after the university's add/drop period had ended for the semester. I measured two different variables. First, I created a dichotomous variable to code whether or not students enrolled in the next course in the physics sequence that followed Physics 161 (yes = 1; no = 0). Second, I counted the number of courses in STEM fields in which students enrolled during the Spring, 2017 semester. Any courses that were administered in the natural sciences, engineering, mathematics, or computer science departments, or similar departments, were coded as STEM courses. I did not include information sciences or social sciences courses, or courses that used math but were not housed within a STEM-specific department (e.g., business statistics), in this count.

Responses to intervention prompts. I measured three variables to address whether students responded to the intervention prompts and essays in the manner that I intended.

Responses to questions. I assessed whether students responded to the intervention or summary materials they were given or whether they clicked through the prompts without responding. I coded three dichotomous variables (1 = yes; 0 = no) for each student in the cost or utility value intervention conditions to assess whether they: (a) wrote in answers for all short answer questions that followed the quotations in Session 1; (b) wrote in answers for the essays they wrote at Session 1; and (c) wrote in answers for the essays they wrote at Session 2. Students who wrote in nonsensical answers (e.g., a string of random letters) were coded as having not answered the question. I coded two variables for students in the control summary condition: (a) how many of the four questions in Session 1 they answered with relevant responses; and (b) how many of the two questions in Session 2 they answered with relevant responses.

Written responses relevant to targeted motivational variables. I examined whether students responded to the essay prompts at each session by writing about the motivational constructs that were the targets of the intervention (i.e., cost and utility value). The essay prompts did not ask students to write about their motivation explicitly, so it was possible that students could have answered the questions without writing about cost or utility value. This was more likely in the cost intervention condition, because students received a general prompt to write about course challenges that could have been interpreted in different ways.

I coded students' essay responses at Sessions 1 and 2 dichotomously (1 = yes; 0 = no) to measure whether students wrote about the utility value of physics or not (in the utility value intervention condition) or whether they wrote about their cost experiences in physics or not (in the cost intervention condition). A trained research assistant familiar with motivation theory, but unfamiliar with the specific hypotheses of this study, also coded students' responses in this way. We resolved any discrepancies in coding through consensus (approximately 15-20% of the cost essays and 5-10% of the utility value essays were resolved in this way). Essays were coded as relevant to utility value if they discussed either the general utility of physics or the specific utility of physics for a given student's life. Essays were coded as relevant to cost if students referenced problems with managing effort or coursework across courses, emotional experiences such as frustration and anxiety, negative consequences that arose out of putting in effort to a course, or giving up activities in order to do coursework. Students who did not write essays were included in analyses, but were given "0" scores.

*Reports of change to targeted motivational variables***.** I examined whether students in either intervention condition explicitly wrote that their perceptions of cost or utility value had changed between Session 1 and Session 2. Students completed an open-ended question at the beginning of Session 2 that asked whether their experiences with course challenges (in the cost intervention condition) or their thinking about how course material related to their lives (in the utility value intervention condition) had changed since the last intervention session. I coded a dichotomous variable (1 = yes; 0 = no) for whether students wrote in that the targeted motivational construct had changed (either positively or negatively) or not.

Post-intervention demographic and participation questionnaire. Students completed a post-intervention questionnaire in which they reported their gender, ethnicity, year in school, major or intended major, and whether or not Physics 161 was a required course for them. They also reported the extent to which they were engaged with the activities during the session, and whether they experienced technical difficulties during the session or were distracted while completing the activities. Students completed demographic questions once, after Session 1; they completed engagement, distraction, and technical difficulty questions after all three sessions. Students responded to engagement and distraction questions using 5-point Likert scales ranging from *Not at All* to *Extremely*.

Procedure

This study consisted of three sessions: the initial activity (Session 1), the refresher dose (Session 2), and a follow-up measurement session (Session 3). Participants completed all three sessions online using the Qualtrics research suite. The course professor reduced the number of questions he asked students on their homework assignments during the weeks in which they completed a session for this study.

Session 1: Initial activity. Approximately six weeks into the Fall, 2016 semester, and one week after their first course exams, students completed a homework assignment that contained an online link directing them to Session 1 of the study. First, they read an information form and consent form that briefly described the nature of the dissertation study and the associated data collection for it. The form included a statement that different students might complete different activities than other students in the class. The purpose of the statement was to prevent students from becoming suspicious if they noticed that they were completing a different activity than their peers. Next, students were directed to the pre-test motivation questionnaire, and then they were randomly assigned to complete one of the four study conditions. They completed the intervention or control activities associated with the condition to which they were assigned. Then, they completed the post-intervention motivation, demographic, and participation questionnaires.

Session 2: Refresher activity. Four weeks after Session 1 was sent out to students, and one week after students completed their second course exams, they again clicked on an online link as part of their weekly homework. They indicated their university ID numbers to the online system, which automatically assigned students back into the condition that they completed in Session 1 and provided the appropriate refresher dose to them. After that, students again completed the post-intervention motivation questionnaire. Only students who completed Session 1 of the study were eligible to participate in Session 2.

Session 3. As part of the last homework assignment of the semester, students in all conditions were directed to an online link that contained the post-test motivation questionnaire. Only students who completed Session 1 of the study were eligible to participate in Session 3.

Analysis of Research Questions

I used multiple linear regression to evaluate my research questions. This method has been used in most previous utility value intervention research (e.g., Durik et al., 2014; Harackiewicz et al., 2016; Hulleman et al., 2010; Hulleman & Harackiewicz, 2009). I conducted all data analyses using SPSS Version 23. Students who completed Session 1 but not Sessions 2 and/or 3 were included in any analyses for which they provided sufficient data.

My first five research questions concerned the effects of the intervention conditions on various outcomes. These outcomes were: (a) perceived utility value, each dimension of students' perceived cost, and total perceived cost, at three time points (Research Question 1); (b) students' competence-related beliefs, intrinsic value, and attainment value, at three time points (Research Question 2); (c) students' course achievement, in terms of average quiz scores, individual and average exam scores, and final grades in the course (Research Question 3); (d) students' course participation, in terms of average time spent on homework and attendance rates in discussion section sessions (Research Question 4); and (e) students' physics and STEM course-taking patterns during the semester following the intervention (Research Question 5).

To answer Research Questions 1 – 5, I ran linear regression model to evaluate each outcome (except in the case of enrollment in the next physics course, in which I ran a logistic regression model). I included three terms in each regression model, representing the two intervention conditions and two control conditions, dummy coded (in different analyses, the survey control condition or the summary condition served as the reference group). The regression coefficients for the cost and utility value intervention terms in Step 1 were used to determine the strength and significance of the impacts of receiving each intervention compared to receiving either control condition.

As noted in Chapter 1, I expected students who received the utility value intervention to show higher utility value, but not to differ in cost, compared to students in the survey control condition at all three time points. I also expected students who received the cost intervention to show lower cost, but not to differ in utility value, compared to students in the survey control condition at all three time points. I did not make specific hypotheses regarding the effects of either intervention on attainment value, intrinsic value, or competence-related beliefs, or about the effects of the summary condition. For Research Questions 1 - 2, I assessed separately each motivational construct at each time point. In the exploratory mediation analyses (Research Questions E1 – E3), I assessed whether the intervention conditions predicted change in utility value or cost over time.

For Research Questions 3 - 5, I predicted that students who received the utility value intervention or the cost intervention would show higher scores on all outcomes related to course achievement, participation, and course-taking compared to students in the survey control condition. I did not make specific hypotheses regarding the summary condition or whether one intervention would affect outcomes more than another.

Research Question 6 addressed whether any of six student-level variables found to be moderators in previous research (baseline competence-related beliefs, prior achievement, belonging uncertainty, beliefs about the malleability of intelligence, gender, or ethnicity) moderated the impacts of either intervention condition relative to either control condition. I explored each of the six moderating variable in separate analyses in order to assess their potential moderation effects independently. To assess this question, I added an additional step to each of the regression models just described. For most analyses, Step 2 included (a) one term representing a given moderating variables as a covariate: baseline competence-related beliefs (standardized); prior achievement (standardized); belonging uncertainty (standardized); beliefs about the malleability of intelligence (standardized); gender (dummy-coded: female = 1; male = 0); and (b) three interaction terms, representing one of the moderating variables multiplied by each of the three dummy-coded terms in Steps 1 and 2 (e.g., for gender, the terms included in Step 3 were: gender x cost intervention vs. survey control, gender x utility intervention vs. survey control, gender x summary vs. survey control). The exception was ethnicity as a moderator, which was represented by three dummy-coded variables. In Step 2 of those models I planned to include three terms representing ethnicity as a moderator (African American, Asian or Asian American, Other Ethnicities, and European American, dummy-coded with European American students as the reference group) and nine interaction terms.

If there were any significant interactions, I followed up by estimating the mean scores for each experimental condition at representative high and low levels of the moderating variable (Aiken & West, 1991). This provided me with a visual depiction of the nature of the interaction that I had observed. I then conducted simple effects analysis to explore whether the differences between a given condition and the reference condition were significant at those different levels of the moderating variable (Aiken & West, 1991). For continuous moderators, I tested for significance at representative high and low levels of the moderator (i.e., one standard deviation above and below the mean). For categorical moderators, I tested for significance at the two levels of the moderator (e.g., males and females). I used the PROCESS macro in SPSS (Hayes, 2013; Model 1) to conduct these analyses.

As noted in Chapters 1 and 2, moderation effects that have been observed in prior motivation interventions have differed widely. I did not make specific hypotheses regarding what the nature of effects would be for any moderator, because results from past studies have been mixed. However, I acknowledged that in the utility value intervention, students' baseline competence-related beliefs and/or prior achievement were more likely to moderate results compared to the other, more exploratory moderators. I considered all moderators to be exploratory when evaluating the effects of the cost intervention versus the survey control condition.

My final research questions concerned whether utility value or cost measured over the course of the semester mediated the relationship between the two interventions (compared to the survey control condition) and two student outcomes that I measured at the end of the semester: exam 3 scores and number of STEM courses in which students enrolled during the semester following the intervention. I measured students' competence-related beliefs as an additional mediator, because it was possible cost or utility value interventions might impact competence-related beliefs (e.g., Hulleman et al., 2016). I used the survey control condition as the control group for these analyses.

There are different ways to assess mediation and indirect effects (Baron & Kenny, 1986; Shrout & Bolger, 2002; Williams & Mackinnon, 2008). The most parsimonious method is to explore indirect effects within a structural equation model that includes the two interventions versus the control group, the two motivational constructs measured at three time points, and the two outcomes measured at the end of the semester. I could not run such a model because my sample size was too small. Instead, I chose to conduct a series of exploratory analyses within the regression framework to assess mediation. In regression, the bootstrapping method is considered to be more powerful with small or medium-sized samples than are other methods such as Baron and Kenny's (1986) causal steps approach (Hayes, 2009; Shrout & Bolger, 2002; Williams & Mackinnon, 2008). In bootstrapping, cases from a set of data are repeatedly sampled with replacement and estimates of the different paths in a mediation model are computed for each new sub-sample that is created. This method provides many estimates of the indirect effect of an independent variable on the dependent variable via the mediating variable, which are averaged together to produce an overall estimate and a specified confidence interval of the indirect effect (e.g., 95%). If the 95% confidence interval does not contain zero, then the mediating variable is considered to be a significant mediator of the relationship between the independent and dependent variable at an alpha level of 0.05 (Hayes, 2009).

Bootstrapping can be conducted within SPSS using the PROCESS macro developed by Hayes (2013).

I ran twelve mediation models which explored the relationship between a particular intervention condition (the cost intervention versus the survey control condition; the utility value intervention versus the survey control condition), a particular motivational variable across three time points as a mediator (cost; utility value; competence-related beliefs), and a particular outcome (final exam scores; course-taking intentions). Cost, utility value, and competence-related beliefs were modeled as serial mediator variables, each measured at three time points during the semester (Sessions 1, 2, and 3). The PROCESS macro (Hayes, 2013) administered in SPSS can evaluate a serial mediator model with a two-category predictor variable (Model 6). I used the macro to evaluate each mediation model with a 95% confidence interval and 5000 bootstrap resamples, in accordance with Hayes' (2013) recommendations. The mediation procedure produced a point estimate for the total indirect effect of the intervention on an outcome via a given mediating variable (utility value, cost, or competence-related beliefs) measured across three time points. The total indirect effect was a sum of seven indirect effects: the effects of the intervention on the outcome via the mediating variable at each *individual* session (i.e., at Session 1, at Session 2, at Session 3), across two of the three sessions (i.e., across Sessions 1 and 2, across Sessions 1 and 3, across Sessions 2 and 3), and across all three sessions (i.e., across Sessions 1, 2, and 3). The procedure also produced a 95% confidence interval for the total indirect effect; if the confidence interval did not contain zero, I could consider the indirect effect to be significant using an alpha criterion of $p < .05$.

I hypothesized that cost would significantly mediate the effect of receiving the cost intervention versus the survey control condition on each outcome, but utility value would not. Also, utility value would significantly mediate the effect of receiving the utility value intervention versus the survey control condition on each outcome, but cost would not. These hypotheses were based on the total indirect effects I would observe. I did not make specific hypotheses regarding whether competence-related beliefs would mediate any relationships.

Addressing the potential for Type 1 Error. I ran many statistical analyses in this study because I had many outcome variables and moderating variables of interest. I acknowledged that there could be an increased risk of making Type 1 errors in interpreting my results as a result of running so many analyses (i.e., I might find a significant effect in my analyses that does not reflect an actual effect). To mitigate this concern, I reported and interpreted effect sizes throughout my results rather than only interpreting the exact *p* values of my effects; this approach follows the recommendations of Nakagawa (2004). The effect sizes I planned to report and interpret were standardized and unstandardized regression coefficient values representing the regression coefficients of different terms in each model. This approach was appropriate because most analyses were based in *a priori* theoretical reasoning about the effects of the interventions on different outcomes. Even when I did not have specific hypotheses, I planned to evaluate constructs that were reasonable candidates to be affected by the given interventions based on previous theory and research. A second step I took was to interpret significant effects that I found with attention to *patterns* of results rather than interpreting heavily each individual significant effect that I observed.

Chapter 4: Results

Study Participation, Engagement, and Attrition

Participation and attrition rates. One hundred sixty-two students completed the first session of the intervention, representing 89.5% of all students enrolled in Physics 161. Most students of the 162 who completed Session 1 of the study also completed Sessions 2 and 3; 143 students (88%) participated in Session 2 and 143 also participated in Session 3 (although not all the same students completed Sessions 2 and 3). Students needed to complete the study activities in order to receive course credit, but they could opt out of releasing their responses to the activities or their course outcome data. Thirteen students of 162 requested that none of their data be included in analyses, one requested that intervention participation and questionnaire data not be included, one requested that demographic information not be included, and eleven requested that different aspects of their course outcome data not be included.

I obtained course enrollment data for the Spring, 2017 semester from the university registrar for 137 students of the 143 who consented to release that information (95.8%). Data from six students could not be obtained due to students typing in incorrect university ID numbers. I obtained course achievement information for 134 students of the 139 who consented to release their course achievement information (96.4%) from the course professor. In this circumstance, three students' ID numbers could not be matched with information from the professor's files. There was also missing data for two students because they withdrew from the course. This resulted in a final sample of 148 students providing data at Session 1 (91.3% of participants), 129 students providing data at Session 2 (79.6% of participants), 132 students providing data at Session 3 (81.4% of participants), 134 students providing course achievement data (82.7% of participants), and 137 students providing course enrollment data (84.5% of participants). Table 2 reports the exact sample size for each variable in the study.

Engagement, distraction, and time taken on Session 1 activities. Table 1 presents descriptive statistics for students' self-reported ratings of engagement and distraction at each of the three intervention sessions. As can be seen, students' mean self-reported engagement was moderate and distraction was relatively low during each of the three sessions. I evaluated whether these ratings differed across study session, using a repeated-measures ANOVA in SPSS. The results were not significant, suggesting that students' engagement and distraction ratings were similar across all three intervention sessions.

Table 1

Descriptive Statistics for Intervention Participation Variables

	N	**M**	**SD**
Session 1			
Engagement	148	3.01	0.89
Distraction	148	2.22	1.01
Task Time	121	349.48	267.36
Session 2			
Engagement	127	3.09	0.88
Distraction	127	2.41	1.11
Session 3			
Engagement	132	3.20	0.96
Distraction	132	2.21	1.15

I also tested whether students' engagement and distraction ratings differed by study condition at each time point using regression with three dummy codes to represent each condition (cost intervention, utility value intervention, and summary) compared to the survey control condition as a reference group. In Session 1, there were some significant differences by condition: Students in the cost intervention condition rated their distraction to be significantly higher ($M = 2.42$, $SD = 1.03$) than did students in the survey control condition ($M = 1.88$, $SD = 0.83$), $\beta = .25$, $t(144) = 2.17$, $p = .03$, and they also rated their engagement to be significantly lower ($M = 2.92$, $SD = 0.85$ versus $M = 3.40$, $SD = 0.71$), $\beta = -.26$, $t(144) = -2.25$, $p = .03$. Also in Session 1, students in the summary condition rated their engagement to be significantly lower ($M = 2.70$, $SD = 1.02$) than did students in the survey control condition, $\beta = -.29$, $t(144) = -2.80$, $p = .01$. There were no significant differences at Sessions 2 or 3.

Table 1 also presents descriptive statistics for students' overall time taken to complete the intervention or summary activities in Session 1. This value did not include the amount of time it took students to complete baseline or post-test questionnaires. In analysis of this variable, I omitted two outliers representing students whose times taken to complete the task were very long. I tested whether the time it took students to complete Session 1 activities differed by condition using a one-way ANOVA with the three groups that completed activities (cost intervention, utility value intervention, and summary). Results of this analysis were not significant, $F(2,120) = 1.39$, $p = .25$, suggesting that students took similar amounts of time to complete their respective activities across the three conditions.

Responses to intervention prompts. As discussed in Chapter 3, I conducted three types of analyses to explore whether students responded to the intervention prompts and essays in the manner that I intended. I assessed first whether students actually responded to the intervention prompts or whether they clicked through the prompts without responding. Most students responded to most of the questions and prompts. In Session 1, all students except two evaluated the quotations if they received the cost or utility value intervention conditions. Furthermore, 77.1% of students in the cost intervention condition and 88.5% of students in the utility value intervention condition completed the short answer and essay-writing questions. In Session 2, 75.0% of students in the cost intervention condition and 78.8% of students in the utility value intervention condition completed the essay-writing questions. In the summary condition, I asked students four questions at Session 1 and two questions at Session 2. At Session 1, 96.7% of students answered at least two of these questions, 69.5% of students answered at least three out of the four, and 39.1% of students answered all four. In Session 2, 73.9% of students answered both questions, and 26.1% of students left both questions blank.

Second, I examined whether students seemed to respond to the essay prompts by writing about the motivational constructs that were the targets of the intervention (i.e., cost and utility value). The majority of students in the utility value intervention condition wrote essays that were related to utility value (86.5% in Session 1; 73.1% in Session 2). However, in the cost intervention condition, few students wrote essays that were directly about their experiences of cost (31.3% in Session 1; 20.8% in Session 2). Many students in the cost intervention condition wrote instead about challenges that were related to memorizing or understanding course material. Those experiences are related to cost, and students likely thought about cost while writing them, but the written responses did not reference explicitly students' own perceptions of cost or how these experiences became less negative over time.

Third, I examined whether students in either intervention condition explicitly believed that their perceptions of cost or utility value had changed between Session 1 and Session 2 of the intervention. Of the 46 students who answered this question in the utility value intervention condition, only nine (19.6%) reported that their thinking about how physics related to their lives had changed since Session 1. Most of the students who responded that their thinking had not changed wrote in that they thought physics was important to their lives before Session 1 and still thought that. The students who reported that their thinking had changed all stated that they found course material to be more relevant to their lives than they had at Session 1. Out of the 41 students who responded to the question in the cost intervention condition, twenty-four (58.5%) reported that they perceived their course challenges to be different between Sessions 1 and 2. However, these students were mixed in reporting whether they perceived their challenges to be more or less negative than they had at Session 1.[1]

Overall, most students responded to the intervention prompts in the manner that I intended. However, not all students explicitly noticed changes to their motivation after receiving the utility value intervention, and students seemed to report different directions of changes to cost in the cost intervention. Furthermore, not all students wrote essays that explicitly discussed their cost experiences in the cost intervention condition. These students likely still thought about their cost experiences, but they did not write explicitly about that topic.

[1] I discussed with my dissertation committee the possibility of analyzing students' responses to a question asking what they remembered about Session 1 of the cost intervention, in order to determine whether some quotations were more salient than others one month after the intervention. Most students responded to this question with a general statement such as "I wrote about how I felt about math." Few students reported on specific elements that they remembered about the intervention task, so results were not particularly informative and I do not discuss them further.

Analyses of the Research Questions

Overview of analyses. I tested six focal research questions and three exploratory mediation questions in the present study. My goal was to determine the overall impacts of the cost and utility value interventions on students' motivation and course outcomes, compared to a control condition. I included two control conditions in this study, summary and survey control, in order to learn how the cost and utility value interventions compared to tasks that provided different amounts of cognitive support to students. My hypotheses concerned only the survey control condition. However, I was curious to explore the effects of the interventions compared to the summary condition as well, to determine how the interventions compared to an intervention that provided cognitive (but not motivational) support. I evaluated the results of the intervention conditions compared to each control condition separately. My plan was to collapse data from the two control conditions together if the results looked similar. However, there were different patterns of results between the two conditions so I did not collapse together this data. Furthermore, as will be seen, the summary condition did not show a clearly interpretable pattern of results and impacted some students' motivation. I do report the significant findings that resulted from comparing the cost or utility value interventions to the summary condition. However, because results from the summary condition were not particularly informative, I do not provide complete statistical output for all analyses using this condition as a reference group. I also focus my discussion on analyses that used the survey control condition as a reference group.

All analyses were conducted using two-step linear regression or, in the case of enrollment in the next physics course, logistic regression. I used data from Step 1 of regression models to answer Research Questions 1-5. This step included three dummy-coded regression terms, one for each condition (coded as a 1) compared to a reference condition (coded as a 0; for my focal hypotheses, the survey control condition served as the reference condition). If a given term was significant, this suggested that the condition coded as a 1 impacted the outcome differently than did the reference condition. I used data from Step 2 of the regression models to answer Research Question 6. This step included four additional terms, one term representing a given moderating variable (standardized if the moderator was continuous, dummy-coded if the moderator was categorical; see Chapter 3) and three terms representing the interaction of that moderator with the three terms from Step 1. If an interaction term was significant, this suggested that the impact of the condition coded as a 1 (versus the reference condition) on the outcome depended on the moderating variable being assessed. When there was a significant interaction, I conducted follow-up analyses to interpret it further. I estimated the value of the outcome variable for the given condition and for the reference group, at different values of the moderating variable. For continuous moderators, I used representative high and low values of the moderator (i.e., one standard deviation above and below the mean); for categorical moderators, I used the two categories of the moderator. I compared these values in order to understand descriptively what the condition means looked like at the two values of the moderator (Aiken & West, 1991). I then conducted simple effects analyses using the PROCESS macro in SPSS (Hayes, 2013) to test whether the two conditions differed significantly at the two values of the moderator.

Descriptive statistics. The descriptive statistics for all continuous variables in this study, collapsed across conditions, are reported in Table 2; I report mean scores on these variables separated by condition throughout the remainder of the chapter. Overall, students showed fairly high motivation and prior achievement. They reported very high utility value and attainment value, and relatively high competence-related beliefs, at all three time points during the semester. They also reported relatively low cost and belonging uncertainty, and they began the intervention with a fairly strong belief that intelligence was malleable. Students had average achievement scores on most outcomes that were in the "B" grade range.

Correlations among the continuous moderating variables measured at baseline are reported in Table 3, correlations among motivational variables are reported in Table 4, and correlations among course outcomes are reported in Table 5. Correlations were in line with prior research and theory. The dimensions of cost showed very strong positive correlations with one another, and the components of task value showed moderate to strong positive correlations with one another and with competence-related beliefs. Cost and competence-related beliefs showed moderate to strong negative correlations with one another. The correlations between cost and the components of task value were less strong than those of cost with competence-related beliefs, and they were not always significant. Course outcomes all showed positive inter-correlations. Prior achievement correlated positively and strongly with baseline competence-related beliefs. These two measures did not correlate with students' fixed (versus malleable) beliefs about intelligence, but they showed a small negative correlation with belonging uncertainty. Fixed (versus malleable) beliefs about intelligence correlated positively with belonging uncertainty.

Table 2

Descriptive Statistics for Continuous Moderator and Outcome Variables

	N	M	SD
Baseline			
Baseline Competence-Related Beliefs	147	4.78	1.28
Prior Achievement (Exam 1 Scores)	133	0.79	0.20
Belonging Uncertainty	148	3.15	1.38
Beliefs about Intelligence	147	2.90	1.20
Session 1			
Utility Value	148	5.09	1.24
Effort Cost	145	3.34	1.26
Outside Effort Cost	144	3.35	1.36
Emotional Cost	148	3.39	1.52
Total Cost	141	3.33	1.25
Competence-Related Beliefs	148	4.83	1.26
Intrinsic Value	148	4.29	1.32
Attainment Value	148	5.39	1.20
Session 2			
Utility Value	127	5.07	1.29
Effort Cost	127	3.14	1.23
Outside Effort Cost	127	3.13	1.33
Emotional Cost	127	3.10	1.40
Total Cost	127	3.12	1.25
Competence-Related Beliefs	127	5.04	1.23
Intrinsic Value	127	4.34	1.35
Attainment Value	127	5.43	1.27
Session 3			
Utility Value	132	5.09	1.29
Effort Cost	132	3.52	1.19
Outside Effort Cost	132	3.50	1.28
Emotional Cost	132	3.46	1.33
Total Cost	132	3.49	1.17
Competence-Related Beliefs	132	4.93	1.24
Intrinsic Value	132	4.26	1.42
Attainment Value	132	5.31	1.30
Course Outcomes			
Average Quiz Scores	133	0.83	0.16
Exam 2 Scores	133	0.87	0.11

Exam 3 Scores	132	0.83	0.16
Average Exam Scores	132	0.85	0.12
Final Course Grades	134	87.38	10.80
Average Homework Time	139	136.63	53.97
Discussion Section Attendance Rates	134	2.81	1.11
Enrollment in STEM Courses	137	3.06	1.08

Table 3

Correlations Among Continuous Moderator Variables

	1.	2.	3.	4.
1. Baseline Competence-Related Beliefs	--			
2. Prior Achievement (Exam 1 Scores)	.67**	--		
3. Belonging Uncertainty	-.29**	-.34**	--	
4. Beliefs about Intelligence	-.11	-.12	.26**	--

Note: *$p \leq .05$; ** $p \leq .01$; $^{+}p < .10$. Variables were all measured at baseline.

Table 4

Correlations among Motivational Variables Measured After Sessions 1, 2, and 3

	1.	2.	3.	4.	5.	6.	7.	8.
Session 1								
1. Utility Value	--							
2. Effort Cost	-.33**	--						
3. Outside Effort Cost	-.14^{+}	.68**	--					
4. Emotional Cost	-.25**	.84**	.68**	--				
5. Total Cost	-.23**	.92**	.84**	.95**	--			
6. Competence-Related Beliefs	.52**	-.56**	-.46**	-.63**	-.60**	--		
7. Intrinsic Value	.56**	-.38**	-.30**	-.39**	-.38**	.64**	--	
8. Attainment Value	.60**	-.22**	-.14^{+}	-.14^{+}	-.14	.32**	.41**	--
Session 2								
1. Utility Value	--							
2. Effort Cost	-.32**	--						
3. Outside Effort Cost	-.22*	.84**	--					
4. Emotional Cost	-.37**	.86**	.80**	--				
5. Total Cost	-.33**	.95**	.92**	.96**	--			
6. Competence-Related Beliefs	.55**	-.59**	-.50**	-.68**	-.64**	--		
7. Intrinsic Value	.65**	-.38**	-.32**	-.37**	-.38**	.59**	--	
8. Attainment Value	.59**	-.20*	-.14	-.18*	-.19*	.38**	.53**	--
Session 3								
1. Utility Value	--							
2. Effort Cost	-.17^{+}	--						
3. Outside Effort Cost	-.08	.79**	--					
4. Emotional Cost	-.16^{+}	.85**	.70**	--				
5. Total Cost	-.15^{+}	.95**	.87**	.94**	--			
6. Competence-Related Beliefs	.42**	-.53**	-.40**	-.65**	-.59**	--		
7. Intrinsic Value	.69**	-.23**	-.21*	-.30**	-.27**	.54**	--	
8. Attainment Value	.73**	-.07	-.12	-.07	-.09	.34**	.62**	--

Note: $*p \leq .05$; $** p \leq .01$; $^{+}p < .10$.

Table 5

Correlations Among Course Outcome Variables

	1.	2.	3.	4.	5.	6.	7.	8.	9.
1. Average Quiz Scores	--								
2. Exam 2 Scores	.32**	--							
3. Exam 3 Scores	.51**	.58**	--						
4. Average Exam Scores	.48**	.84**	.93**	--					
5. Course Grades	.72**	.66**	.81**	.84**	--				
6. Average Homework Time	.19*	.05	.11	.10	.17^{+}	--			
7. Discussion Section Attendance	.35**	.09	.29**	.25**	.34**	.34**	--		
8. Enrollment in STEM Courses	.38**	.25**	.31**	.32**	.31**	.15^{+}	.43**	--	
9. Enrollment in Next Physics Course	.30**	.25**	.31**	.33**	.33**	.11	.32**	.43**	--

Note: *$p \leq .05$; ** $p \leq .01$; $^{+}p < .10$.

Research Question 1. I evaluated how the cost and utility value interventions impacted the focal motivational constructs targeted by the interventions (i.e., effort cost, outside effort cost, emotional cost, total cost, and utility value) at each of three time points (i.e., Sessions 1, 2, and 3). Students' mean scores by condition on these variables are reported in Table 6, and regression results for these outcomes are reported in Table 7. Contrary to my hypotheses, neither the cost intervention condition nor the utility value intervention condition impacted significantly students' cost or utility value, compared to either the survey control condition or the summary condition.

Table 6

Mean Scores by Condition on Cost and Utility Value (Research Question 1)

	Condition											
	Cost Intervention			Utility Value Intervention			Summary			Survey Control		
	N	**M**	**SD**	**N**	**M**	**SD**	**N**	**M**	**SD**	**N**	**M**	**SD**
Session 1												
Utility Value	48	4.97	1.33	52	5.12	1.25	23	5.13	1.18	25	5.26	1.10
Effort Cost	48	3.45	1.13	52	3.30	1.33	22	3.39	1.39	23	3.17	1.30
Outside Effort Cost	47	3.52	1.23	50	3.25	1.41	23	3.62	1.46	24	3.00	1.37
Emotional Cost	48	3.58	1.43	52	3.32	1.59	23	3.34	1.52	25	3.19	1.61
Total Cost	47	3.47	1.06	50	3.24	1.33	22	3.41	1.39	22	3.14	1.36
Session 2												
Utility Value	41	4.96	1.43	46	5.22	1.07	17	5.09	1.60	23	4.98	1.22
Effort Cost	41	3.21	1.15	46	3.22	1.18	17	2.76	1.46	23	3.11	1.33
Outside Effort Cost	41	3.12	1.32	46	3.22	1.36	17	3.09	1.52	23	3.03	1.19
Emotional Cost	41	3.09	1.34	46	3.21	1.34	17	2.97	1.64	23	2.96	1.51
Total Cost	41	3.14	1.21	46	3.22	1.23	17	2.93	1.47	23	3.03	1.28
Session 3												
Utility Value	44	4.89	1.45	45	5.16	1.15	19	5.53	1.15	24	5.00	1.35
Effort Cost	44	3.50	1.19	45	3.57	1.11	19	3.82	1.37	24	3.21	1.18
Outside Effort Cost	44	3.43	1.19	45	3.57	1.37	19	3.84	1.28	24	3.22	1.30
Emotional Cost	44	3.36	1.37	45	3.42	1.14	19	3.81	1.31	24	3.45	1.58
Total Cost	44	3.42	1.17	45	3.51	1.09	19	3.82	1.20	24	3.31	1.32

Table 7

Regression Results on Cost and Utility Value (Research Question 1)

	Utility Value		Effort Cost		Outside Effort Cost		Emotional Cost		Total Cost	
	B	**S.E.**	**B**	**S.E.**	**B**	**S.E.**	**B**	**S.E.**	**B**	**S.E.**
Session 1										
Intercept	5.26	0.25	3.17	0.27	3.00	0.28	3.19	0.31	3.14	0.27
Cost Intervention (Cost)	-0.29	0.31	0.27	0.32	0.52	0.34	0.40	0.38	0.33	0.33
Utility Value Intervention (UV)	-0.15	0.30	0.12	0.32	0.25	0.34	0.14	0.37	0.10	0.32
Summary (Sum)	-0.13	0.36	0.22	0.38	0.62	0.40	0.15	0.44	0.27	0.38
Model R^2	.01		.01		.03		.01		.01	
Session 2										
Intercept	4.98	0.27	3.11	0.26	3.03	0.28	2.96	0.30	3.03	0.26
Cost Intervention (Cost)	-0.02	0.34	0.10	0.32	0.08	0.35	0.13	0.37	0.11	0.33
Utility Value Intervention (UV)	0.24	0.33	0.11	0.32	0.19	0.34	0.25	0.36	0.19	0.32
Summary (Sum)	0.11	0.42	-0.35	0.40	0.06	0.43	0.01	0.45	-0.10	0.40
Model R^2	.01		.02		.003		.01		.01	
Session 3										
Intercept	5.00	0.26	3.21	0.24	3.22	0.26	3.45	0.27	3.31	0.24
Cost Intervention (Cost)	-0.11	0.33	0.29	0.30	0.21	0.33	-0.09	0.34	0.12	0.30
Utility Value Intervention (UV)	0.16	0.33	0.37	0.30	0.35	0.33	-0.03	0.34	0.20	0.30
Summary (Sum)	0.53	0.40	0.61^{+}	0.36	0.62	0.39	0.36	0.41	0.51	0.36
Model R^2	.03		.02		.02		.01		.02	

Note: *$p \leq .05$; ** $p \leq .01$; $^{+}p < .10$. All results show unstandardized slope and intercept values and are based on the survey control condition as a reference group. Standardized slope values for significant effects are reported in text. All intercept values are significant at $p < .01$.

Research Question 2. I next evaluated how the cost and utility value interventions impacted motivational constructs that were not targeted directly by the interventions (i.e., competence-related beliefs, intrinsic value, and attainment value). Students' mean scores by condition on these constructs are reported in Table 8, and regression results for these outcomes are reported in Table 9. Again, neither the cost intervention nor the utility value intervention impacted significantly these aspects of students' motivation compared to either control condition.

Table 8

Mean Scores by Condition on Non-Focal Motivational Constructs (Research Question 2)

	Condition											
	Cost Intervention			**Utility Value Intervention**			**Summary**			**Survey Control**		
	N	**M**	**SD**	**N**	**M**	**SD**	**N**	**M**	**SD**	**N**	**M**	**SD**
Session 1												
Competence-Related Beliefs	48	4.62	1.28	52	4.88	1.19	23	5.09	1.49	25	4.92	1.15
Intrinsic Value	48	4.27	1.31	52	4.25	1.36	23	4.46	1.23	25	4.28	1.39
Attainment Value	48	5.21	1.21	52	5.55	1.14	23	5.35	1.55	25	5.46	0.91
Session 2												
Competence-Related Beliefs	41	4.88	1.27	46	5.09	1.13	17	5.39	1.42	23	4.97	1.23
Intrinsic Value	41	4.38	1.50	46	4.17	1.28	17	4.59	1.36	23	4.43	1.25
Attainment Value	41	5.34	1.40	46	5.52	1.22	17	5.79	1.30	23	5.11	1.08
Session 3												
Competence-Related Beliefs	44	4.77	1.23	45	4.94	1.16	19	5.19	1.49	24	4.99	1.22
Intrinsic Value	44	4.15	1.46	45	4.19	1.40	19	4.63	1.37	24	4.31	1.45
Attainment Value	44	5.14	1.48	45	5.43	1.12	19	5.58	1.28	24	5.19	1.26

Table 9

Regression Results on Non-Focal Motivational Constructs (Research Question 2)

	Competence-Related Beliefs		Intrinsic Value		Attainment Value	
	B	**S.E.**	**B**	**S.E.**	**B**	**S.E.**
Session 1						
Intercept	4.92	0.25	4.28	0.27	5.46	0.24
Cost Intervention (Cost)	-0.30	0.31	-0.01	0.33	-0.25	0.30
Utility Value Intervention (UV)	-0.04	0.31	-0.03	0.32	0.09	0.29
Summary (Sum)	0.17	0.37	0.18	0.38	-0.11	0.35
Model R^2	.02		.01		.01	
Session 2						
Intercept	4.97	0.26	4.44	0.28	5.11	0.26
Cost Intervention (Cost)	-0.09	0.32	-0.06	0.36	0.23	0.33
Utility Value Intervention (UV)	0.13	0.32	-0.26	0.35	0.41	0.32
Summary (Sum)	0.42	0.39	0.15	0.44	0.69^{+}	0.41
Model R^2	.02		.01		.03	
Session 3						
Intercept	4.99	0.25	4.31	0.29	5.19	0.27
Cost Intervention (Cost)	-0.22	0.32	-0.17	0.36	-0.05	0.33
Utility Value Intervention (UV)	-0.05	0.32	-0.12	0.36	0.25	0.33
Summary (Sum)	0.20	0.38	0.32	0.44	0.39	0.40
Model R^2	.01		.01		.02	

Note: $*p \leq .05$; $** p \leq .01$; $^{+}p < .10$. All results show unstandardized slope and intercept values and are based on the survey control condition as a reference group. Standardized slope values for significant effects are reported in text. All intercept values are significant at $p < .01$.

Research Question 3. This research question addressed how the cost and utility value interventions impacted students' exam scores, quiz scores, and final course grades in physics. Mean scores by condition on these outcomes are reported in Table 10, and regression results are reported in Table 11. Again, contrary to my hypotheses, neither the cost nor the utility value intervention conditions impacted significantly students' course achievement outcomes compared to either control condition.

Table 10

Mean Scores by Condition on Outcomes Related to Course Achievement (Research Question 3)

	Condition											
	Cost Intervention			**Utility Value Intervention**			**Summary**			**Survey Control**		
	N	**M**	**SD**	**N**	**M**	**SD**	**N**	**M**	**SD**	**N**	**M**	**SD**
Average Quiz Scores	45	0.81	0.18	46	0.84	0.13	19	0.82	0.17	23	0.84	0.14
Average Exam Scores	45	0.84	0.11	45	0.87	0.09	19	0.85	0.16	23	0.84	0.15
Exam 2 Scores	45	0.87	0.08	46	0.90	0.08	19	0.84	0.17	23	0.87	0.14
Exam 3 Scores	45	0.81	0.16	45	0.85	0.12	19	0.85	0.19	23	0.81	0.18
Final Course Grades	46	86.46	10.47	46	88.29	8.45	19	87.08	14.52	23	87.64	12.58

Research Question 4. This research question addressed how the intervention conditions impacted students' amount of time spent completing homework assignments and attendance rates in discussion sections. Mean scores by condition on these constructs are found in Table 12, and regression analyses for these outcomes are reported in Table 13. There was one significant effect: Students in the cost intervention condition spent significantly longer on homework than did students in the survey control condition, β = 0.24, $t(135) = 2.06$, $p = .04$. This difference was not significant when comparing the cost intervention to the summary condition. The utility value intervention condition did not impact significantly students' course participation compared to either control condition.

Table 11

Regression Results on Outcomes Related to Course Achievement (Research Question 3)

	Average Quiz Scores		**Average Exam Scores**		**Exam 2 Scores**		**Exam 3 Scores**		**Final Course Grades**	
	B	S.E.	B	S.E.	B	S.E.	B	SE	B	S.E.
Intercept	0.84	0.03	0.84	0.03	0.87	0.02	0.81	0.03	87.64	2.27
Cost Intervention (Cost)	-0.02	0.04	0.00	0.03	0.00	0.03	0.00	0.04	-1.18	2.78
Utility Value Intervention (UV)	0.01	0.04	0.03	0.03	0.03	0.03	0.04	0.04	0.65	2.78
Summary (Sum)	-0.02	0.05	0.01	0.04	-0.03	0.03	0.04	0.05	-0.56	3.38
Model R^2	.01		.02		.03		.02		.01	

Note: $^*p \leq .05$; $^{**} p \leq .01$; $^+p < .10$. All results show unstandardized slope and intercept values and are based on the survey control condition as a reference group. Standardized slope values for significant effects are reported in text. All intercept values are significant at $p < .01$.

Research Question 5. This research question addressed how the intervention conditions impacted students' likelihood of enrolling in the next course in the physics sequence and the number of STEM courses in which students enrolled during the semester following the intervention. Mean scores by condition on the number of STEM courses students took are reported in Table 12, and regression and logistic regression results for both outcomes are reported in Table 13. Contrary to my hypotheses, neither of the intervention conditions impacted significantly students' course-taking patterns compared to either control condition.

Table 12

Mean Scores by Condition on Outcomes Related to Course Participation and Course-Taking Patterns (Research Questions 4 and 5)

	Condition											
	Cost Intervention			**Utility Value Intervention**			**Summary**			**Survey Control**		
	N	**M**	**SD**	**N**	**M**	**SD**	**N**	**M**	**SD**	**N**	**M**	**SD**
Av. Homework Time	46	150.48	67.72	48	133.69	45.62	20	128.74	33.92	25	123.11	50.36
Discussion Section Attendance	44	2.77	1.12	46	2.91	1.13	19	2.63	1.17	25	2.84	1.03
Enrollment in STEM Courses	45	3.02	1.16	48	3.25	0.98	20	2.80	0.95	24	2.96	1.23

Table 13

Regression Results on Outcomes Related to Course Participation and Course-Taking Patterns (Research Questions 4 and 5)

	Average Homework Time		**Discussion Section Attendance**		**Enrollment in Next Physics Course**		**Enrollment in STEM Courses**	
	B	**S.E.**	**B**	**S.E.**	**Log-Odds**	**S.E.**	**B.**	**S.E.**
Intercept	123.11	10.71	2.84	0.22	-0.09	0.42	2.96	0.22
Cost Intervention (Cost)	27.37*	13.30	-0.07	0.28	0.31	0.51	0.06	0.27
Utility Value Intervention (UV)	10.57	13.20	0.07	0.28	0.25	0.51	0.29	0.27
Summary (Sum)	5.63	16.06	-0.21	0.34	0.49	0.62	-0.16	0.33
Model R^2/ χ^2	R^2 = .04		R^2 = .01		χ^2 = 0.68		R^2 = .02	

Note: *$p \leq .05$; ** $p \leq .01$; $^+p < .10$. All results show unstandardized slope and intercept values and are based on the survey control condition as a reference group. Standardized slope values for significant effects are reported in text. All intercept values are significant at $p < .01$.

Research Question 6. This research question addressed whether the effects of the conditions on any outcomes were moderated by any of the following variables: baseline competence-related beliefs, prior achievement, belonging uncertainty, beliefs about the malleability of intelligence, gender, or ethnicity. I tested for moderation effects by examining interaction terms in regression models, one moderating variable at a time.

Before beginning data analysis, I noticed that fewer African American students enrolled in Physics 161 in Fall, 2016 than I had estimated would enroll based on prior semesters (in this study, n = 12). Only one African American student received each control condition. My theoretical rationale for testing ethnicity as a moderator was due to the possibility that I would find differential effects for African American students versus European American students as a result of reducing these students' belonging uncertainty (Walton & Cohen, 2007; 2011). I could not test this possibility with such a small sample, so I chose not to conduct the planned analyses regarding ethnicity as a moderating variable. I report results for the other five moderators below.

Mean scores by condition for the four continuous moderating variables are reported in Table 14. I tested whether the conditions differed at baseline on any moderating variables using regression with three dummy-coded terms representing each condition compared to a reference group (in different analyses, the summary or survey control condition served as a reference group). There was only one significant difference: Students in the cost intervention condition had lower prior achievement than did students in the summary condition, $\beta = -0.29$, $t(129) = -2.26$, $p = .03$. The breakdown of gender by condition was: 29.8% female in the cost intervention condition, 28.8% female in the utility value intervention condition, 26.1% female in the summary condition, and 24.0%

female in the survey control condition. I confirmed that this gender breakdown was similar across conditions using crosstabs analyses.

Table 14

Mean Scores by Condition on Continuous Moderating Variables

	Condition											
	Cost Intervention			**Utility Value Intervention**			**Summary**			**Survey Control**		
	N	**M**	**SD**	**N**	**M**	**SD**	**N**	**M**	**SD**	**N**	**M**	**SD**
Baseline Competence-Related Beliefs	48	4.57	1.30	52	4.80	1.26	23	5.12	1.42	24	4.79	1.14
Prior Achievement (Exam 1 Scores)	45	0.74	0.21	46	0.80	0.19	19	0.86	0.16	23	0.80	0.21
Belonging Uncertainty	48	2.98	1.24	52	3.26	1.62	23	3.35	1.37	25	3.06	1.11
Beliefs about Intelligence	48	2.77	1.17	52	3.11	1.28	23	3.07	1.25	24	2.57	1.03

In the three sections that follow, I discuss the results of moderation analyses for the cost intervention, the utility value intervention, and the summary condition. Tables 15 (cost intervention versus survey control), 16 (utility value intervention versus survey control), and 17 (summary versus survey control) provide a visual depiction of the moderation analyses I conducted for the major variables in this study. The tables depict which of the simple effects for students at different levels of the moderating variables were fully, marginally, or not significant. For clarity, they also note whether those effects were positive for students (noted as a "P") or undermining (noted as a "U"). The tables do not simply report whether outcomes were higher or lower, because higher cost actually is an undermining effect. I omitted from these tables attainment value, intrinsic value, enrollment in the next physics course, discussion section attendance, and homework time, because I did not find consistent patterns of moderation on any of these outcomes for either intervention. Appendix F provides complete unstandardized regression output for all moderation analyses conducted, using the survey control condition as a reference group. I report in text the standardized regression coefficients or, in the case of enrollment in the next physics course, odds ratios, for any significant interaction terms. I also report in the sections below graphs for some moderation effects. Rather than reporting a graph for every significant interaction, I report graphs that represented the most clear and consistent patterns of results I observed in the data.

Moderation effects in the cost intervention condition. Table 15 depicts all the moderation analysis effects for the major outcome variables explored in the cost intervention, and Appendix F shows the complete output for all regression analyses conducted. I discuss each moderating variable in turn. I considered moderators in the cost intervention condition to be exploratory, because the intervention was new and I was unsure how it would impact particular students.

Table 15

Moderation Effects for Major Outcomes: Cost Intervention versus Survey Control Condition

	Moderator									
	Baseline Competence -Related Beliefs		**Prior Achievement**		**Belonging Uncertainty**		**Beliefs about Intelligence**		**Gender**	
	Low	High	Low	High	Low	High	Mal	Fix	Male	Fem
Session 1										
Utility Value	-	-	-	-	U	-	-	-	-	-
Effort Cost	-	-	-	-	-	-	-	U	-	-
Outside Effort Cost	-	-	-	-	-	-	-	U	-	-
Emotional Cost	-	U	-	*U*	-	-	-	U	-	-
Total Cost	-	-	-	-	-	-	-	U	-	-
Competence-Related Beliefs	-	-	-	-	-	-	P	U	*U*	-
Session 2										
Utility Value	-	-	-	-	-	-	-	-	-	-
Effort Cost	-	-	P	-	-	-	-	U	-	-
Outside Effort Cost	-	-	-	-	-	-	*P*	U	-	-
Emotional Cost	-	-	P	-	-	-	P	U	-	-
Total Cost	-	-	P	-	-	-	*P*	U	-	-
Competence-Related Beliefs	-	-	-	-	-	-	P	U	-	P
Session 3										
Utility Value	*P*	U	-	-	-	-	-	-	-	-
Effort Cost	-	-	-	-	-	-	-	U	-	-
Outside Effort Cost	-	-	-	-	U	-	-	-	-	-
Emotional Cost	P	-	P	-	-	-	*P*	*U*	-	P
Total Cost	-	*U*	-	-	U	-	-	U	-	*P*
Competence-Related Beliefs	-	-	-	-	-	-	-	U	-	-
Course Outcomes										

Average Quiz Score	-	-	-	-	-	-	-	U	-	-
Exam 2 Score	P	*U*	P	*U*	*U*	-	-	-	-	P
Exam 3 Score	-	-	-	-	-	-	-	-	-	*P*
Average Exam Score	-	-	P	-	*U*	*P*	-	-	-	P
Final Course Grade	-	-	-	-	U	*P*	-	U	-	*P*
Enrollment in Future STEM Courses	-	-	-	-	-	-	-	-	-	P

Note: P = Positive impact on outcome (i.e., decreases to cost, increases to competence-related beliefs, utility value, or course outcomes); U = Undermining impact on outcome (i.e., increases to cost, decreases to competence-related beliefs, utility value, or course outcomes). All non-italicized effects are significant at $p < .05$; italicized effects are marginally significant at $p < .10$. Effects shown are simple effects for students at different levels of the moderating variables.

Moderation effects for the cost intervention: baseline competence-related beliefs. There were some interactions suggesting that the impact of the cost intervention versus the survey control condition depended on students' baseline competence-related beliefs. For the outcome of utility value at Session 3, there was a significant interaction between receiving the cost intervention versus the survey control condition and students' baseline competence beliefs, $\beta = -0.40$, $t(123) = -2.49$, $p = .01$. This effect did not occur at Sessions 1 or 2. Follow-up analyses revealed that students with low baseline competence-related beliefs (i.e., one standard deviation below the mean) showed a marginally significant positive trend suggesting that they reported higher utility value in the cost intervention versus the survey control condition. However, students with high baseline competence-related beliefs (i.e., one standard deviation above the mean) showed an undermining effect, reporting significantly lower utility value if they received the cost intervention versus the survey control condition.

There were also interactions between receiving the cost intervention versus the survey control condition and students' baseline competence-related beliefs on emotional cost at Session 1, $\beta = 0.26$, $t(139) = 2.14$, $p = .03$, and Session 3, $\beta = 0.33$, $t(123) = 2.48$, $p = .02$. There was a similar interaction on total cost at Session 3, $\beta = 0.28$, $t(123) = 1.99$, $p = .05$; this effect was marginally significant at Session 1, $\beta = 0.24$, $t(133) = 1.91$, $p = .06$. Follow-up analyses for significant interactions revealed one positive effect: Students with low baseline competence-related beliefs reported significantly lower emotional cost at Session 3 in the cost intervention versus the survey control condition. These students also appeared to report slightly lower emotional cost at Session 1 and total cost at Session 3 if they received the cost intervention versus the survey control condition, but these differences were not statistically significant. However, there were also undermining effects: Students with high baseline competence-related beliefs reported significantly higher emotional cost at Session 1, and showed a marginally significant trend suggesting they reported higher total cost at Session 3, if they received the cost intervention versus the survey control condition.

For non-focal motivational constructs, there were interactions between receiving the cost intervention versus the survey control condition on intrinsic value at Session 2, $\beta = -0.35$, $t(118) = -2.36$, $p = .02$, and Session 3, $\beta = -0.41$, $t(123) = -2.64$, $p = .01$; this effect was also marginally significant at Session 1, $\beta = -0.25$, $t(139) = -1.89$, $p = .06$. A similar interaction was found for attainment value at Session 3, $\beta = -0.42$, $t(123) = -2.49$, $p = .01$. Follow-up analyses for significant effects revealed that students with low baseline competence-related beliefs showed marginally significant trends suggesting that they reported higher attainment and intrinsic value at these time points if they received the cost intervention versus the survey control condition. However, there was also an undermining effect such that students with high baseline competence-related beliefs reported significantly lower intrinsic value at Session 3, and showed a marginally significant trend suggesting that they reported lower attainment value at Session 3, if they received the cost intervention. These students also appeared to report slightly lower intrinsic value at Session 2 if they received the cost intervention versus the survey control condition, but this difference was not statistically significant.

In terms of course outcomes, on exam 2 scores there was an interaction between receiving the cost intervention versus the survey control condition and students' baseline competence-related beliefs, β = -0.40, $t(124)$ = -2.64, p = .01. This effect was not significant for exam 3 scores or quiz scores but it was marginally significant for average exam scores, β = -0.26, $t(123)$ = -1.73, p = .09. Follow-up analyses for the significant interaction revealed a positive effect such that students with low baseline competence-related beliefs earned higher exam 2 scores if they received the cost intervention compared to the survey control condition. However, students with high baseline competence-related beliefs showed a marginally significant trend suggesting that they reported lower exam 2 scores in the cost intervention condition versus the survey control condition. There were no interactions between cost and baseline competence-related beliefs on homework time, discussion section attendance, or course-taking patters.

The moderation effects just noted did not hold when using the summary condition as a reference group. There were no significant interactions between receiving the cost intervention versus the summary condition and students' baseline competence-related beliefs on any motivational variables at any session, or on students' course outcomes.

Moderation effects for the cost intervention: prior achievement. There were also interactions suggesting that the effects of the cost intervention versus the survey control condition depended on students' prior course achievement. There were no interactions between receiving the cost intervention versus the survey control condition and prior achievement on utility value. For cost, there was an interaction between receiving the cost intervention versus the survey control condition and prior achievement on emotional cost at Session 1, $\beta = 0.27$, $t(125) = 2.13$, $p = .04$, Session 2, $\beta = 0.32$, $t(107) = 2.19$, $p = .03$, and Session 3, $\beta = 0.28$, $t(113) = 2.17$, $p = .03$, effort cost at Session 2, $\beta = 0.32$, $t(107) = 2.09$, $p = .04$, and total cost at Session 2, $\beta = 0.29$, $t(107) = 1.96$, $p = .05$ (see Figure 2). Follow-up analyses revealed positive effects such that students who had low prior achievement (i.e., one standard deviation below the mean) reported significantly lower effort and total cost at Session 2 and emotional cost at Sessions 2 and 3 in the cost intervention versus the survey control condition. These students also reported slightly lower emotional cost at Session 1, but this difference was not statistically significant. Conversely, students who had high prior achievement (i.e., one standard deviation above the mean) showed a marginally significant undermining effect suggesting that they reported higher emotional cost at Session 1 in the cost intervention versus the survey control condition. For the other interactions, these students reported slightly higher cost if they received the cost intervention versus the survey control condition, but differences were not statistically significant.

In terms of non-focal motivational constructs, there was an interaction between the cost intervention and prior achievement on attainment value at Session 3, $\beta = -0.36$, $t(113) = -2.41$, $p = .02$. Follow-up analyses revealed that students with low prior achievement reported slightly higher attainment value if they received the cost intervention versus the survey control condition, but this difference was not statistically significant. Conversely, students with high prior achievement showed a marginally significant undermining effect suggesting that they reported lower attainment value if they received the cost intervention versus the survey control condition.

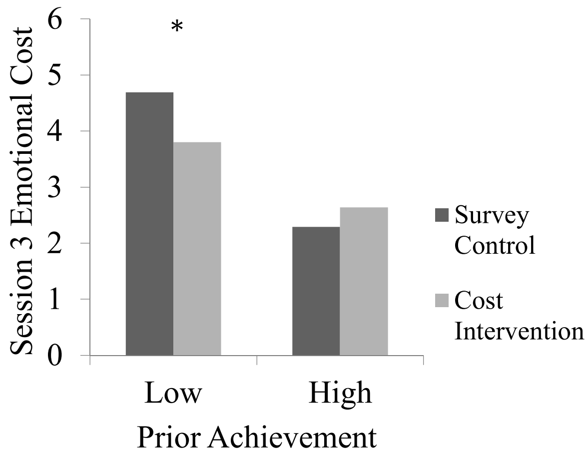

Figure 2. Impact of the cost intervention on Session 3 emotional cost, as a function of students' prior achievement. High = +1SD from the mean; low = -1SD from the mean. $*p < .05$.

In terms of course outcomes, there were interactions between receiving the cost intervention versus the survey control condition and prior achievement on exam 2 scores, $\beta = -0.39$, $t(125) = -3.13$, $p = .002$, and average exam scores, $\beta = -0.27$, $t(124) = -2.47$, $p = .02$ (see Figure 3). This effect was marginally significant for course grades, $\beta = -0.15$, $t(125) = -1.78$, $p = .08$, but did not hold for exam 3 scores or quiz scores. Follow-up analyses for significant interactions revealed some positive effects: Students with low prior achievement earned significantly higher exam 2 scores and average exam scores if they received the cost intervention versus the survey control condition. Students with high prior achievement showed a marginally significant undermining effect suggesting that they earned lower exam 2 scores in the cost intervention versus the survey control condition, but they did not differ by condition on average exam scores. There were no effects on homework time, discussion section attendance, or course-taking patterns.

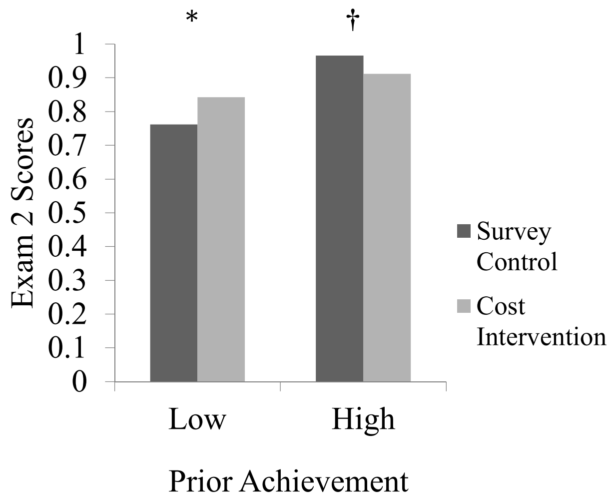

Figure 3. Impact of the cost intervention on exam 2 scores, as a function of students' prior achievement. High = +1SD from the mean; low = -1SD from the mean. *$p < .05$; † $p < .10$.

Again, effects looked different when comparing the cost intervention to the summary condition. At Session 2, there was an interaction effect on utility value at significance, $\beta = -0.47$, $t(107) = -1.97$, $p = .05$. This effect was marginally significant in Session 3, $\beta = -0.42$, $t(113) = -1.81$, $p = .07$, but was not significant in Session 1. Follow-up analyses for the significant effect revealed that students with low prior achievement reported slightly higher utility value at Session 2, and students with high prior achievement reported slightly lower utility value at Session 2, if they received the cost intervention condition versus the summary condition. However, these differences were not statistically significant. There were also interactions on the outcome of competence-related beliefs at Session 1, $\beta = -0.31$, $t(125) = -2.02$, $p = .05$, Session 2, $\beta = -0.50$, $t(107)$

= -2.68, p = .01, and Session 3, β = -0.41, $t(113)$ = -2.18, p = .03. Students with low prior achievement showed marginally significant or fully significant positive effects suggesting that they reported higher competence-related beliefs at Sessions 2 and 3 if they received the cost intervention versus the summary condition. However, students with high prior achievement showed a marginally significant undermining effect suggesting they reported lower competence-related beliefs at Session 2 if they received the cost intervention versus the summary condition. They also reported slightly lower competence-related beliefs at Sessions 1 and 3, but differences between the cost intervention and summary condition were not statistically significant. Finally, there were interactions between receiving the cost intervention versus the summary condition on course grades, β = -0.39, $t(125)$ = -3.66, p < .001, exam 2 scores, β = -0.47, $t(125)$ = -2.92, p = .004, and average exam scores, β = -0.35, $t(124)$ = -2.48, p = .01. Follow up analyses suggested that students with low prior achievement earned significantly higher scores on all outcomes in the cost intervention versus the summary condition. Students with high prior achievement did not show differences between the conditions.

Moderation effects for the cost intervention: belonging uncertainty. There were a few interactions suggesting that the effects of the cost intervention versus the survey control condition depended on students' belonging uncertainty. For the outcome of utility value, there was a significant interaction at Session 1, β = 0.32, $t(140)$ = 2.06, p = .04. This effect was marginally significant in Session 3, β = .27, $t(124)$ = 1.77, p = .08, but did not occur in Session 2. Follow-up analyses for the significant interaction revealed an undermining effect such that students with low belonging uncertainty (i.e., one standard deviation below the mean) reported significantly lower utility value at Session 1 if they received the cost intervention versus the survey control condition. Students with high belonging uncertainty (i.e., one standard deviation above the mean) reported slightly higher utility value in the cost intervention condition versus the survey control condition, but this difference was not statistically significant.

For cost, there were interactions between receiving the cost intervention versus the survey control condition and belonging uncertainty on outside effort cost, $\beta = -0.40$, $t(124) = -2.70$, $p = .01$, and total cost, $\beta = -0.29$, $t(124) = -2.01$, $p = .05$, at Session 3. Follow-up analyses revealed undermining effects such that students with low belonging uncertainty reported higher outside effort cost and total cost if they received the cost intervention versus the survey control condition. Descriptively, students with high belonging uncertainty reported slightly lower cost in both analyses if they received the cost intervention versus the survey control condition, but these differences were not statistically significant.

There was also an interaction between belonging uncertainty and receiving the cost intervention versus the survey control condition on attainment value at Session 3, $\beta = 0.41$, $t(124) = 2.64$, $p = .01$. Follow-up analyses revealed an undermining effect such that students with low belonging uncertainty reported significantly lower attainment value if they received the cost intervention compared to the survey control condition. However, students with high belonging uncertainty showed a marginally significant positive trend towards reporting higher attainment value in the cost intervention versus the survey control condition.

For course outcomes, there were interactions between receiving the cost intervention versus the survey control condition and belonging uncertainty on course grades, $\beta = 0.38$, $t(126) = 2.58$, $p = .01$, exam 2 scores, $\beta = 0.31$, $t(125) = 2.06$, $p = .04$, and average exam scores, $\beta = 0.33$, $t(124) = 2.26$, $p = .03$. There was a marginally significant interaction on exam 3 scores, $\beta = 0.29$, $t(124) = 1.92$, $p = .06$, but there was no effect on quiz scores. Follow-up analyses for significant interactions showed undermining effects such that students with low belonging uncertainty earned significantly lower course grades, and showed marginally significant trends suggesting that they earned lower exam 2 scores and average exam scores, in the cost intervention versus the survey control condition. Descriptively, students with high belonging uncertainty seemed to earn slightly higher course grades, average exam scores, and exam 2 scores if they received the cost intervention versus the survey control condition. However, these differences were not statistically significant for exam 2 scores and were marginally significant for the other two outcomes. There were no effects on homework time, discussion section attendance, or course-taking patterns.

Like with competence-related beliefs, effects did not hold when using the summary condition as a reference group. There were no interactions between receiving the cost intervention versus the summary condition and students' belonging uncertainty on motivation. There was a significant interaction between receiving the cost intervention versus the summary condition and belonging uncertainty on the likelihood of students enrolling in the next physics course during the semester following the intervention, exp(B) = 0.25, Wald = 4.32, $p = .04$; there was also a marginally significant interaction on the number of STEM courses in which students enrolled in the semester following the intervention, $\beta = -0.57$, $t(129) = -1.84$, $p = .07$. Follow-up analyses of the significant interaction revealed that students with low belonging uncertainty appeared to be more likely to take the course if they received the cost intervention versus the summary condition, but students with high belonging uncertainty appeared to be less likely to take the course if they received the cost intervention.

Moderation effects for the cost intervention: beliefs about the malleability of intelligence. There were many interactions suggesting that the effects of the cost intervention versus the survey control condition were moderated by students' beliefs about the malleability of intelligence. There were no interaction effects on utility value. For cost, there were interactions on effort cost at Session 1, $\beta = 0.39$, $t(136) = 2.48$, $p = .02$, Session 2, $\beta = 0.46$, $t(118) = 2.89$, $p = .01$, and Session 3, $\beta = 0.38$, $t(123) = 2.14$, $p = .04$, emotional cost at Session 1, $\beta = 0.45$, $t(139) = 2.90$, $p = .004$, Session 2, $\beta = 0.51$, $t(118) = 3.24$, $p = .002$, and Session 3, $\beta = 0.42$, $t(123) = 2.30$, $p = .02$, outside effort cost at Session 1, $\beta = 0.31$, $t(136) = 1.99$, $p = .05$, and Session 2, $\beta = 0.50$, $t(118) = 3.18$, $p = .002$, and total cost at Session 1, $\beta = 0.41$, $t(133) = 2.55$, $p = .01$, Session 2, $\beta = 0.52$, $t(118) = 3.29$, $p = .001$, and Session 3, $\beta = 0.42$, $t(123) = 2.32$, $p = .02$ (see Figure 4). The interaction with outside effort cost at Session 3 was marginally significant, $\beta = 0.35$, $t(123) = 1.91$, $p = .06$.

Follow up analyses for significant interactions revealed that if students began the intervention with strong malleable beliefs about intelligence (i.e., their score was one standard deviation below the mean on the beliefs about intelligence construct), they showed significant or marginally significant positive effects suggesting that they reported lower outside effort cost, emotional cost, and total cost at Session 2, and emotional cost at Session 3. In the other analyses, these students also reported slightly lower cost after receiving the cost intervention; however, simple effects were not statistically significant.

Conversely, students who began the intervention with a stronger belief that intelligence was fixed (i.e., their score was one standard deviation above the mean on the beliefs about intelligence construct) showed significant or marginally significant undermining effects for all analyses suggesting that they reported higher levels of cost if they received the cost intervention versus the survey control condition.

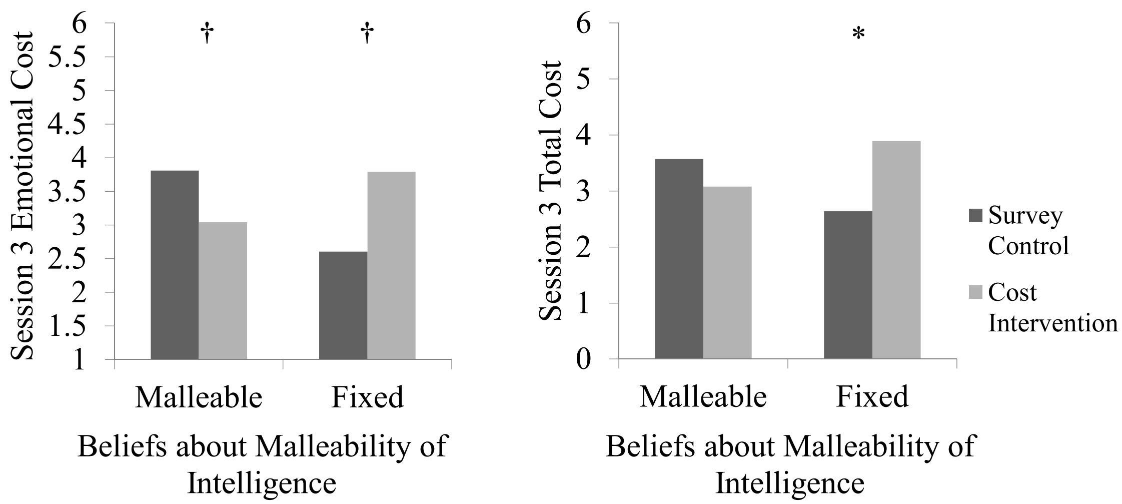

Figure 4. Impact of the cost intervention on emotional and total cost at Session 3, as a function of students' beliefs about the malleability of intelligence. Malleable = -1SD from the mean; fixed = +1SD from the mean. $^*p < .05$; † $p < .10$.

There were also interactions between receiving the cost intervention versus the survey control condition and beliefs about the malleability of intelligence on the outcomes of competence-related beliefs at Session 1, $\beta = -0.59$, $t(139) = -3.90$, $p < .001$, Session 2, $\beta = -0.49$, $t(118) = -3.13$, $p = .002$, and Session 3, $\beta = -0.37$, $t(123) = -2.07$, $p = .04$, intrinsic value at Session 1, $\beta = -0.33$, $t(139) = -2.11$, $p = .04$, and attainment value at Session 1, $\beta = -0.32$, $t(139) = -2.03$, $p = .05$. Follow-up analyses showed a positive effect such that students with strong malleable beliefs about intelligence reported significantly higher competence-related beliefs at Sessions 1 and 2 if they received the cost intervention versus the survey control condition. These students also reported slightly higher competence-related beliefs at Session 3, and intrinsic and attainment value at Session 1, if they received the cost intervention. However, those simple effects were not statistically significant. Conversely, students with strong fixed beliefs about intelligence showed undermining effects such that they reported significantly lower competence-related beliefs at all three sessions and lower attainment value at Session 1 if they received the cost intervention versus the survey control condition. They also reported slightly lower intrinsic value if they received the cost intervention versus the survey control condition, but this difference was not statistically significant.

For course outcomes, there were significant interactions between receiving the cost intervention versus the survey control condition and beliefs about the malleability of intelligence on course grades, $\beta = -0.37$, $t(125) = -2.26$, $p = .03$ and average quiz scores, $\beta = -0.33$, $t(124) = -2.01$, $p = .05$ (see Figure 5). The effect on exam 3 scores was marginally significant, $\beta = -0.29$, $t(123) = -1.74$, $p = .09$, but there were no effects on exam 2 scores or average exam scores. Follow-up analyses for significant effects showed that students who began the intervention with strong malleable beliefs about intelligence earned slightly higher levels of these outcomes if they received the cost intervention versus the survey control condition, but these differences were not statistically significant. Students who began the intervention with strong malleable beliefs about intelligence showed undermining effects such that they earned significantly lower course grades and quiz scores if they received the cost intervention versus the survey control condition. There were no interactions for homework time, discussion section attendance, or course-taking patterns.

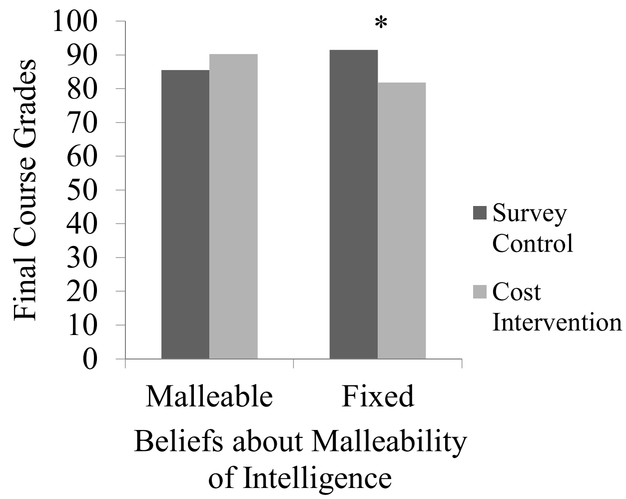

Figure 5. Impact of the cost intervention on final course grades, as a function of students' beliefs about the malleability of intelligence. Malleable = -1SD from the mean; fixed = +1SD from the mean. *$p < .05$.

Like with the other moderators, these effects did not all hold when comparing the cost intervention condition to the summary condition. There were significant interactions on effort cost at Session 1, $\beta = 0.32$, $t(136) = 2.23$, $p = .03$, and Session 2, $\beta = 0.48$, $t(118) = 2.82$, $p = .01$, and total cost at Session 2, $\beta = 0.37$, $t(118) = 2.15$, $p = .03$. Follow up analyses for these interactions revealed that students with strong malleable beliefs about intelligence reported slightly lower levels of cost in all analyses if they received the cost intervention condition versus the summary condition, but these differences were not statistically significant. Conversely, students with strong fixed beliefs about intelligence showed significant or marginally significant undermining effects such that they reported higher effort and total cost if they received the cost intervention versus the summary condition. There was also a significant interaction on attainment value at Session 3, $\beta = 0.33$, $t(123) = 1.99$, $p = .05$. This interaction differed from prior effects: Follow-up analyses revealed that students who began the intervention with strong malleable beliefs about intelligence showed an undermining effect, reporting *lower* attainment value after receiving the cost intervention compared to the summary condition. Students who began the intervention with strong fixed beliefs about intelligence reported slightly higher attainment value after receiving the cost intervention versus the summary condition, but this difference was not statistically significant.

Moderation effects for the cost intervention: gender. There were some interactions suggesting that the effects of the cost intervention versus the survey control condition depended on students' gender. There were no significant interaction effects of the cost intervention versus the survey control condition and gender on utility value. For cost, there were interactions at Session 3 on emotional cost, $\beta = -0.50$, $t(123) = -2.98$, $p = .003$, and total cost, $\beta = -0.37$, $t(123) = -2.20$, $p = .03$. Follow-up analyses revealed positive effects such that female students reported significantly lower emotional cost and showed a marginally significant trend towards reporting lower total cost in the cost intervention versus the survey control condition. Male students reported slightly higher cost in these interactions if they received the cost intervention versus the survey control condition, but differences were not statistically significant.

In terms of non-focal motivational constructs, there were interactions between receiving the cost intervention versus the survey control condition and gender on the outcome of competence-related beliefs at Session 1, $\beta = .32$, $t(140) = 1.95$, $p = .05$, Session 2, $\beta = 0.48$, $t(119) = 2.68$, $p = .01$, and Session 3, $\beta = .34$, $t(123) = 2.02$, $p = .05$ (see Figure 6). Follow-up analyses revealed positive effects such that female students reported higher competence-related beliefs at all three sessions if they received the cost intervention versus the survey control condition, whereas male students showed undermining effects suggesting that they reported lower competence-related beliefs. At

Session 1, these differences were not significant for females and were marginally significant for males. At Session 2, differences were significant for females but not significant for males. At Session 3, differences were not significant for either gender.

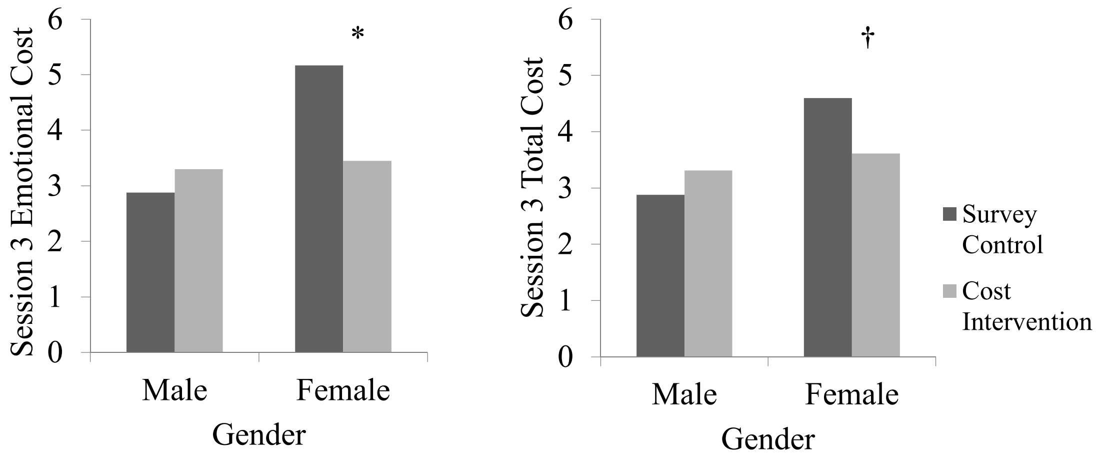

Figure 6. Impact of the cost intervention on emotional and total cost at Session 3, as a function of students' gender. *$p < .05$; † $p < .10$.

For course outcomes, there were interactions between receiving the cost intervention versus the survey control condition and gender on course grades, $\beta = 0.39$, $t(125) = 2.24$, $p = .03$, exam 2 scores, $\beta = 0.46$, $t(124) = 2.76$, $p = .01$, exam 3 scores, $\beta = 0.35$, $t(123) = 2.01$, $p = .05$, and average exam scores, $\beta = 0.44$, $t(123) = 2.60$, $p = .01$, and the number of STEM courses in which students enrolled in the semester following the intervention, $\beta = 0.36$, $t(128) = 2.13$, $p = .04$ (see Figure 7). Follow-up analyses revealed positive effects such that female students showed significantly or marginally significantly higher levels of all outcomes if they received the cost intervention versus the survey control condition. Male students earned slightly lower levels of these outcomes if they received the cost intervention versus the survey control condition, but no differences were statistically significant. There were no interactions on quiz scores, time spent on homework, discussion section attendance, or enrollment in the next physics course in the sequence.

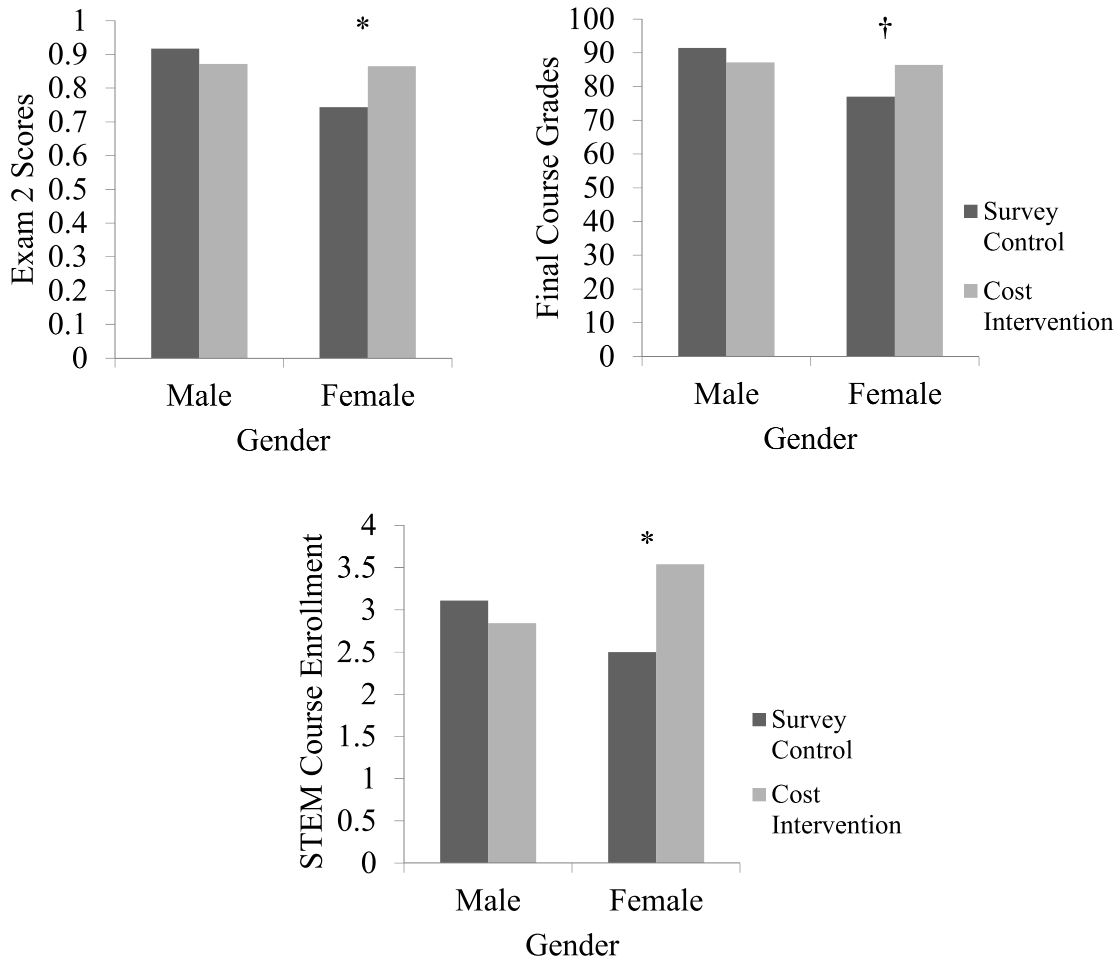

Figure 7. Impact of the cost intervention on students' exam 2 scores, final course grades, and STEM course enrollment during the semester following the intervention, as a function of students' gender. *$p < .05$; † $p < .10$

Again, few of these interactions held when comparing the cost intervention to the summary condition as a reference group. There was only one significant interaction, on competence-related beliefs at Session 2, $\beta = 0.38$, $t(118) = 2.15$, $p = .03$. Follow-up analyses suggested that female students reported slightly higher competence-related beliefs if they received the cost intervention versus the summary condition, but this difference was not statistically significant. Male students showed an undermining effect, reporting significantly lower competence-related beliefs in the cost intervention versus the summary condition.

Overview of moderation in the cost intervention condition. Compared to the survey control condition, receiving the cost intervention caused students with low baseline competence-related beliefs, low prior achievement, strong malleable intelligence beliefs, and who were female to report lower cost. Female students and students with strong malleable beliefs about intelligence also reported higher competence-related beliefs. Students with low baseline competence-related beliefs, low prior achievement, and female students earned higher course grades and exam scores after receiving the cost intervention, and female students took more STEM courses in the semester following the intervention. These effects were consistent with my hypotheses that the cost intervention would impact students positively and would reduce their perceptions of cost.

Contrary to my hypotheses, receiving the cost intervention versus the survey control condition caused some undermining effects: Students with strong fixed beliefs about intelligence or low belonging uncertainty in physics reported higher cost over time, students with low prior achievement and low baseline competence-related beliefs reported higher cost at Session 1, and students with strong fixed beliefs about intelligence reported lower competence-related beliefs. Students with strong fixed beliefs about intelligence and students with low belonging uncertainty in turn earned lower course grades and/or quiz scores if they received the cost intervention. Finally, there were marginally significant differences suggesting that students with low baseline competence-related beliefs and low prior achievement reported lower exam 2 scores after receiving the cost intervention versus the survey control condition.

Moderation effects in the utility value intervention condition. Table 16 depicts which effects were significant for the major outcome variables explored in the utility value intervention, and Appendix F shows the complete output for all regression analyses conducted. I discuss each moderating variable in turn, starting with the two theoretical moderating variables that were the most likely to moderate results: baseline competence-related beliefs and prior achievement. I then turn to discuss the three exploratory moderating variables: belonging uncertainty, beliefs about the malleability of intelligence, and gender.

Theoretical moderation effects in the utility value intervention: baseline competence-related beliefs. There were some interactions suggesting that the effects of the utility value intervention versus the survey control condition depended on students' baseline competence-related beliefs. There were no interactions on utility value at any time point. In Session 3, there were interactions on emotional cost, $\beta = 0.44$, $t(123) = 3.27$, $p = .001$, and total cost, $\beta = 0.37$, $t(123) = 2.57$, $p = .01$ (see Figure 8). Follow-up analyses showed positive effects such that students with low baseline competence-related beliefs reported lower emotional cost after receiving the utility value intervention versus the survey control condition. These students also reported slightly lower total cost if they received the utility value intervention condition, but the simple effect was not statistically significant. Conversely, students with high baseline competence-related beliefs showed undermining effects such that they reported significantly higher emotional and total cost in the utility value intervention versus the survey control condition.

Table 16

Moderation Effects for Major Outcomes: Utility Value Intervention versus Survey Control Condition

	Moderator									
	Theoretical				Exploratory					
	Baseline Competence-Related Beliefs		Prior Achievement		Belonging Uncertainty		Beliefs about Intelligence		Gender	
Summary	Low	High	Low	High	Low	High	Mal	Fix	Male	Fem
Session 1										
Utility Value	-	-	-	-	-	-	-	-	-	-
Effort Cost	-	-	-	-	-	-	-	-	-	-
Outside Effort Cost	-	-	-	-	-	-	-	-	-	-
Emotional Cost	-	-	-	-	-	-	-	U	-	-
Total Cost	-	-	-	-	-	-	-	-	-	-
Competence-Related Beliefs	-	-	-	-	-	-	*P*	U	-	-
Session 2										
Utility Value	-	-	-	-	-	-	P	-	-	-
Effort Cost	-	-	-	-	-	-	-	-	-	-
Outside Effort Cost	-	-	-	-	-	-	-	U	-	-
Emotional Cost	-	-	-	U	-	-	-	U	-	-
Total Cost	-	-	-	*U*	-	-	-	U	-	-
Competence-Related Beliefs	-	-	-	-	-	-	P	-	-	P
Session 3										
Utility Value	-	-	-	-	-	-	-	-	-	-
Effort Cost	-	-	-	-	-	-	-	-	-	-
Outside Effort Cost	-	-	-	-	-	-	-	-	*U*	-
Emotional Cost	P	U	-	*U*	-	-	-	-	-	P
Total Cost	-	U	-	U	-	-	-	-	-	*P*
Competence-Related Beliefs	-	-	-	-	-	-	-	-	-	-
Course Outcomes										

Average Quiz Score	-	-	-	-	-	-	-	-	-	-
Exam 2 Scores	P	-	P	-	-	P	-	-	-	P
Exam 3 Scores	-	-	P	-	-	-	-	-	-	P
Average Exam Scores	P	-	P	-	-	P	-	-	-	P
Final Course Grades	*P*	-	P	-	-	P	-	-	-	*P*
Enrollment in Future STEM Courses	-	-	-	-	-	P	-	-	-	-

Note: P = Positive impact on outcome (i.e., decreases to cost, increases to competence-related beliefs, utility value, or course outcomes); U = Undermining impact on outcome (i.e., increases to cost, decreases to competence-related beliefs, utility value, or course outcomes. All non-italicized effects are significant at $p < .05$; italicized effects are marginally significant at $p < .10$. Effects shown are simple effects for students at different levels of the moderating variables.

For course outcomes, there were interactions between receiving the utility value intervention and baseline competence-related beliefs on final course grades, $\beta = -0.30$, $t(125) = -2.20$, $p = .03$, exam 2 scores, $\beta = -0.43$, $t(124) = -2.89$, $p = .01$, and average exam scores, $\beta = -0.35$, $t(123) = -2.33$, $p = .02$ (see Figure 9). Follow-up analyses revealed positive effects such that students with low baseline competence-related beliefs earned significantly higher exam 2 and average exam scores after receiving the utility value intervention versus the survey control condition. They also showed a marginally significant trend suggesting that they earned higher course grades. Students with high baseline competence-related beliefs appeared to earn slightly lower grades and exam scores in the utility value intervention versus the survey control condition, but no differences were statistically significant. There were no interactions on homework time, discussion section attendance, or course-taking patterns.

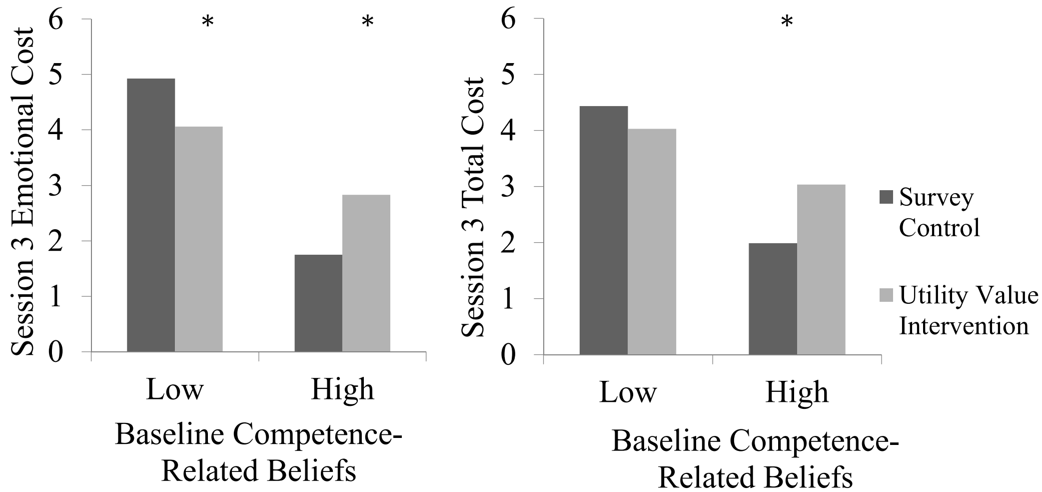

Figure 8. Impact of the utility value intervention on emotional and total cost at Session 3, as a function of students' competence-related beliefs. Low = -1SD from the mean; high = +1SD from the mean. *$p < .05$.

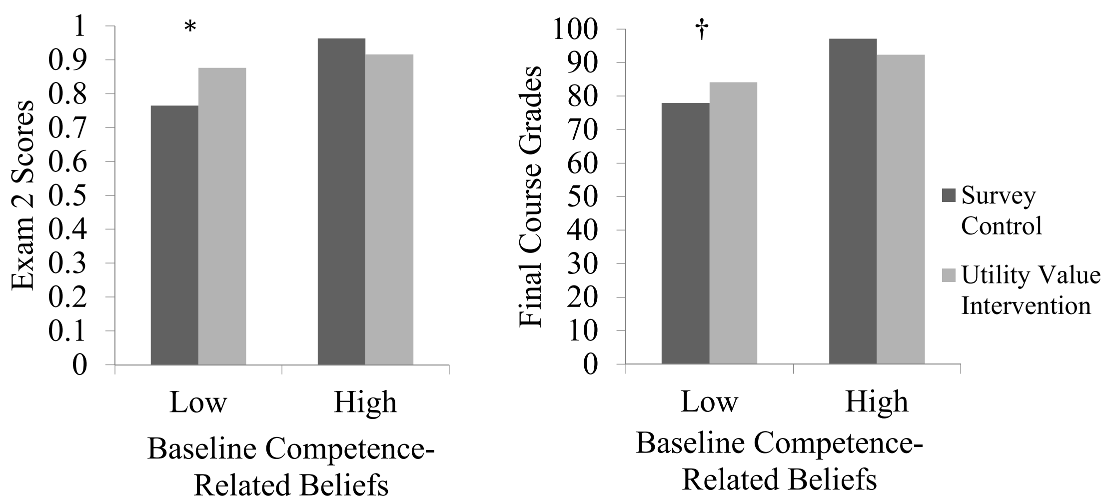

Figure 9. Impact of the utility value intervention on exam 2 scores and final course grades, as a function of students' competence-related beliefs. Low = -1SD from the mean; high = +1SD from the mean. *$p < .05$; † $p < .10$.

There was only one significant interaction when looking at interactions of competence-related beliefs with the utility value intervention compared to the summary condition. It was on course grades, $\beta = -0.26$, $t(125) = -2.01$, $p = .05$. Follow-up analyses showed that students with low baseline competence-related beliefs earned significantly higher course grades in the utility value intervention versus the summary condition. Students with high baseline competence-related beliefs did not differ by condition on course grades.

Theoretical moderation effects in the utility value intervention: prior achievement. There were some interactions between suggesting that the effects of the utility value intervention versus the survey control condition depended on students' prior achievement. For cost, there were interactions on emotional cost at Session 2, $\beta = 0.32$, $t(107) = 2.22$, $p = .03$, and Session 3, $\beta = 0.29$, $t(113) = 2.37$, $p = .02$, and total cost at Session 2, $\beta = 0.30$, $t(107) = 2.08$, $p = .04$, and Session 3, $\beta = 0.26$, $t(113) = 2.02$, $p = .05$ (see Figure 10). Follow up analyses revealed that students with low prior achievement reported slightly lower cost in all four interactions if they received the utility.value intervention versus the survey control condition, but no differences were statistically significant. Conversely, students with high prior achievement showed marginally significant or significant undermining effects suggesting that they reported higher emotional and total cost at Sessions 2 and 3 in the utility value intervention versus the survey control condition.

For course outcomes, there were interactions between receiving the utility value intervention versus the survey control condition and prior achievement on course grades, $\beta = -0.21$, $t(125) = -2.74$, $p = .01$, exam 2 scores, $\beta = -0.36$, $t(125) = -3.15$, $p = .002$, exam 3 scores, $\beta = -0.23$, $t(124) = -2.24$, $p = .03$, and average exam scores, $\beta = -0.32$, $t(124) = -3.24$, $p = .002$ (see Figure 11). Follow up analyses revealed positive effects such that students with low prior achievement earned significantly higher levels of all of the course outcomes after receiving the utility value intervention versus the survey control condition. Students with high prior achievement did not show significant differences between conditions on any outcomes, but they appeared to earn slightly lower exam 2 scores if they received the utility value intervention. There were no interactions on homework time, discussion section attendance, or course-taking patterns.

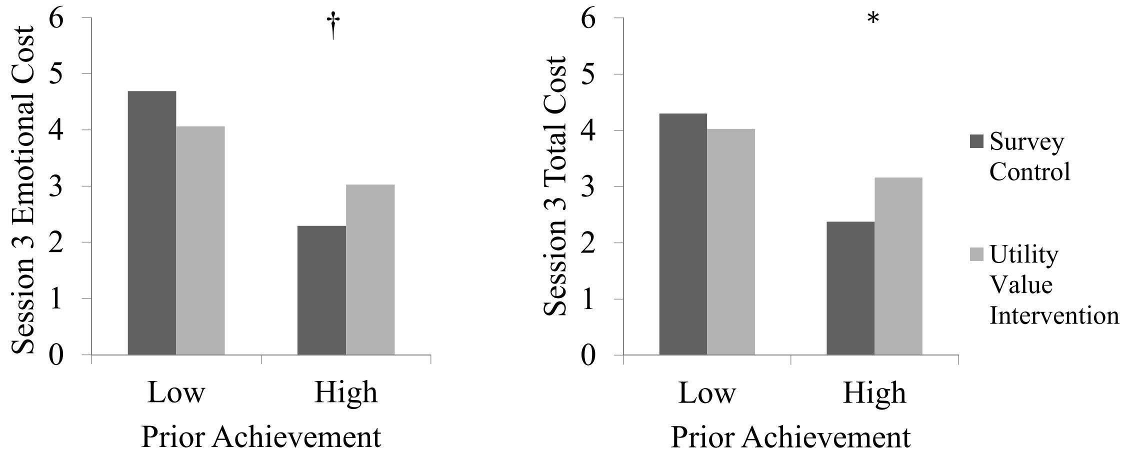

Figure 10. Impact of the utility value intervention on emotional and total cost at Session 3, as a function of students' prior achievement. Low = -1SD from the mean; high = +1SD from the mean. *$p < .05$; † $p < .10$.

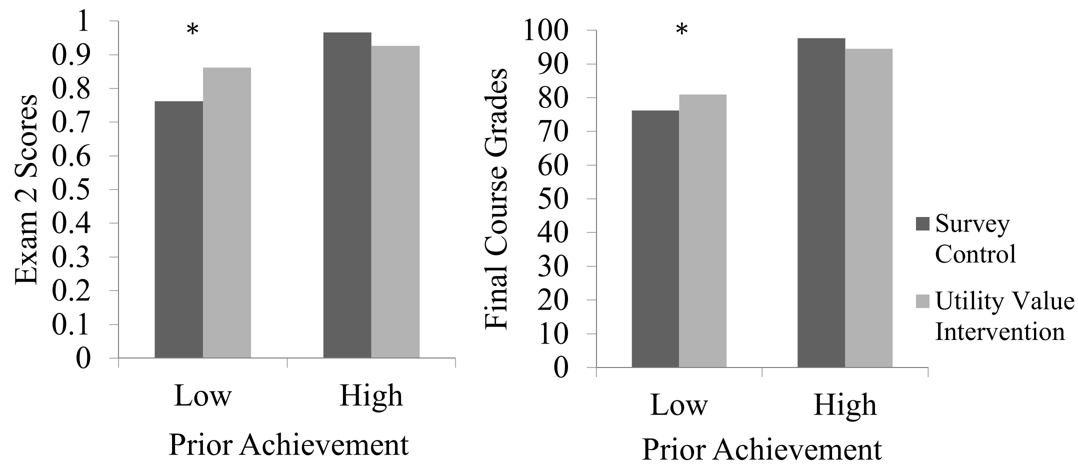

Figure 11. Impact of the utility value intervention on exam 2 scores and final course grades, as a function of students' prior achievement. Low = -1SD from the mean; high = +1SD from the mean. *$p < .05$.

There were some interactions between receiving the utility value intervention versus the summary condition and prior achievement. There was an interaction on the outcome of outside effort cost at Session 2, $\beta = 0.45$, $t(107) = 2.12$, $p = .04$. Follow-up analyses revealed a positive effect such that students with low prior achievement showed a marginally significant trend suggesting that they reported lower outside effort cost if they received the utility value intervention versus the summary condition. Students with high prior achievement reported slightly higher cost in the utility value intervention versus the survey control condition, but this difference was not statistically significant. There was also an interaction on competence-related beliefs at Session 2, $\beta = -0.46$, $t(107) = -2.46$, $p = .02$; this effect was marginally significant at Session 3, $\beta = -0.36$, $t(113) = -1.93$, $p = .06$. Follow up analysis of the significant interaction showed a positive effect such that students with low prior achievement reported significantly higher competence-related beliefs at Session 2 in the utility value intervention versus the summary condition. Students with high prior achievement reported slightly lower competence-related beliefs at Session 2 after receiving the utility value intervention versus the survey control condition, but this difference was not significant.

There were also interactions on course grades, $\beta = -0.43$, $t(125) = -4.38$, $p < .001$, exam 2 scores, $\beta = -0.44$, $t(125) = -2.96$, $p = .004$, exam 3 scores, $\beta = -0.29$, $t(124) = -2.19$, $p = .03$, average exam scores, $\beta = -0.40$, $t(124) = -3.10$, $p = .002$, and average quiz scores, $\beta = -0.35$, $t(125) = -2.18$, $p = .03$. Follow up analyses revealed positive effects such that students with low prior achievement earned significantly higher levels of all course outcomes if they received the utility value intervention versus the summary condition. Students with high prior achievement did not show differences on any outcomes between conditions.

Overview of theoretical moderation effects in the utility value intervention condition. The utility value intervention did not impact students' utility value as a function of their baseline competence-related beliefs or prior achievement in physics. If students began the intervention with low competence-related beliefs in physics, they reported lower cost at Session 3 and earned higher course outcomes after receiving the utility value intervention versus the survey control condition. Students with low prior achievement earned higher course outcomes in the intervention condition versus the survey control condition but did not show differences in motivation. These results were consistent with my hypotheses that the utility value intervention would improve students' course outcomes, and with the hypothesis that competence-related beliefs and/or prior achievement would moderate results, but they were inconsistent with my hypotheses that the intervention would change utility value. Also inconsistent with my hypotheses, students with high prior achievement and high competence-related beliefs reported higher cost if they received the utility value intervention versus the survey control condition. These students did not differ in course outcomes by condition.

Exploratory moderation effects in the utility value intervention: belonging uncertainty. There were no interaction effects between receiving the utility value intervention versus the survey control condition and students' belonging uncertainty on motivation.

For course outcomes, there were interactions between receiving the utility value intervention versus the survey control condition and belonging uncertainty on course grades, $\beta = 0.38$, $t(126) = 2.58$, $p = .01$, exam 2 scores, $\beta = 0.43$, $t(125) = 2.33$, $p = .02$, average exam scores, $\beta = 0.42$, $t(124) = 2.29$, $p = .02$, and the number of STEM courses students took in the semester following the intervention, $\beta = 0.43$, $t(129) = 2.33$, $p = .02$. The effect on exam 3 scores was marginally significant, $\beta = 0.34$, $t(124) = 1.80$, $p = .07$, but there was no effect on quiz scores. Follow-up analyses for the significant interactions showed that students with low belonging uncertainty seemed to earn slightly lower course outcomes if they received the utility value intervention versus the survey control condition, but no differences were statistically significant. Conversely, students with high belonging uncertainty showed positive effects suggesting that they earned significantly higher scores on all four outcomes if they received the utility value intervention compared to the survey control condition. There were no significant effects on the homework time, discussion section attendance, or course-taking patterns.

Exploratory moderation effects in the utility value intervention: beliefs about the malleability of intelligence. There were some interaction effects suggesting that the effects of receiving the utility value intervention versus the survey control condition depended on students' beliefs about the malleability of intelligence. For the outcome utility value, there was a significant interaction at Session 2, $\beta = -0.34$, $t(118) = -1.95$, $p = .05$. Follow-up analyses revealed a positive effect such that students with strong malleable beliefs about intelligence reported higher utility value at Session 2 if they received the utility value intervention versus the survey control condition. Students with strong fixed beliefs about intelligence reported slightly lower utility value if they received the utility value intervention versus the survey control condition, but this difference was not statistically significant.

There were significant interactions between receiving the utility value intervention versus the survey control condition and beliefs about the malleability of intelligence on emotional cost at Session 1, $\beta = 0.39$, $t(139) = 2.25$, $p = .03$, Session 2, $\beta = 0.45$, $t(118) = 2.64$, $p = .01$, and Session 3, $\beta = 0.40$, $t(123) = 2.09$, $p = .04$, outside effort cost at Session 2, $\beta = 0.36$, $t(118) = 2.14$, $p = .03$, and total cost at Session 1, $\beta = 0.35$, $t(133) = 1.95$, $p = .05$, and Session 2, $\beta = 0.41$, $t(118) = 2.40$, $p = .02$. The effect on total cost at Session 3 was marginally significant, $\beta = 0.36$, $t(123) = 1.89$, $p = .06$. Follow-up analyses for the significant interactions revealed that students who began the intervention with strong malleable beliefs about intelligence seemed to report slightly lower cost in all interactions if they received the utility value intervention versus the survey control condition, but no differences were statistically significant. Conversely, students who began the intervention with strong fixed beliefs about intelligence showed undermining effects such that they reported significantly higher emotional cost at Sessions 1 and 2, outside effort cost at Session 2, and total cost at Session 2 if they received the utility value intervention versus the survey control condition. For the other analyses, they appeared to report slightly higher cost in the utility value intervention versus the survey control condition; however, the differences between conditions were not statistically significant.

For non-focal motivational constructs, there were interactions on the outcomes of competence-related beliefs at Session 1, $\beta = -0.46$, $t(139) = -2.77$, $p = .01$, and Session 2, $\beta = -0.43$, $t(118) = -2.57$, $p = .01$, and intrinsic value at Session 2, $\beta = -0.38$, $t(118) = 2.18$, $p = .03$. The effect on competence-related beliefs was marginally significant at Session 3, $\beta = -0.37$, $t(123) = -1.93$, $p = .06$. Follow-up analyses for significant interactions revealed that students with strong malleable beliefs about intelligence reported significantly higher competence-related beliefs at Session 2, and showed a marginally significant trend suggesting that they reported higher competence-related beliefs at Session 1, after receiving the utility value intervention versus the survey control condition. They also reported slightly higher intrinsic value at Session 2 if they received in the utility value intervention, but this simple effect was not statistically significant. Conversely, students with strong fixed beliefs about intelligence reported significantly lower competence-related beliefs at Session 1 and intrinsic value at Session 2 after receiving the utility value intervention versus the survey control condition. These students also appeared to report slightly lower competence-related beliefs at Session 2 if they received the utility value intervention, but this difference was not statistically significant. There were no interactions between receiving the utility value intervention versus the survey control condition and beliefs about the malleability of intelligence on any course outcomes.

Most of the effects just noted did not hold when using the summary condition as a reference group. There was one significant interaction on course grades, $\beta = 0.33$, $t(125) = 1.97$, $p = .05$. The nature of this interaction was different from what I observed using the survey control condition as a reference group. Students with strong malleable beliefs about intelligence earned slightly lower grades in the utility value intervention versus the summary condition, but the simple effect was not statistically significant. Students with strong fixed beliefs about intelligence showed a marginally significant positive trend suggesting that they earned higher course grades after receiving the utility value intervention versus the summary condition.

Exploratory moderation effects in the utility value intervention: gender. There were some interactions suggesting that the utility value intervention impacted outcomes as a function of students' gender. For cost, there were interactions at Session 3 on outside effort cost, $\beta = -0.35$, $t(123) = -2.03$, $p = .05$, emotional cost, $\beta = -0.44$, $t(123) = -2.65$, $p = .01$, and total cost, $\beta = -0.38$, $t(123) = -2.25$, $p = .03$ (see Figure 12). The effect on emotional cost was marginally significant at Session 2, $\beta = -0.31$, $t(119) = -1.71$, $p = .09$. Follow-up analyses for significant interactions revealed positive effects such that female students reported significantly lower emotional cost if they received the utility value intervention versus the survey control condition, and they showed a marginally significant trend suggesting that they reported lower total cost. They reported slightly lower outside effort cost if they received the utility value intervention, but the simple effect was not statistically significant. Conversely, male students reported slightly higher cost in all analyses if they received the utility value intervention versus the survey control condition; this difference was not statistically significant for emotional or total cost and was marginally significant for outside effort cost.

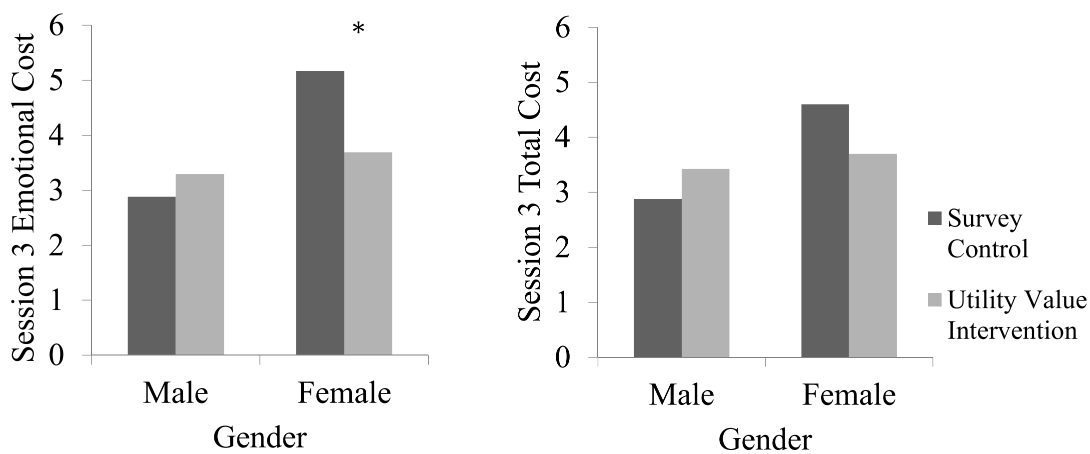

Figure 12. Impact of the utility value intervention on emotional and total cost at Session 3, as a function of students' gender. *$p < .05$.

There was also an interaction on the outcome of Session 2 competence-related beliefs, $\beta = 0.36$, $t(119) = 2.00$, $p = .05$. Follow-up analyses revealed that female students reported higher competence-related beliefs after receiving the utility value intervention versus the survey control condition. Male students did not differ by condition.

In terms of course outcomes, there were interactions between receiving the utility value intervention versus the survey control condition and gender on course grades, $\beta = 0.36$, $t(125) = 2.08$, $p = .04$, exam 2 scores, $\beta = 0.36$, $t(124) = 2.17$, $p = .03$, exam 3 scores, $\beta = 0.42$, $t(123) = 2.45$, $p = .02$, and average exam scores, $\beta = 0.45$, $t(123) = 2.65$, $p = .01$ (see Figure 12). There was no effect on quiz scores. Follow-up analyses revealed positive effects such that female students earned significantly or marginally significantly higher scores on all outcomes if they received the utility value intervention versus the survey control condition. Male students did not show differences by condition on any outcomes. There were no effects on homework time, discussion section attendance, or course-taking patterns. There were also no interactions between receiving the utility value intervention versus the summary condition and gender on any outcomes.

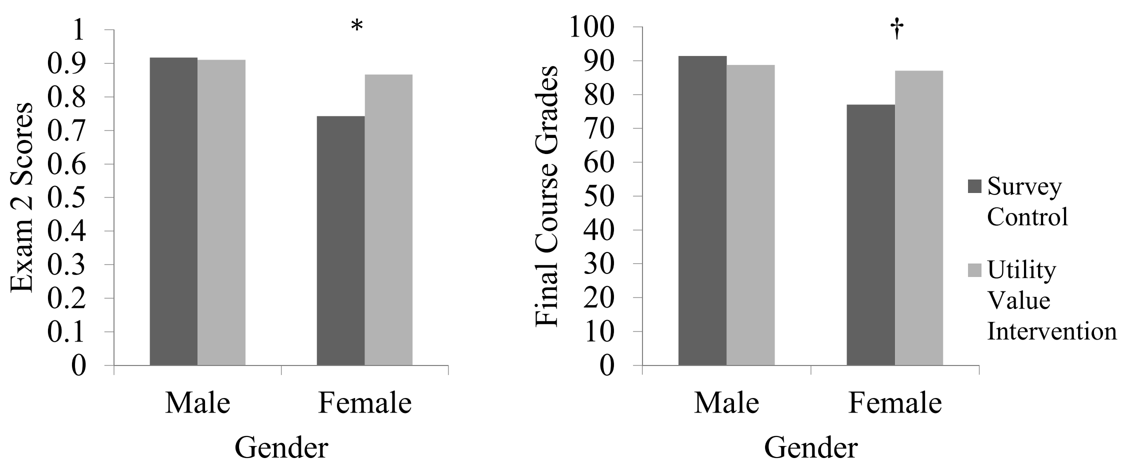

Figure 13. Impact of the utility value intervention on exam 2 scores and final course grades, as a function of students' gender. *$p < .05$; † $p < .10$.

Overview of exploratory moderation effects in the utility value intervention condition. The utility value intervention did not impact students' utility value consistently as a function of their beliefs about intelligence, belonging uncertainty, or gender, besides one effect suggesting that the intervention caused students with strong malleable beliefs about intelligence to report higher utility value at Session 2. Female students reported lower cost at Session 3 and earned higher course outcomes after receiving the utility value intervention versus the survey control condition. Students who had strong malleable beliefs about intelligence and female students also reported higher competence-related beliefs after receiving the utility value intervention. Students with high belonging uncertainty in physics earned higher course outcomes in the utility value intervention condition versus the survey control condition but did not show differences in motivation. Receiving the utility value intervention relative to the survey control condition showed some undermining effects on motivation, however. In particular, students with strong fixed beliefs about intelligence reported higher cost, and students with strong fixed beliefs about intelligence reported lower competence-related beliefs at Session 1.

Moderation effects in the summary condition. Table 17 depicts which effects were significant for the major outcome variables explored in the summary condition, and Appendix F shows the complete output for all regression analyses conducted.

As discussed above, I evaluated the main effects of each condition compared to the survey control or summary conditions using Step 1 of two-step regression models; this step included three terms representing the intervention and control conditions, but it did not include moderating variables or interaction terms. I did not find any significant main effects of the summary condition compared to the other conditions in those models.

To assess Research Question 6, I looked at Step 2 of the two-step models; this step included a given moderating variable as a covariate as well as the interaction of that moderating variable with the three terms from Step 1. I was interested primarily in the interaction terms in Step 2 of the models. However, as a result of adding a moderator and interaction terms to the models, the main effects of the summary condition changed slightly from what I had observed in Step 1 of the models. In particular, there emerged some evidence of main effects in the summary condition compared to the other conditions that I had not observed in my focal models addressing Research Questions 1-5. I discuss first these additional main effects from Step 2 of these models, and then I turn to discuss the interaction effects that I observed in Step 2 of the models.

Main effects of the summary condition that emerged only in moderation analyses. There were some additional main effects at Step 2 of the models testing for moderation which suggested that the summary condition might have caused students to report higher perceptions of cost overall. In models that tested competence-related beliefs as a moderator, students in the summary condition at Session 3 showed undermining effects compared to the cost intervention condition such that they reported overall higher effort cost, $\beta = 0.29$, $t(123) = 1.79$, $p = .02$, outside effort cost, $\beta = 0.26$, $t(123) = 1.99$, $p = .05$, emotional cost, $\beta = 0.34$, $t(123) = 3.11$, $p = .002$, and total cost, $\beta = 0.33$, $t(123) = 2.79$, $p = .01$. In the same models, students in the summary condition also reported higher cost at Session 3 compared to the utility value intervention condition (emotional cost, $\beta = 0.27$, $t(123) = 2.51$, $p = .01$; total cost, $\beta = 0.25$, $t(123) = 2.17$, $p = .03$) and the survey control condition (effort cost: $\beta = 0.30$, $t(123) = 2.99$, $p = .003$; outside effort cost, $\beta = 0.27$, $t(123) = 2.46$, $p = .02$; emotional cost, $\beta = 0.23$, $t(123) = 2.95$, $p = .004$; total cost, $\beta =$

0.29, $t(123) = 2.95$, $p = .004$; there was also an effect on outside effort cost at Session 1, $\beta = 0.22$, $t(136)= 2.27$, $p = .03$). Additionally, in models testing for beliefs about the malleability of intelligence as a moderator, students in the summary condition showed an undermining effect such that they reported higher total cost at Session 3, $\beta = 0.23$, $t(123) = 2.05$, $p = .04$, versus students in the survey control condition.

In models that tested the effects of the conditions on non-focal motivational constructs, there were two additional main effects of the summary condition. In the models testing for beliefs about the malleability of intelligence as a moderator, students in the summary condition showed a positive effect such that they reported higher competence-related beliefs than did students in the cost intervention condition at Session 1, $\beta = 0.22$, $t = 1.96$, $p = .05$. Also, in models testing for gender as a moderator, students in the summary condition reported higher competence-related beliefs at Session 2 than did students in the cost intervention condition, $\beta = 0.39$, $t(118) = 2.46$, $p = .02$.

Finally, in models that assessed the effects of the conditions on course achievement, there were some main effects suggesting that students in the summary condition had overall lower achievement. In models testing for prior achievement as a moderator, there were main effects suggesting that students in the summary condition earned lower course grades, $\beta = -0.31$, $t(125) = -4.15$, $p < .05$, exam 2 scores, $\beta = -0.33$, $t(125) = -2.95$, $p = .004$, and average exam scores, $\beta = -0.24$, $t(124) = -2.457$, $p = .02$, than did students in the cost intervention condition. Compared to the utility value intervention condition and the survey control condition, students in the summary condition also earned lower outcomes: compared to utility value, grades: $\beta = -0.27$, $t(125) = -3.65$, $p < .001$; exam 2 scores: $\beta = -0.41$, $t(125) = -3.64$, $p < .001$; average exam scores: β = -0.29, t(124) = -2.93, p = .004; compared to survey control, grades: β = -0.17, t(125) = -2.81, p = .01; exam 2 scores, β =0.21, t(125) = -2.21, p = .03. In models testing for competence-related beliefs as a moderator, students in the summary condition showed an additional undermining effect such that they earned lower exam 2 scores compared to students in the utility value intervention condition, β = 0.32, t(124) = 2.67, p = .01.

Moderation effects in the summary condition: baseline competence-related beliefs. There were only two interactions between receiving the summary versus the survey control condition and students' baseline competence-related beliefs. These were on emotional cost at Session 1, β = 0.23, t(139)= 2.22, p = .03, and effort cost at Session 1, β = 0.22, t(136) = 1.96, p = .05. In Session 3 the effect on emotional cost was marginally significant, β = .20, t(123) = 1.92, p = .06. Follow-up analyses of significant effects revealed that students who began the intervention with low baseline competence-related beliefs reported slightly lower emotional and effort cost if they received the summary versus the survey control condition, but no differences were statistically significant. Conversely, students with high baseline competence-related beliefs showed undermining effects, reporting significantly higher effort and emotional cost if they received the summary versus the survey control condition.

Table 17

Moderation Effects for Major Outcomes: Summary Condition versus Survey Control Condition

	Moderator									
	Baseline Competence-Related Beliefs		**Prior Achievement**		**Belonging Uncertainty**		**Beliefs about Intelligence**		**Gender**	
	Low	High	Low	High	Low	High	Mal	Fix	Male	Fem
Session 1										
Utility Value	-	-	-	-	-	-	-	-	-	-
Effort Cost	-	U	-	-	-	-	-	-	-	-
Outside Effort Cost	-	-	-	-	-	-	-	-	-	-
Emotional Cost	-	U	-	-	-	-	-	-	-	-
Total Cost	-	-	-	-	-	-	-	-	-	-
Competence-Related Beliefs	-	-	*U*	-	-	-	P	U	-	-
Session 2										
Utility Value	-	-	-	-	-	-	-	-	-	-
Effort Cost	-	-	-	-	-	P	-	-	-	-
Outside Effort Cost	-	-	-	-	-	-	-	-	-	-
Emotional Cost	-	-	-	-	-	-	-	-	-	-
Total Cost	-	-	-	-	-	-	-	-	-	-
Competence-Related Beliefs	-	-	-	-	-	-	-	-	-	-
Session 3										
Utility Value	-	-	-	-	-	-	-	-	-	-
Effort Cost	-	-	-	-	-	-	-	-	-	-
Outside Effort Cost	-	-	-	-	-	-	-	-	-	-
Emotional Cost	-	-	-	-	-	-	-	-	-	-
Total Cost	-	-	-	-	-	-	-	-	-	-
Competence-Related Beliefs	-	-	-	-	-	-	-	-	-	-
Course Outcomes										
Average Quiz Scores	-	-	-	-	-	-	-	-	-	-

Exam 2 Scores	-	-	-	-	-	-	-	-	-	-
Exam 3 Scores	-	-	-	-	-	-	-	-	-	-
Average Exam Scores	-	-	-	-	-	-	-	-	-	-
Final Course Grades	-	-	U	-	-	*P*	*P*	*U*	-	-
Enrollment in Future STEM Courses	-	-	-	-	-	-	-	-	-	-

Note: P = Positive impact on outcome (i.e., decreases to cost, increases to competence-related beliefs, utility value, or course outcomes); U = Undermining impact on outcome (i.e., increases to cost, decreases to competence-related beliefs, utility value, or course outcomes. All non-italicized effects are significant at $p < .05$; italicized effects are marginally significant at $p < .10$. Effects shown are simple effects for students at different levels of the moderating variable.

Moderation effects in the summary condition: prior achievement. There were two significant interactions between receiving the summary condition versus the survey control condition and prior course achievement. One was on students' competence-related beliefs at Session 1, $\beta = 0.19$, $t(125) = 2.12$, $p = .04$; this effect was marginally significant at Session 3, $\beta = 0.18$, $t(113) = 1.79$, $p = .08$. The other was on course grades, $\beta = 0.13$, $t(125) = 2.10$, $p = .04$. Follow-up analyses suggested undermining effects such that students with low prior achievement earned significantly lower course grades, and showed a marginally significant trend suggesting that they reported lower competence-related beliefs, in the summary versus the survey control condition. Students with high prior achievement did not show differences in grades by condition. These students appeared to report slightly higher competence-related beliefs in the summary versus the survey control condition, but this difference was not statistically significant.

Moderation effects in the summary condition: belonging uncertainty. There were three interactions between receiving the summary versus the survey control condition and students' belonging uncertainty. One was on effort cost at Session 2, $\beta = -0.27$, $t(119) = -$

2.11, $p = .04$. Follow-up analyses revealed that students with low belonging uncertainty appeared to report slightly higher effort cost if they received the summary versus the survey control condition, but this difference was not statistically significant. Students with high belonging uncertainty showed a positive effect, reporting significantly lower effort cost at Session 2 if they received the summary versus the survey control condition. The second was an interaction on attainment value at Session 3, $\beta = 0.33$, $t(124) = 2.58$, $p = .01$. Students with low belonging uncertainty did not differ significantly by condition on attainment value, but they appeared to report slightly lower value if they received the summary versus the survey control condition. Students with high belonging uncertainty showed a positive effect such that they reported significantly higher attainment value in the summary condition.

There was also an interaction between receiving the summary versus survey control condition and belonging uncertainty on course grades, $\beta = 0.27$, $t(126) = 2.28$, $p = .02$. That effect was marginally significant for quiz scores, $\beta = 0.22$, $t(125) = 1.72$, $p = .09$. Follow-up analyses for the significant interaction revealed that students with low belonging uncertainty seemed to earn slightly lower grades if they received the summary versus the survey control condition, but this difference was not statistically significant. Students with high belonging uncertainty showed a marginally significant positive trend suggesting that they earned higher grades in the summary versus the survey control condition.

Moderation effects in the summary condition: beliefs about the malleability of intelligence. There were some interactions between receiving the summary condition versus the survey control condition and students' beliefs about the malleability of intelligence. There were interactions on the outcomes of competence-related beliefs at Session 1, β = -0.36, $t(139)$ = -3.33, p = .001, attainment value at Session 2, β = -0.24, $t(118)$ = -2.03, p = .04, and intrinsic value at Session 2, β = -0.27, $t(118)$ = 1.95, p = .05. There was also a marginally significant interaction on competence-related beliefs at Session 2, β = -0.20, $t(118)$ = -1.70, p = .09. Follow-up analyses for significant interactions revealed that students with strong malleable beliefs about intelligence showed positive effects such that they reported significantly higher Session 1 competence-related beliefs and attainment value, and showed a marginally significant trend towards reporting higher intrinsic value, after receiving the summary versus the survey control condition. Students with strong fixed beliefs about intelligence, conversely, showed an undermining effect such that they reported significantly lower Session 1 competence-related beliefs in the summary versus the survey control condition. They also appeared to report slightly lower intrinsic value and attainment value in the summary condition versus the survey control condition, but this difference was not statistically significant.

In terms of course outcomes, there was an interaction between receiving the summary versus survey control condition and beliefs about the malleability of intelligence on grades, β = -0.32, $t(125)$ = -2.49, p = .01. These effects were marginally significant for exam 3 scores, β = -0.23, $t(123)$ = -1.73, p = .09, and average exam scores, β = -0.25, $t(123)$ = -1.87, p = .06. Follow-up analyses of the significant effect revealed that students with strong malleable beliefs about intelligence showed a marginally significant trend suggesting that they earned higher grades in the summary versus the survey control condition. Students with strong fixed beliefs about intelligence showed a marginally significant effect suggesting that they earned lower grades in the summary versus the survey control condition. There was also an interaction on the outcome of discussion section attendance, $\beta = 0.25$, $t(125) = 1.95$, $p = .05$. Follow-up analyses of this interaction revealed that students with strong malleable beliefs about intelligence appeared to have slightly higher attendance if they received the summary versus the survey control condition, but this difference was not statistically significant. Students with strong fixed beliefs about intelligence showed much higher discussion section attendance after receiving the summary versus the survey control condition; however, the difference between the two conditions was only marginally significant.

Moderation effects in the summary condition: gender. There were a few interactions between the effects of the summary condition versus the survey control condition and gender. There were interactions on intrinsic value at Session 1, $\beta = 0.26$, $t(140) = 2.07$, $p = .04$, and Session 3, $\beta = 0.33$, $t(123) = 2.43$, $p = .02$. The effect at Session 2 was marginally significant, $\beta = 0.27$, $t(119) = 1.80$, $p = .08$. There was also an interaction on attainment value at Session 3, $\beta = 0.28$, $t(123) = 1.97$, $p = .05$. This effect was marginally significant at Session 1, $\beta = 0.23$, $t(140) = 1.97$, $p = .08$. Follow-up analyses of the significant interactions revealed positive effects such that female students reported significantly higher intrinsic and attainment value if they received the summary condition versus the survey control condition. Male students appeared to report slightly lower intrinsic value if they received the summary versus the survey control condition, but differences were not statistically significant. There were no interactions on course outcomes.

Overview of moderation in the summary condition. There were few clear results of receiving the summary condition versus the survey control condition on motivation or course outcomes. There was some evidence the summary condition may have caused students to report higher cost and to earn lower course outcomes overall. Additionally, female students, students with high belonging uncertainty, and students with strong malleable beliefs about intelligence reported higher attainment and/or intrinsic value as a result of receiving the summary versus the survey control condition. However, these students did not show differences in course outcomes.

Research questions e1 – e3: exploratory mediation analyses. I ran mediation models to explore whether the effects of a particular intervention condition on exam 3 scores or STEM course enrollment in the semester following the intervention were driven by changes to cost, utility value, or competence-related beliefs. I planned these analyses in order to explore whether changes to motivation explained any effects of the cost or utility value interventions on these two outcomes. As I noted above, I did not find any main effects of either intervention condition on either outcome. However, I still ran the mediation models, because it is possible that there can be a mediating effect even if there is no overall effect of a variable on an outcome (Hayes, 2009).

To conduct the planned mediation analyses, I ran twelve serial mediation models, with the mediators being students' perceived cost, perceived utility value, and perceived competence-related beliefs. I evaluated whether cost, utility value, or competence-related beliefs measured across the three sessions mediated the relationship between (a) receiving the cost intervention versus the survey control condition and students' final exam scores, (b) receiving the cost intervention versus the survey control condition and students' enrollment in STEM courses during the semester following the intervention, (c) receiving the utility value intervention versus the survey control condition and students' final exam scores, and (d) receiving the utility value intervention versus the survey control condition and students' STEM course enrollment. My analyses only used participants from the respective intervention and control conditions that were the focus of each model.

Results are presented in Table 18. The mediation procedure (Hayes, 2013, Model 6) produced a point estimate for the total indirect effect of an intervention on an outcome via a given mediating variable. The procedure also produced a 95% confidence interval for the indirect effect; if the confidence interval did not contain zero, I could consider the effect to be significant using an alpha criterion of $p < .05$. The estimate of the total indirect effect was a sum of seven indirect effects that constituted my serial mediation model: the effects of the intervention on the outcome via the mediating variable at each *individual* session (i.e., at Session 1, at Session 2, at Session 3), *across two* of the three sessions (i.e., across Sessions 1 and 2, across Sessions 1 and 3, across Sessions 2 and 3), and *across all three sessions* (i.e., across Sessions 1, 2, and 3). No overall indirect effects were significant. I also looked at each of the seven component indirect effects that produced the total indirect effect in each mediation analysis, to determine whether there was any evidence of mediation in the individual effects, but there were only three significant differences among 84 possible results. Together, results suggest that cost, utility value, and competence-related beliefs did not mediate overall the relationship between the cost or utility value interventions and students' exam 3 scores or STEM course enrollment.

Table 18

Results for Planned Mediation Analyses on Exam 3 Scores and Later STEM Course Enrollment

Outcome	**Condition**	**Mediator**	**Total Indirect Effect**	**Boot SE**	**Boot LLCI**	**Boot ULCI**
Exam 3 Scores	Cost Intervention	Utility Value	.002	.01	-.02	.03
		Cost	-.001	.03	-.06	.06
		Competence-Related Beliefs	-.02	.03	-.09	.03
	Utility Value Intervention	Utility Value	.005	.01	-.02	.03
		Cost	-.02	.02	-.07	.01
		Competence-Related Beliefs	-.01	.02	-.05	.03
Enrollment in STEM Courses	Cost Intervention	Utility Value	.05	.08	-.07	.28
		Cost	.09	.12	-.14	.34
		Competence-Related Beliefs	-.05	.13	-.35	.18
	Utility Value Intervention	Utility Value	-.08	.10	-.35	.07
		Cost	.10	.11	-.10	.36
		Competence-Related Beliefs	-.05	.11	-.33	.13

Note: *$p \leq .05$; ** $p \leq .01$; $^{+}p < .10$. Results are based on the survey control condition as a reference group.

As just noted, the planned mediation models were based on hypotheses that I would observe overall effects of the interventions. However, I found very few overall effects of the cost or utility value intervention conditions on any outcomes in this study. Instead, most significant effects observed in this study were interactions, meaning that the effects of a given intervention on motivation or achievement depended on various moderating variables. I therefore chose to conduct additional exploratory analyses to test whether moderated mediation effects might explain results. If regular mediation occurs, this means that the impact of an independent variable on an outcome variable can be explained in part by some mediating variable. *Moderated* mediation means that the impact of an independent variable on a mediating variable differs as a function of some moderating variable, or the impact of a mediating variable on an outcome differs as a function of a moderating variable (Hayes, 2015). In the present study, it appeared that the impact of the cost and/or utility value interventions on the mediating variables of cost and competence-related beliefs might differ as a function of moderating variables. I wanted to test whether these moderated mediation effects could explain the changes to course achievement that I observed for some students.

I was cautious in conducting additional analyses. I had run many models to test for moderation and I did not want to test for moderated mediation in all of those because it would inflate my risk of Type 1 error very much. I also had a small sample and thus I could not test for conclusive proof of moderated mediation in my data. To address these concerns, I chose a few specific models to probe further some clear patterns of moderating results that had emerged when assessing the research questions and I planned consider these results to be a first step in understanding the mechanisms of effects in this study.

I tested moderated mediation only for the three moderators that had shown consistent effects on students' motivation and course outcomes: gender, beliefs about the malleability of intelligence, and prior achievement. I focused on a mediator measured at a single time point, students' motivation after Session 2, and a single outcome, students' course grades. This represented the outcome and time point at which I most often observed interactions with the three moderators just mentioned. Finally, I tested only cost and competence-related beliefs as mediators, because I had observed few effects on utility value. This resulted in twelve possible moderated mediation models, which assessed whether any of the three moderators (gender, beliefs about the malleability of intelligence, and prior achievement) influenced the indirect effect of a mediator to explain the relationship between an intervention and an outcome. The mediating relationships in question were: (1) The mediator of cost explaining the relationship between receiving the utility value intervention and students' course grades; (2) The mediator of cost explaining the relationship between receiving the cost intervention and students' course grades; (3) The mediator of competence-related beliefs explaining the relationship between receiving the utility value intervention and students' course grades; (4) The mediator of competence-related beliefs explaining the relationship between receiving the cost intervention and students' course grades. I ran ten of these twelve models. I did not observed moderation by prior achievement on competence-related beliefs in either intervention, so I omitted the two models testing whether prior achievement moderated the ability of competence-related beliefs to mediate effects of either intervention on grades.

I ran moderated mediation models using the PROCESS macro in SPSS with a 95% confidence interval and 5000 bootstrap resamples (Hayes, 2013; Model 7). Results are reported in Table 19. Reported is the index of moderated mediation for each model, which indicates the extent to which the indirect effect in a mediation model differs at different levels of a moderating variable (Hayes, 2015). The index of moderated mediation was significant for 7 of the 10 models I tested. Beliefs about the malleability of intelligence showed moderated mediation for all four models that included this moderator: the cost intervention affecting course grades through cost, the utility value intervention affecting course grades through cost, the cost intervention affecting course grades through competence-related beliefs, and the utility value intervention affecting course grades through competence-related beliefs. Prior achievement showed moderated mediation for the two models that included this moderator: the cost and utility value interventions affecting course grades through cost. Finally, gender moderated the mediating relationship of the cost intervention affecting exam scores through competence-related beliefs.

I next looked at the estimates of the indirect effects of the mediating variables at different levels of a given moderating variable. As can be seen in Table 19, students with strong fixed beliefs about intelligence showed significant indirect effects for the mediators of cost and competence-related beliefs in both interventions. This suggests that higher cost and lower competence-related beliefs explained why fixed-belief students earned lower course grades if they received the cost or utility value intervention conditions versus the survey control condition. Students with strong malleable beliefs about intelligence showed significant mediating effects through competence-related beliefs in both interventions, and they showed mediating effects through cost in the cost intervention. This suggests that higher competence-related beliefs explained why malleable-belief students earned higher course grades if they were in the cost or utility value intervention compared to the survey control condition, and lower cost also explained part of this relationship for students receiving the cost intervention condition. The other indirect effects were not significant. However, the direction of all indirect effects was consistent with what would be expected if changes to cost and/or competence-related beliefs did explain the differential impacts of the interventions on grades as a function of the moderators. This pattern of results supports the interpretation that the effects of the interventions on different students' course grades were due to the interventions impacting those students' cost and/or competence-related beliefs.

Table 19

Results for Moderated Mediation Analyses on Final Course Grades

Condition	**Moderator**	**Mediator**	**Index of Moderated Mediation**		**Indirect Effect at -1 SD/ Males**		**Indirect Effect at +1 SD/ Females**	
			Est.	**SE**	**Est.**	**SE**	**Est.**	**SE**
Cost Intervention	Beliefs about Intelligence	Cost	-6.83*	2.30	5.54*	2.94	-7.24*	2.73
		Competence-Related Beliefs	-5.36*	2.17	4.44*	2.64	-5.59*	2.49
	Prior Achievement	Cost	-3.31*	1.61	5.12*	2.55	-1.65	1.82
	Gender	Cost	5.40	5.06	-1.41	1.87	3.98	4.58
		Competence-Related Beliefs	9.62*	4.78	-2.70	1.43	6.92	4.48
Utility Value Intervention	Beliefs about Intelligence	Cost	-3.40*	1.66	1.92	1.97	-4.97*	2.29
		Competence-Related Beliefs	-4.21*	2.00	4.10*	2.41	-4.45*	2.64
	Prior Achievement	Cost	-2.51*	1.18	2.21	1.45	-2.70*	1.56
	Gender	Cost	4.33	3.78	-1.73	1.37	2.59	3.40
		Competence-Related Beliefs	6.27	4.56	-0.90	1.13	5.37	4.38

Note: *Bootstrap 95% CI does not include zero and result should be considered significant at $p < .05$. The mediator variables were measured at Session 2 and the outcome used in analyses is students' final course grades

Chapter 5: Discussion

In this chapter, I discuss how the cost and utility value intervention conditions affected undergraduate students' motivation and course outcomes in physics as well as the implications of these findings for educational practice and for future research. First, I focus on the key findings for each condition and note the major contributions of these findings to the field. Then, I discuss *why* this specific pattern of results may have occurred. In particular, I focus on explaining the effects of moderating variables in the cost and utility value interventions. Finally, I discuss the implications of this study for developing interventions in the future.

Throughout the preceding chapter I conducted many analyses. Thus it was possible that some of the effects observed in Chapter 4 did not represent true differences between conditions but instead occurred as a function of study-wise error. This is a limitation of the present study. To address it in part, I focus my discussion on patterns of significant results rather than interpreting each significant effect that I observed.

Major Findings: Impacts of the Cost and Utility Value Interventions in College Physics

Major findings for the cost intervention. The primary goal of this study was to explore whether an intervention could be developed that would reduce students' perceptions of cost and improve their outcomes in an introductory college physics course. The cost intervention did impact students' perceptions of cost and their subsequent course outcomes relative to the survey control condition. However, contrary to my hypotheses, the intervention did not benefit all students. Instead, student-level moderating variables influenced whether the cost intervention showed positive or undermining impacts.

Specifically, female students receiving the cost intervention reported lower perceptions of cost and higher competence-related beliefs, earned higher course grades and exam scores, and took more STEM courses in the following semester compared to female students in the survey control condition. Students with low prior achievement or low baseline competence-related beliefs in physics also reported lower cost and showed higher achievement if they received the cost intervention. Finally, students with strong malleable beliefs about intelligence reported lower cost and higher competence-related beliefs, but did not earn different course outcomes, after receiving the cost intervention condition versus the survey control condition. Although not fully supportive of my hypotheses, these effects were consistent with my predictions in that the cost intervention condition impacted some students positively. These findings also are consistent with prior research demonstrating that many motivation interventions benefit some students more than others (see Rosenzweig & Wigfield, 2016, for review). Unexpectedly, however, students who began the intervention with strong fixed beliefs about intelligence or low uncertainty about belonging in the course reported higher cost and had lower course grades if they received the cost intervention versus the survey control condition.

Although not all of the effects on cost were positive, this study is the first to demonstrate that it is possible to develop a motivation intervention that changes students' perceptions of cost in any way. Results support the notion that it is possible to implement interventions that target specifically motivational constructs from EEVT besides utility value. Further, the cost intervention impacted some students' physics course grades, exam scores, quiz scores, and course-taking patterns. Research has demonstrated that cost is a strong predictor of students' STEM outcomes (e.g., Conley, 2012; Jiang et al., 2017;

Perez et al., 2014). This study adds to that work by demonstrating that it is possible to *cause* changes to STEM outcomes as a function of administering an intervention that changes students' perceptions of cost. Results provide a first step in exploring how educators might use interventions to target the perceived cost and subsequent course outcomes of students enrolled in introductory college STEM courses.

Another important finding is that the cost intervention affected the motivation and course outcomes of some students positively but impacted other students negatively. Few prior researchers have conducted interventions targeting cost. However, researchers have reported that students who are likely to have high belonging uncertainty (i.e., female students; African American students) were the only ones who benefitted after receiving interventions that aimed to reduce students' belonging uncertainty (Walton & Cohen, 2007; 2011; Walton et al., 2015). Belonging uncertainty is related to emotional cost, so my observed results are consistent with the conclusion that a cost-related intervention will impact some college students differently than others. These results expand upon that prior work because they demonstrate which specific student characteristics impact whether students respond well or poorly to cost-targeting interventions. Students with low prior achievement or competence-related beliefs, who believe that intelligence is malleable, or who are female likely will benefit from interventions targeting perceived cost. This is a particularly important conclusion, because female students and low-achieving students are among those who are the most at risk for dropout in STEM fields. However, students with a strong fixed belief about intelligence or who have low belonging uncertainty might perceive more cost or doubt their competence as a result of being asked to complete an intervention focused on cost. These results suggest that researchers should proceed with caution when implementing cost interventions with some students, due to potential undermining effects. As I discuss below, the utility value intervention also showed some undermining effects on motivation. Thus *all* researchers conducting EEVT-based interventions need to explore further the potential for undermining effects of those interventions.

A secondary goal of this study was to examine college students' perceptions of cost over time and consider what might be the ideal timing to intervene to reduce students' perceptions of cost. Students' average reports for cost in the survey control condition were low overall, but they were slightly higher at Session 3 than at Session 1. These findings suggest that cost experiences increase for students over the course of the semester, and so some time may need to pass in a semester before students can reflect on their cost experiences. However, the majority of the effects on cost occurred at Sessions 2 or 3. It is possible that the intervention's impacts on cost only emerged when students' perceptions of cost in the course were high. It is also possible that the cost intervention needed multiple sessions to impact students' motivation, or some time needed to elapse before the intervention showed an impact. If the latter point is true, cost-targeting interventions may be best implemented in the middle of a semester. In that circumstance, students have experienced sufficient cost to think about it during an intervention, but there is still time for the intervention to impact students over time. I do not make strong recommendations about intervention implementation, because some students' changes to cost and competence-related beliefs were negative in this study. Researchers need to understand further how to mitigate potential undermining effects before considering the best timing to implement a cost intervention.

Major findings for the utility value intervention. My second major goal for this study was to evaluate whether a utility value intervention would impact students' utility value and course outcomes in a college physics course. The utility value intervention did not show overall impacts on students' utility value or course outcomes. However, the intervention impacted consistently students' course outcomes as a function of moderating variables and showed some less consistent moderated effects on students' perceptions of cost and competence-related beliefs. In terms of theoretical moderators, students with low baseline competence-related beliefs and low prior achievement earned higher exam scores if they received the utility value intervention versus the survey control condition; students with low baseline competence-related beliefs also reported lower emotional cost at Session 3. Conversely, students with high baseline competence-related beliefs in physics or high prior physics achievement reported higher cost at Sessions 2 and/or 3 if they received the utility value intervention versus the survey control condition. In terms of exploratory moderators, female students reported lower cost and higher competence-related beliefs, and earned higher exam scores, if they received the utility value intervention. Students who began the intervention with strong fixed intelligence beliefs reported higher cost at Session 2 if they received the utility value intervention versus the survey control condition, whereas students with strong malleable beliefs about intelligence reported lower cost. Finally, students with high belonging uncertainty in physics earned higher course outcomes if they received the utility value intervention.

Findings regarding the utility value intervention condition are important for several reasons. First, they are broadly consistent with prior research suggesting that utility value interventions improve the course outcomes of students with low competence-related beliefs and/or low prior achievement (Hulleman & Harackiewicz, 2009; Hulleman et al., 2010; 2016). Findings extend that work by demonstrating that utility value interventions benefit the same groups of students in college physics. They also extend prior work by showing that female students and students with high belonging uncertainty are likely to earn higher course outcomes as a result of receiving a utility value intervention in college physics.

It is interesting that the utility value intervention failed to increase students' self-reported utility value, despite impacting positively some students' course outcomes. As I discuss in the next section, the lack of observed effects on self-reported utility value likely occurred because of a ceiling effect in the physics context in which I administered the intervention. This finding is inconsistent with those of several researchers who have demonstrated that self-reported utility value increased after students completed a utility value intervention (e.g., Canning & Harackiewicz, 2015; Gaspard, Dicke, Flunger, Brisson, et al., 2015; Rosenzweig et al., 2017a). These findings suggest that there may be boundary conditions under which utility value interventions fail to impact students' self-reported utility value, such as students' initial average levels of utility value in a given educational context.

These findings also suggest that there may be alternative mechanisms that explain the effects of the utility value intervention beyond changes to students' explicit perceptions of utility value. As I discuss later in the chapter, mechanisms operating in this study could include changes to students' perceptions of cost and/or competence-related beliefs, as well as changes to students' strength of connections with or engagement with course material. Hulleman et al. (2016) and Canning and Harackiewicz (2015) both reported that utility value interventions impacted some students' course achievement positively because they improved competence-related beliefs, not utility value. However, few researchers have discussed the possibility that students might benefit from utility value interventions in ways that would not change their perceptions of utility value. Because there is little research on this topic, it is unclear precisely how utility value interventions impact different students' course outcomes and whether these mechanisms are similar across different educational contexts. Researchers need to address this topic further in order to understand how utility value interventions impact students in different courses.

Another important finding from the utility value intervention was that it impacted students' outcomes differently as a function of moderating variables. As noted above, findings regarding the moderators of baseline competence-related beliefs and prior achievement support previous research suggesting that these variables moderate the effects of utility value interventions (e.g., Canning & Harackiewicz, 2015; Hulleman & Harackiewicz, 2009; Hulleman et al., 2010, 2016). However, only one prior research group (Gaspard, Dicke, Flunger, Brisson, et al., 2015) has reported that female students benefitted more from utility value interventions, and no prior utility value intervention researchers have explored the moderators of beliefs about intelligence or belonging uncertainty. Thus this study helps develop a base of literature regarding whether and how different student-level characteristics impact students' responses to utility value interventions.

Finally, results of the utility value intervention are important because I observed some undermining effects on motivation as a function of moderating variables. There were a small number of significant undermining effects relative to the total number of analyses I conducted, so it is possible that these effects were partially due to Type 1 error. Additionally, there were no significant differences that suggested the utility value intervention undermined any students' course outcomes. However, no prior researchers have reported undermining effects of utility value interventions to date, and the possibility that these interventions might impact some students negatively is critical to explore further. Researchers should address directly how to mitigate any potential negative responses students might have to utility value intervention materials, because undermining effects could lower students' course outcomes in the future.

Comparing the interventions. A third goal of the present study was to implement cost and utility value interventions in the same educational context and consider whether the two interventions showed similar impacts. Broadly speaking, the cost and utility value interventions showed similar effects on the outcomes of cost and competence-related beliefs. In both conditions, female students and students with low baseline competence-related beliefs reported lower cost, and female students and students with strong malleable beliefs about intelligence reported higher competence-related beliefs, as a result of receiving either intervention versus the survey control condition. Additionally, students with strong fixed beliefs about intelligence, those with high baseline competence-related beliefs, and those with high prior achievement reported higher cost if they received either intervention versus the survey control condition.

It is possible that the similar patterns of results are due to a common methodological element in both interventions: asking students to read and respond to quotations from other students about their course experiences. As I will discuss in the next section, reading quotations from other students may have caused many students to identify with or compare themselves to others in the course, which contributed to changes in cost and/or competence-related beliefs. Reading and responding to quotations may impact negatively certain college students' perceptions of cost (e.g., students with strong fixed beliefs about intelligence, students with high prior achievement or competence-related beliefs) without them needing to engage in intervention activities that target directly these motivational constructs. It is important to consider this method effect when thinking about the best way to support motivation in college STEM courses. It is possible that other EEVT-based interventions, or perhaps any motivation interventions, could impact college students' perceived cost and/or competence-related beliefs if they include examples from other students.

Despite the similarities just noted, the cost and utility value intervention conditions did not show the same patterns of results. First, the impacts on motivation observed for the utility value intervention were less consistent than those observed for the cost intervention. Second, the two interventions showed different undermining effects on cost: Receiving the utility value intervention caused students with low competence-related beliefs and prior achievement to report higher cost at the end of the semester, but in the cost intervention these students only reported higher cost at Session 1. Third, the cost intervention reduced cost for some students (students with strong malleable beliefs about intelligence, and students with low prior achievement), but these same effects were not found in the utility value intervention. Fourth, the cost intervention lowered the course achievement of students with strong fixed beliefs about intelligence and students with low belonging uncertainty, but the utility value intervention did not show any significant undermining effects on achievement. The intervention that was designed to target cost showed more consistent effects on cost, which supports the idea that aligning interventions with theory is critical to produce strong effects (Rosenzweig and Wigfield, 2016; Yeager and Walton; 2011).

It is important that in both interventions I found impacts as a function of moderating variables. This evidence contributes to a growing body of intervention literature arguing that motivation interventions' effects often depend on various student-level variables (see Rosenzweig & Wigfield, 2016, for review). All researchers who administer motivation interventions should consider and assess likely moderating variables in order to make appropriate conclusions about the circumstances under which their interventions are likely to impact students. Had I not conducted moderating variable analyses, it would have appeared that there were no overall effects of either intervention. I may have concluded that these interventions were ineffective in college physics, or that I should implement the interventions with a higher dose in order to strengthen their effects. This conclusion would have been misguided and actually could have further undermined course outcomes for the students who were impacted negatively.

Summary condition. Although not a central goal of this study, I planned to compare the use of two different control conditions and consider how the cost and utility value interventions compared to these. I included a control condition in which students completed only surveys, as well as a condition in which students summarized what they were learning. I wanted the latter condition to provide students with cognitive, but not motivational, support. Unfortunately, I could not draw strong conclusions regarding the summary condition. I had no prior hypotheses regarding what I would observe, my sample for this condition was small, and the pattern of results was not clearly interpretable for any outcome. In some models, students in the summary condition reported higher cost than did students in the other conditions. In other models, students in the summary condition earned overall lower course outcomes compared to students in the other conditions. These effects did not occur consistently across models, and notably, they were not present in the focal models that I used to test for main effects of the summary condition. I also found that female students, students with high belonging uncertainty, and students with strong malleable beliefs about intelligence reported higher attainment value and/or intrinsic value after receiving the summary versus the survey control condition. However, these effects were not consistent across time points, and students did not show corresponding increases to any course outcomes. The fact that this condition impacted motivation at all was counter to my expectations, and this finding meant that it would not be appropriate to consider this condition to be a cognitive-support control group. The impact of this condition on motivation also was not particularly clear. It is likely that the sample size for this condition may have been too small to detect meaningful patterns of effects, which is a major limitation of this study.

Because I could not draw clear conclusions regarding this condition, I do not discuss it further below. The one recommendation I make regarding the summary condition is that intervention researchers should consider their choice of control group carefully. Researchers have administered summary control conditions in prior studies because they argued that the conditions provide cognitive support (e.g., Hulleman & Harackiewicz, 2009). At least in college physics, the summary condition also impacted some students' motivation. Had I used only this control group, it might have been problematic for understanding how the cost and utility value interventions impacted students. Researchers may want to consider using a survey control condition in addition to or instead of a summary control condition, in order to detect accurately the effects of motivation interventions.

Explaining Individual Differences in the Effects of the Cost and Utility Value Interventions

The prior section outlined how the results of the present study contributed to existing research. Many key findings concerned the fact that the cost and utility value interventions impacted different students differently. In this section I address more closely *why* the cost and utility value interventions caused some students to have more positive motivation and course outcomes but caused other students to experience undermining effects to their motivation and/or course outcomes. In each section below, I discuss the effects for each major outcome that each intervention impacted, and then I discuss briefly those outcomes for which I did *not* observe consistent effects. I discuss all results with reference only to the survey control condition as a control group. I also discuss only those effects for which I observed clear and meaningful patterns; I do not discuss at length every effect reported in Chapter 4.

Explaining individual difference effects in the cost intervention. To understand why the cost intervention showed both positive and undermining effects on students' perceptions of cost, I consider each moderating variable in turn. Students with strong malleable beliefs about intelligence reported significantly or marginally significantly lower perceptions of all dimensions of cost if they received the cost intervention versus the survey control condition. However, students with strong fixed beliefs about intelligence reported higher perceptions of all dimensions of cost if they received the cost intervention. Students with malleable beliefs about intelligence believe that their abilities can grow and change over time (Dweck & Master, 2009). Prior research has shown that students exhibit selective attention for information that is consistent with their beliefs about intelligence (Plaks et al., 2001; Plaks et al., 2004). Malleable-belief students might have read very closely the examples of other students' cost experiences getting better, and they might have thought deeply about how their own cost experiences also could improve over time and were common to others. They would have responded to the intervention as intended, by perceiving that their course challenges were more short-term and common than they had before and reporting lower cost.

Conversely, students with strong fixed beliefs about intelligence believe their abilities are stable and cannot change, and they are likely to perceive challenges in a course as reflective of their own low ability (Dweck & Master, 2009). This means that these students are less likely to believe that course challenges can change over time. Plaks et al. (2004) found that when individuals were confronted with information that contradicted their beliefs about intelligence, they responded with cognitive scrutiny and skepticism to the information presented. They also showed increased anxiety, because they had encountered information that challenged the framework with which they understood their course experiences. These processes could have impacted students with strong fixed beliefs about intelligence in the present study. Those students might have been skeptical that other students' course challenges became less salient over time and downplayed the content of those students' messages. They also might have experienced higher anxiety and uncertainty about their own experiences in physics as a result of addressing information that challenged their beliefs, which could lead to higher perceptions of cost. An additional force causing these students' cost to increase could be social comparisons with the students featured in the quotations (Dweck & Master, 2009). Fixed-belief students may have been reminded that their own cost experiences would be stable into the future, whereas other students' cost experiences had changed. This comparison could cause students to believe that their own cost experiences were even more stable than they might have thought otherwise, causing them to report higher perceived cost.

If students started the intervention with low prior achievement or low baseline competence-related beliefs in physics, they reported lower emotional and/or effort cost if they received the cost intervention versus the survey control condition. Students with high prior achievement and baseline competence-related beliefs reported higher emotional cost at Session 1 after receiving the cost intervention condition, but these differences did not persist over time. Students with low prior achievement or baseline competence-related beliefs likely thought often about their course challenges because these challenges interfered with their course performance. These students may have reported lower cost after receiving the intervention because the information helped them think about their salient cost experiences as being more short-term and common than they originally thought. Conversely, students with higher prior achievement or competence-related beliefs may not perceive their cost experiences to be particularly detrimental, because they were still able to perform well in the class despite those challenges. Flake et al. (2015) noted that it is critical that students perceive their cost experiences as being "too much" for them in order for those experiences to impact them negatively, and

Yeager and Walton (2011) have argued that brief interventions only impact students in a lasting fashion if they target recurring psychological processes and beliefs. For students with high baseline competence-related beliefs or prior achievement, receiving the intervention would have targeted a motivational issue that they did not think about often. Perhaps thinking about cost increased their initial perceptions of cost because that topic was more salient than it had been. However, these students likely did not show long-term changes to cost because they did not think about this topic often over time.

Receiving the cost intervention condition caused female students to report lower emotional and total cost compared to the survey control condition, but male students were not affected. It is likely that receiving the cost intervention reduced female students' belonging uncertainty in physics. As noted in Chapter 2, some students experience uncertainty about their belonging in a course as a result of negative stereotypes regarding who typically performs well in the course (Smith, Lewis, Hawthorne, & Hodges, 2012). When female students encounter course challenges in these fields, concerns about these stereotypes can cause them to attribute the challenges to their own low abilities, have lower perceived competence, and feel very uncertain about belonging (Walton et al., 2015). By reading and thinking about how other students' course challenges were common and short-term, female students in the cost intervention might have been better able to separate their own perceptions of their course challenges from their concerns about belonging. This would reduce these students' perceptions of emotional cost.

One factor complicating this explanation is that students with high belonging uncertainty did not show lower emotional cost after receiving the cost intervention condition. Also, students with *low* belonging uncertainty reported *higher* outside effort cost and total cost at Session 3 as a result of receiving the cost intervention relative to the survey control condition. Researchers have shown that sometimes students with low belonging uncertainty can be impacted negatively by interventions in which they think and write about personal values, because it causes them to think more about other things they value besides coursework (Miyake et al., 2010). Similar effects may have occurred in this study: Students with lower-than-average belonging uncertainty may have become more aware of other experiences that were interfering with their coursework when they read quotations about this topic in the cost intervention. However, it is not clear why these students would not benefit from re-interpreting those cost experiences, or why students with high belonging uncertainty would not also report lower emotional cost. There were some issues with measuring belonging uncertainty and emotional cost in this study, so these may have prevented me from assessing accurately these constructs. In particular, the measure of emotional cost did not include any items assessing directly emotional consequences associated with belonging concerns. Also, one of the two items used to measure belonging uncertainty ("Sometimes I feel that I belong at the University of Maryland, and sometimes I feel that I don't belong at the University of Maryland") was worded in a way that students who might answer the question negatively instead of positively if they always believed they did not belong. Future research should explore these topics using alternative measures.

I discuss next the impact of the cost intervention on competence-related beliefs. At all three sessions, students with strong fixed beliefs about intelligence reported significantly lower competence-related beliefs after receiving the cost intervention versus the survey control condition. Conversely, at Sessions 1 and 2, students with strong malleable beliefs about intelligence reported higher competence-related beliefs after receiving the cost intervention. Additionally, at Session 2 female students reported higher competence-related beliefs after receiving the cost intervention versus the survey control condition.

As noted above, female students and students with strong fixed beliefs about intelligence are both particularly likely, for different reasons, to perceive physics course challenges as being reflective of their own low physics abilities. It follows that these variables might moderate the effects of the cost intervention on competence-related beliefs. After reading examples about how other students addressed cost experiences, students with strong fixed beliefs about intelligence might have perceived that they were less competent to succeed in the course because they had been reminded that their own course challenges would be stable into the future. Conversely, female students might have perceived themselves as more competent after reading quotations from other students reminding them that their course challenges were not indicative of low physics abilities. Students with strong malleable beliefs about intelligence would not perceive that course challenges are indicative of their low ability, but rather they would be particularly likely to believe that their ability could change over time. Being reminded of how they and other students had overcome course challenges might have reinforced these students' beliefs that they too could overcome challenges and succeed in the course.

Changes to cost and competence-related beliefs in this intervention seemed to translate to changes in students' course outcomes. Female students, students with low prior achievement, and students with low baseline competence-related beliefs all earned higher scores on at least one course outcome if they received the cost intervention versus the survey control condition. These students all had reported lower cost and/or higher competence-related beliefs after receiving the cost intervention. Conversely, students with strong fixed beliefs about intelligence and students with low belonging uncertainty in physics earned significantly lower course grades, and students who believed intelligence was fixed also earned lower quiz scores, after receiving the cost intervention relative to the survey control condition. These students had reported lower cost and/or higher competence-related beliefs as a result of receiving the cost intervention. This pattern of results suggests that changes to cost might explain why these students' outcomes differed in the cost intervention versus the survey control condition. For female students and those with a strong fixed belief about intelligence, changes to competence-related beliefs also may explain some of the differences observed between conditions on course outcomes. A growing body of theoretical and empirical research demonstrates that cost is a negative predictor of students' course outcomes (e.g., Battle & Wigfield, 2003; Conley, 2012; Jiang et al., 2017; Perez et al., 2014), and much prior research has demonstrated that competence-related beliefs are among the strongest predictors of students' achievement (see Wigfield et al., 2016, for review). The observed results are consistent with what that body of research would predict.

The exploratory moderated mediation analyses provided partial confirmation of this idea. For students with a malleable belief about intelligence, decreases to cost and increases to competence-related beliefs explained the positive relation between receiving the cost intervention versus the survey control condition and course grades. Conversely, for students with a fixed belief about intelligence, increases to cost and decreases to competence-related beliefs both explained the negative relation of the cost intervention versus the survey control condition and course grades. Among students with low prior achievement, decreases to cost again explained the positive relation between receiving the cost intervention (versus the survey control condition) and course grades. Effects for female students were not significant, but the direction of the observed indirect effects of the cost intervention on students' course grades at different levels of the moderating variables were consistent with cost and/or competence-related beliefs mediating these relations. I conducted only a small subset of all possible moderated mediation analyses and had a small sample, so a lack of significant mediation does not necessarily imply that these constructs did not mediate the relations in question.

Not all changes to motivation were consistent with changes to course outcomes. Students who began the intervention with strong malleable beliefs about intelligence reported lower cost and higher competence-related beliefs, but did not show significant differences on course outcomes, if they received the cost intervention versus the survey control condition. However, the moderated mediation analyses reported that changes to cost significantly explained changes to course grades among these students. Also, descriptively, course grades and quiz scores appeared to be higher for these students if they received the cost intervention versus the survey control condition; however, these differences were not statistically significant. Together, results suggest that the cost intervention did impact the course outcomes of students with malleable belies about intelligence in a positive way, likely by reducing cost and/or increasing competence-related beliefs. However, it did not impact these outcomes significantly. Perhaps in another educational context, malleable-belief students would show increased achievement as a result of receiving a cost-targeting intervention.

The findings just noted discussed those outcomes that the cost intervention impacted clearly. There were also some outcomes that the intervention did *not* impact clearly. First, I did not find consistent effects of the cost intervention on students' utility value, attainment value, or intrinsic value. This is not surprising because I did not expect that the cost intervention would engage students in thinking about the value of their course material to any great extent. The only exception to this trend was that students with low competence-related beliefs showed marginally significant trends suggesting that they reported higher intrinsic, attainment, and utility value at Session 3 if they received the cost intervention versus the survey control condition; students with high baseline competence-related beliefs reported marginally significantly or significantly lower levels of these three constructs. It is possible that over time, changes to cost caused these students' perceptions of value to change (e.g., if I do not perceive my coursework to be as effortful as I once did, I may believe that it is more worthwhile for that amount of effort). Many EEVT researchers consider cost to impact negatively task value (Eccles-Parsons et al., 1983; Wigfield et al., in press). However, I did not observe the same simple effects when examining the moderator of prior achievement, and many effects were marginally significant. Thus future researchers should explore this possibility more fully.

I also did not find consistent effects of the cost intervention on three outcomes: time spent on homework, discussion section attendance, and enrollment in the next physics course in the semester following the intervention. For the latter two variables, factors external to the study may explain the lack of effects. The course professor did not collect as many data points for discussion section attendance as was originally planned, so many students had only one or two data points available from which to compute an attendance rate. For course enrollment, over 80% of students indicated that the next physics course after Physics 161 was required for their major. It is likely that many students had plans to take the next physics course in the sequence regardless of their motivation for the course. For the variable of homework time, students receiving the cost intervention spent more time *overall* on homework than did students in the survey control condition. However, I did not observe any interactions suggesting that the effects of the cost intervention on this outcome depended on any moderating variables. This effect is inconsistent with the moderation effects just reported, because I would expect that only students for whom the intervention caused them to report lower cost or higher competence-related beliefs might spend more time on homework. Additionally, this main effect did not hold in all models. For all of these effects, results are not clear enough to draw conclusions, and future research will need to explore this possibility further.

Explaining individual difference effects in the utility value intervention. The utility value intervention did not impact utility value consistently overall or in moderation analyses, which was inconsistent with my hypotheses and with prior utility value intervention research (e.g., Gaspard, Dicke, Flunger, Brisson, et al., 2015). There are several reasons the utility value intervention might not have impacted utility value. It is possible that there were implementation issues, such as not having the intervention last long enough, or participation issues, such as students not reading closely the examples from their peers. It is also possible that the intervention materials were not impactful enough: Some versions of the utility value intervention encourage students to summarize the topic about which they make personal connections before making those connections, in order to activate relevant content knowledge (e.g., Harackiewicz et al., 2016;

Hulleman & Harackiewicz, 2009). However, these explanations are not particularly likely. This is because the methods and dosage of the utility value intervention were chosen based on prior research regarding best practices in intervention design and were revised through substantial pilot work. Students wrote essays that were relevant to the topics being discussed, and they reported moderate engagement during the study.

The intervention's failure to impact self-reported utility value was more likely due to a ceiling effect. Students in the present study already thought of physics as very useful. Among the participants, 92.7% were pursing engineering or computer science degrees, which are fields in which knowledge of mechanical physics is very important. Students also reported very high levels of utility value for physics (at Session 1, $M = 5.09$, $SD = 1.24$, on a scale of 1 to 7). Therefore, it is likely that these students did not report additional utility value in physics after receiving the intervention because the intervention did not provide them with any information that could change their already-high perceptions of physics. The analyses of students' open-ended responses regarding whether utility value had changed between Sessions 1 and 2 supported this conclusion. Eighty percent of students wrote in that their utility value had not changed between the two sessions, and almost all of those students wrote that their utility value did not change because it had been high at Session 1 and was still high at Session 2.

The utility value intervention did not show overall impacts on other outcomes, but it showed a few moderation effects suggesting that it may have impacted some students' perceptions of cost, competence-related beliefs, and course outcomes. Like with the cost intervention, the explanations for the intervention's effects on these variables differ according to the moderating variable in question, so I discuss each moderator in turn. It is important to note that I observed patterns of results on motivation that differed from prior research, and these effects were not as numerous or consistent as the effects that I observed on students' motivation in the cost intervention. Thus, it is possible that some of the effects of the utility value intervention on students' cost and competence-related beliefs were due to Type 1 error. I discuss why these effects on motivation may have occurred based on prior research and theory, but I caution readers against drawing strong conclusions regarding these effects until they have been replicated.

In terms of theoretical moderators, students with low baseline competence-related beliefs reported lower emotional cost at Session 3 if they received the utility value intervention versus the survey control condition. This effect may have occurred because students with low baseline competence-related beliefs reported very high emotional cost before beginning the intervention. Students who value a subject highly but have low perceived competence for it often show lower self-worth as a result of this discrepancy, which can have negative affective consequences (Covington, 1992; Harter, 1990). Reading and connecting with the quotations might have reminded students with low baseline competence-related beliefs that their course experiences were similar to the experiences of other students in the course. Although the quotations did not explicitly mention course challenges, it is possible that a large variety of course experiences were costly to students with low prior achievement due to them having such high emotional cost. Having an opportunity to think about how their course experiences were common to others might have helped these students re-consider their perceptions of their course experiences in a more positive light and thus reduce their emotional cost.

Conversely, students with high prior achievement and high baseline competence-related beliefs reported higher emotional and total cost at Sessions 2 and/or 3 if they received the utility value intervention versus the survey control condition. These students had very low initial emotional cost, and so they likely did not think often about course challenges being costly because they were able to perform well despite any challenges. For this reason, students with high baseline competence-related beliefs or high prior achievement likely did not often think often about the potential negative consequences of failing to learn physics. In the utility value intervention, students reflected on how useful their course material was. This could have made the potential negative consequences of *not* learning the material more salient than they had been previously, resulting in an increase in emotional cost among these students.

Students' course outcomes also differed as a function of the two theoretical moderators, such that students with low baseline competence-related beliefs and low prior achievement earned higher exam scores and course grades if they received the utility value intervention versus the survey control condition. Students with high baseline competence-related beliefs and high prior achievement did not differ in their course outcomes by condition. This pattern of effects is consistent with prior researchers' findings that students with low prior competence-related beliefs and low prior achievement often benefit most from utility value interventions (e.g., Canning & Harackiewicz, 2015; Hulleman & Harackiewicz, 2009; Hulleman et al., 2010; 2016). However, it is inconsistent with what one might expect based on the effects of the utility value intervention on motivation.

There are several explanations for why the effects observed on motivation differed from those observed on achievement in this study. First, students' perceptions of cost and course outcomes both might have changed as a function of baseline competence-related beliefs and prior achievement, but the changes were not all statistically significant. Broadly, this explanation makes sense because a large body of research has shown that cost predicts students' course achievement (e.g., Battle & Wigfield, 2003; Conley, 2012; Jiang et al., 2017; Perez et al., 2014). Specifically, I observed significant interactions between receiving the utility value intervention and the moderators of prior achievement and baseline competence-related beliefs on the outcomes of emotional cost, total cost, exam scores, and final course grades. When plotting these interactions, I observed consistent patterns of results: Students with high prior achievement or high baseline competence-related beliefs reported slightly higher cost and earned slightly lower course outcomes, whereas students with low prior achievement or low baseline competence-related beliefs reported slightly lower cost and earned slightly higher course outcomes, if they received the utility value intervention. The simple effects testing for differences between the utility value and control conditions were not all statistically significant, but patterns of results suggested that these moderators impacted similarly students' motivation and their course outcomes.

An alternative explanation for the discrepant effects observed in this study is that other factors impacted the utility value intervention to benefit students' course outcomes besides their perceptions of cost. In particular, the utility value intervention may have changed how low-competence or low-achieving students perceived the utility of their courses in a way that was not captured using a self-report measure of utility value.

Tibbetts et al. (2016) reviewed utility value interventions and noted two explanations for the benefits of these interventions: students engaged more with their course activities and students made stronger personal connections to their course material after receiving an intervention. In the present study, maybe some students thought about the utility of their courses more specifically after receiving the intervention, or their existing perceptions of utility value became more salient to them. This could have caused them to make stronger personal connections or to engage more with their course material, and to earn higher course outcomes, without them necessarily reporting more utility value on questionnaires.

A third explanation for these effects is that the theory underlying how students respond to utility value interventions is not complete. Perhaps students benefit from utility value interventions as a function of mechanisms beyond motivation, such as cognitive engagement that is not related to the utility of their courses. It is possible that more than one of the mechanisms just noted impacted students while they participated in the utility value intervention in this study. As I discuss below, it is critical that researchers explore further the mechanisms by which this intervention impacted students.

In terms of exploratory moderators, students with strong fixed beliefs about intelligence reported higher cost at Sessions 1 and 2 if they received the utility value intervention versus the survey control condition. It is possible that these students reported higher cost because they engaged in social comparisons after reading the quotations in the utility value intervention. Research demonstrates that students with strong fixed beliefs about intelligence are more likely to endorse performance goals for learning (Dweck & Master, 2009). Performance goals emphasize demonstrating one’s competence for learning and often involve comparing one’s ability relative to others (Maehr & Zusho, 2009). Thus students with strong fixed beliefs about intelligence are very aware of social comparison information and rely heavily on it to determine their learning progress and ability. Reading the quotations may have caused fixed-belief students to compare their own use of physics to the ways in which the students in the quotations used physics. These comparisons could cause an increase to frustration or anxiety for fixed-belief students, increasing their perceptions of cost. Conversely, students with strong malleable beliefs about intelligence reported no differences in cost, but they reported higher competence-related beliefs at Sessions 1 and 2, if they received the utility value intervention versus the survey control condition. These students likely read the quotations from other students and were reminded that that they too could apply physics in their lives; this could have increased their perceptions of their own competence in physics.

The utility value intervention did not cause differences in students' course outcomes as a function of their beliefs about the malleability of intelligence. However, moderated mediation analyses suggested that for students with strong fixed and strong malleable beliefs about intelligence, changes to cost did explain some of the relations between receiving the utility value intervention (relative to the survey control condition) and changes to students' final course grades. Perhaps students' course outcomes did change slightly, but these differences were not strong enough to be statistically significant. It would be interesting to explore whether students benefit or show lower course outcomes as a function of their beliefs about intelligence in other educational contexts, and what the mechanisms of those changes might be.

Female students reported lower emotional cost at Session 3 if they received the utility value intervention versus the survey control condition. Similar to the cost intervention, this effect may have occurred because female students experienced less belonging uncertainty after receiving the utility value intervention. The intervention asked female students to make connections to quotations from other students who had used physics in their careers and daily lives. The process of identifying with others in the course and perceiving one's course experiences as common could reduce female students' perceptions of belonging uncertainty and hence their emotional cost. This conclusion is consistent with points made by Harackiewicz et al. (2016), who argued that students at the highest risk for belonging uncertainty benefitted the most from a utility value intervention as a result of having an opportunity to connect personally with information from the course. Female students also perceived their competence-related beliefs to be higher at Session 2 if they received the intervention. One explanation for this effect is that a reduction in belonging uncertainty helped female students separate their perceptions of course challenges from their perceptions of their ability, similar to how the cost intervention impacted these students. Finally, female students earned higher final course grades and exam scores if they received the utility value intervention. It is possible that changes in female students' course outcomes were due to changes in their motivation or due to the other mechanisms I discussed above, such as increased engagement.

Similar to the cost intervention, students with high uncertainty about belonging in physics earned higher course outcomes if they received the utility value intervention versus the survey control condition. However, they did not show differences in motivation. Also similar to the cost intervention, the utility value intervention did not impact students' attainment or intrinsic value, discussion section attendance, time spent on homework assignments, or enrollment in the next physics course. Some prior researchers have shown that utility value interventions impacted attainment value or intrinsic value, or the related construct of interest (e.g., Gaspard, Dicke, Flunger, Brisson, et al., 2015; Hulleman et al., 2010; Hulleman & Harackiewicz, 2009). However, because utility value did not change in this study I would not have expected attainment value and intrinsic value to change. The same concerns with the other variables that I discussed in the cost intervention section apply to the utility value intervention, so future research will need to study those variables with better measures in order to study them fully.

Educational Implications of the Cost and Utility Value Interventions in College Physics

My overall recommendation regarding how to use these results to impact educational practice is: do so cautiously. In this study, the cost intervention showed complex patterns of results that included some positive effects but also some undermining ones. I do not recommend that this intervention be implemented broadly across introductory college STEM courses. Instead, it has much potential to impact educational practice in a meaningful way after researchers have explored more fully potential contextual factors that limit its effects and how to reduce potential undermining effects that may occur for certain students.

There are two ways in which the cost intervention may be informative for current educational practice. First, I described above that the context of the college physics course might have contributed to some of the undermining effects observed in this study. It is possible that the cost intervention might show overall positive results if implemented in an educational context where students have lower overall value for learning. Second, this intervention could be useful for educational practitioners if students clearly fit the profiles of those who benefitted from the cost intervention in the present study (i.e., they are female, have low initial competence-related beliefs and/or prior achievement, or have a fixed belief about intelligence). These students are likely to benefit from a cost-targeting intervention across educational settings and this intervention could be a viable way to improve their STEM motivation and course outcomes at a low cost.

Many prior researchers have found that utility value interventions show positive effects and have concluded that researchers should explore whether the intervention's effects generalize across more settings (Hulleman & Harackiewicz, 2009; Hulleman et al., 2010; Tibbetts et al., 2016; Yeager & Walton, 2011). The results of the present study demonstrated that some students did benefit from the utility value intervention in college physics, and that it may be a useful way to improve the outcomes of students with low competence-related beliefs, low prior achievement, or who are female. However, this intervention did not show universally positive effects on students' motivation. I recommend that researchers consider carefully the potential that the intervention may produce moderated and/or undermining effects if they plan to implement a utility value intervention, especially if the intervention is being implemented in an educational context where many students might already have high value for the course.

One important factor to consider when interpreting the educational implications of both interventions is the effect sizes observed in this study. The effect sizes for most interactions related to the cost and utility value interventions were small to moderate. The effect sizes estimated for students at specific levels of the moderating variables (in most cases, one standard deviation above and below the mean) ranged from no effect to large effects, depending on the interaction in question. However, the majority of these specific effects were small to moderate. Results suggest that cost and utility value interventions are not likely to change students' achievement in extreme ways. However, they do produce meaningful changes, especially considering the small amount of time and low effort required to implement these two interventions. Even moderate changes in course outcomes can be critical for students. For example, a one-half standard deviation change in students' course grades (i.e., a moderate effect) would be approximately five points. A five-point increase in a grade would change students' GPAs. Thus these interventions' effects are large enough to be impactful for educational practice.

Limitations and Extensions of the Present Study

This work provides important insights regarding the effects of cost and utility value interventions with college physics students, but much more work remains to be done before understanding the implications of this line of work fully.

A major limitation of the present study is that my sample was smaller than anticipated and some of my analyses were underpowered, in particular the moderated mediation analyses and with the analyses comparing students in the summary condition to students in the survey control condition. Future researchers should test similar interventions with a larger sample of students in order to determine conclusively whether and how each moderating variable impacted students' motivation and course outcomes. A larger sample also is critical to understand how multiple moderating variables could work together to impact students' course outcomes. For example, it is possible that low-achieving female students respond differently to a cost or utility value intervention than do high-achieving female students. With a larger sample, I could test for three-way interactions or conduct person-centered analyses. I could determine how often different moderating variables co-occurred among students and how those patterns might have interacted with the intervention's impacts on students' motivation and course outcomes.

A second limitation of this study is that many students in the cost intervention condition did not write essays that were directly about cost. It is likely that most students thought about cost while completing the intervention activities and while writing their essays. However, it is possible that the cost intervention showed some moderated or undermining effects because students engaged with the intervention prompts in unexpected ways, and not because they thought about their cost experiences differently. I discuss at the end of this section my plans to conduct textual analysis of students' responses in the cost intervention in order to assess these possibilities more fully. Future interventions targeting cost should include a more direct prompt with more scaffolding to help students re-attribute their cost experiences more directly.

A third limitation of this study is that students were enrolled in only one college physics course. This course was intended to be representative of all introductory college STEM courses, but as noted above, students showed particularly high value for learning. Thus it likely did not represent students in all introductory college STEM courses. Future researchers should explore the effects of cost or utility value interventions using students enrolled in a broader variety of STEM courses or in other physics courses that serve a broader variety of STEM students.

A fourth limitation is that I did not evaluate fully effects over time of the interventions. I interpreted results with attention to whether effects appeared at three time points. However, I did not evaluate in the same analysis whether the effects of either intervention as a function of moderating variables faded or strengthened over time.

Additionally, I measured impacts of the interventions only in the semester following the study. Although the interventions were designed to target students' perceptions of physics in a specific course, it is possible that they would also impact students' motivation or behavior in physics over a longer period of time. Perhaps effects even would differ for some students over time. For example, students with strong fixed beliefs about intelligence may report higher motivation for physics over time after they received the cost or utility value interventions, even though they reported higher cost at first. This might occur because those students were able to resolve any initial cognitive dissonance they experienced when interacting with intervention materials.

Finally, I could have measured additional variables or measured variables more thoroughly in this study. I observed changes to some students' course outcomes that did not correspond to changes in these students' motivation. Students with high belonging uncertainty showed this pattern of results in both intervention conditions, and students with high prior achievement and high baseline competence-related beliefs showed this pattern in the utility value intervention condition. As I noted above, the measure of cost used in this study did not assess specifically students' emotional cost relevant to uncertainty about belonging in the course, and the measure of belonging uncertainty was not worded in a particularly clear way. Additionally, I did not measure completely the ways in which students might have considered the utility of their course material as a result of receiving the utility value interventions. Researchers should use more comprehensive measures of these constructs in the future, in order to understand precisely how some students benefitted from receiving cost or utility value interventions.

There are four major ways in which future researchers can expand on the work presented in this dissertation. The most important next step in this research program is to understand better the mechanisms by which the cost and utility value interventions impacted different students in the present study. Although beyond the scope of this dissertation, I plan to conduct supplementary textual analysis of students' responses to the essays they produced in the two intervention conditions. This type of analysis will address in more detail whether students responded to intervention prompts by writing about the motivational variables that were targeted by the interventions, or whether they primarily engaged with other cognitive or motivational processes that might have impacted how they responded.

For the utility value intervention, I plan to code students' essays for the strength of the personal and specific connections they made to their course material (Harackiewicz et al., 2016; Hulleman et al., 2010). I also will assess what type of utility value students wrote about in their essays (e.g., personal hobbies or interests, future careers, helping others). Finally, I will assess the extent to which students referenced topics related to their competence-related beliefs, attainment value, intrinsic value, perceived cost, or course engagement in the utility value essays. For the cost intervention, it would be useful to code the extent to which students articulated personal and specific experiences of cost in their essays. I also will code what types of challenges students wrote about, and the extent to which students wrote that those challenges were surmountable. I will code whether students articulated beliefs that cost might be positive or that the challenges they experienced might benefit them in any way, because perhaps some students did not perceive their course challenges as negative. Finally, I will assess whether students referenced topics related to their attainment value, utility value, intrinsic value, competence-related beliefs, and/or course engagement in their cost essays, similar to the utility value intervention coding.

A second critical extension of the present study is to explore whether it is possible to eliminate undermining effects in future interventions. It is possible to make students' beliefs about intelligence more malleable using interventions (e.g., Blackwell, Trzesniewski, & Dweck, 2007; Good, Aronson, & Inzlicht, 2003; Paunesku et al., 2016). By adding support for malleable intelligence beliefs before providing the cost or utility value intervention to college physics students, this could allow more students to benefit from the interventions and potentially reduce undermining effects among students with strong beliefs that intelligence is fixed. It also may be possible to show overall positive effects of both interventions if a researcher were to implement them in a different educational context, possibly one in which students have overall lower value for their courses. More students in these educational contexts might benefit from thinking about how course material relates to their lives or from re-framing their experiences of cost.

Methodological changes also might ameliorate some of the undermining intervention effects observed in this study. In particular, many students reported higher cost or lower competence-related beliefs because they engaged in negative social comparisons with students from the quotations. If researchers did not show students quotations from other students, they may avoid some undermining effects. Also, some students might not readily believe the idea that their cost experiences would ever decrease during a semester. Rather, they might perceive that their coursework would only get harder over time. Researchers might want to include information in the intervention about *how* students can change their perceptions of cost rather than simply saying that cost will change. Interventions targeting cost could include information about time management strategies that help students manage better their coursework. Interventions also could include motivation regulation strategies, such as tips for resisting distraction or reducing anxiety, to help students address future cost experiences. Alternatively, interventions could include information about the value of cost experiences or why those experiences might actually be beneficial to students.

A third extension of the present study is to try and better understand individual differences in students' perceptions of cost. Few researchers have written about these differences to date and they would be helpful to understand the differential impacts of the cost intervention on students as a function of moderating variables. Researchers should conduct more quantitative work to understand whether students of different genders, or students with different beliefs about the malleability of intelligence, have stronger perceptions of the dimensions of cost in college physics. Researchers also may want to conduct qualitative work to ascertain more directly how different students think about their cost experiences in physics and what experiences are costly to them.

A final extension of this work is that researchers may want to explore new approaches to EEVT-based interventions. In this study, both the cost and utility value interventions impacted some students' competence-related beliefs. It may be effective for researchers to target students' competence-related beliefs in college physics courses directly in order to improve students' STEM achievement and course taking. Researchers have designed interventions that address self-efficacy and other competence-related beliefs in STEM courses, but the studies that have targeted competence-related beliefs specifically in college have produced mixed results. Thus this field of research needs to be explored further (see Rosenzweig & Wigfield, 2016, for review). An additional way in which researchers could try to impact students is to allow them to choose among different EEVT-based interventions, or assign them to an intervention from which they are likely to benefit based on a pre-test of their motivation, instead of providing the same intervention to all students. Some research outside of EEVT suggests that this approach to motivation interventions is beneficial (Song & Keller, 2001).

Conclusion

Students' experiences in introductory college STEM courses are critical attrition points in their pursuit of STEM-related careers. In this study, I developed and implemented two interventions, a cost intervention and a utility value intervention, to try and improve students' motivation for an introductory college physics course as well as their academic outcomes. The utility value intervention was an adaptation of an intervention that has improved STEM course outcomes in many prior settings. However, it had never been tested in college physics. The cost intervention was novel, but it was adapted from prior interventions designed to mitigate students' uncertainty about belonging in college. It encouraged students to think about their course challenges as more short-term and common than they originally believed. I compared results from both conditions to results from a condition in which students completed only surveys.

Neither intervention showed overall effects on students' course motivation or achievement compared to the survey control condition. However, both interventions showed consistent moderation effects suggesting that they impacted some students' cost, competence-related beliefs, and/or course outcomes positively but impacted some outcomes for other students negatively. The cost intervention caused students who were female and who had low prior achievement or baseline competence-related beliefs in physics to report lower cost and/or higher competence-related beliefs and to earn higher course outcomes. However, it caused students with strong fixed beliefs about intelligence to report higher cost and lower competence-related beliefs, and to earn lower course outcomes. The utility value intervention caused students to earn higher course outcomes if they were female, had low baseline competence-related beliefs in physics, or had low prior physics achievement. Female students and those with low baseline competence-related beliefs also reported lower cost as a result of receiving the intervention. Less consistent patterns of results demonstrated that the utility value intervention may have caused students to report higher cost if they had high baseline competence-related beliefs, high prior achievement, or strong fixed intelligence beliefs.

Results suggest that both cost and utility value interventions can impact students' motivation and outcomes in college physics. However, effects were not all positive. The cost intervention seems to be useful for improving at-risk students' motivation and achievement, but researchers should conduct much future work to understand more fully potential undermining effects for other students and whether those effects can be eliminated. The utility value intervention benefitted the same types of students who have benefitted from past utility value interventions. However, in this educational context the utility value intervention did not impact students' self-reported utility value. Thus researchers should explore in more depth the precise mechanisms by which utility value interventions cause changes to different students' course outcomes. They also should address further the potential for these interventions to undermine some students' motivation. Overall, the results of this study provide interesting and novel insights regarding how to develop EEVT-based interventions and the consequences of these interventions for different students in introductory college physics.

Appendix A: Text of Utility Value Intervention Condition

Today, we want to know your thoughts regarding how physics relates to your life. A lot of times, students only see physics as useful because they want to get a good grade in it, or because they need to know it for a test. But the content you learn in physics also can relate to your life in different ways.

We interviewed students who took Physics 161 last year and asked them to think about how physics was related to their lives. For example, we asked them to think about how physics related to their hobbies, careers, or activities they did.

We want to share what some of the students had to say with you. We want to know what you think about these statements. Your feedback will be helpful so that we can better understand students' attitudes about physics and help students who take this class in the future.

Here are some of the students' thoughts:

"I want to be an engineer so I want a good grade in physics to get a job. But what I'm actually learning in class will also be helpful for doing my job later. A lot of engineers improve the design of different machines using equations or programs from electrical or mechanical physics. I could end up in a job where I need to know how to reduce a machine's friction, or change its structure so it uses less energy. The machines then might last longer or cost less, and I would be able to help do that. That process is cool to me, and I also like that the job could help the environment. More efficient machines waste less, and if I used physics to improve machines like windmills or solar panels that can make a difference for renewable energy."

- Alex, age 19, major: *Mechanical Engineering*

"I can connect my life to physics if I think about it, although to be honest, I mostly think of physics as a hurdle to get through so I can get into a good grad school. But there are these scenes in action movies where people survive a far jump, or a really high fall. Sometimes I ignore whether or not the scene's realistic, because it's fun to watch. But other times I can't help but wonder whether what I'm watching could actually happen in the real world. I have thought about physics when doing that. For instance it isn't believable if people don't roll when they land a long jump or fall in movies, because you have to do that to minimize force on any one part of your body. And if cars jump over a big gap, they would have to have a lot of weight in the back of the car to reduce torque, or the car would actually nose dive straight into the ground."

- Jenna, age 20, major: *Chemical Engineering*

"I am interested in computer programming and I know that lots of equations from Physics 161 are embedded into computer programs and video games. In video games where people walk or move, programmers need to use the equations for projectile motion. Otherwise it doesn't look or feel realistic. Even if it's not for a game, lots of computer programming jobs ask people to do simulations for buildings, machines, roller coasters, or other things. That would require using equations about motion, friction, and force."

- Nick, age 19, major: *Computer Science*

"I got a really low blood pressure reading once at my doctor's office, and physics helped me understand why it happened. A really low reading sometimes means something is wrong with you. But I was sitting with my legs dangling. The doctor said that I didn't have to worry because body position can change the force of blood and create a weird reading. Normal forces push up on my feet if they touch the ground. Without that, the blood pressure up by my arm might be lower because gravity pushed that blood into my legs. Obviously I don't use it for everything, but I think physics can be helpful for understanding body processes like this and why I didn't need to worry in that situation."

- Yi, age 20, major: *Biochemistry*

We'd like to know what you think of each example. Please answer the following questions about each student quote.

1. How much is the quote similar to your own experiences in physics?
2. How interesting is the quote to read?

Next, we'd like you to rank the quotes according to which you liked the most. You can click on each quote and move it up and down on the list. The top quote should be the one that you liked the most, and the bottom quote should be the one that you liked the least.

For the quote you ranked as the one you liked most, what about it made you rank it first?

Now that you've had a chance to read what other students had to say, what would you tell another student about how what you're learning in physics relates to your life? Please write 3-5 sentences.

Appendix B: Text of Cost Intervention Condition

We want to know your thoughts regarding the challenges you might experience in physics class and how you deal with them.

We interviewed students who took Physics 161 last year and asked them to think about a challenge they experienced in class and what they did to address it. We asked them to think about things like the effort they put into class or other classes, or negative emotional experiences that they had. Many students reported that there were challenges that made it hard for them to do their best in physics. But they also said many things that were challenging to them at first weren't as challenging later in the semester.

We want to share what some of the students had to say with you. We want to know what you think about these statements. Your feedback will be helpful so that we can better understand students' attitudes about physics and help students who take this class in the future.

Here are some of the students' thoughts.

"I thought the MasteringPhysics homework was challenging because there are so many questions. It felt like way too much work to complete each assignment at first. The workload for homework never really went down over the semester, but I did think the effort seemed more manageable as the weeks went on. It's because I learned how much time and effort it took to do an average homework. Even though the assignments took a similar amount of work every week, it seemed less bad because I was expecting it and didn't get caught off guard by the amount of effort required."
- Alex, age 19, major: *Mechanical Engineering*

"For me it was hard to juggle the work in physics with my other classes. A bunch of times I had assignments, quizzes, and tests due for four classes in the same week. It was hard to commit to working on physics. Something that helped was that my good friend was in the same situation. That made me think this is a normal part of college that most people go through. Plus, sometimes thinking about all the courses together was super overwhelming at the beginning of those busy weeks. But then I'd work on everything one day at a time, and it was tough, but on any given day it wasn't so overwhelming that I couldn't handle it."
- Jenna, age 20, major: *Chemical Engineering*

"I got really frustrated after the first physics exam. The material from lectures and homework didn't match the questions on the exam, and I studied really hard but it didn't pay off as much as I thought in my performance. I think anyone would get frustrated by that situation. By the end of the semester I really wasn't that frustrated about the first exam anymore. A few days after I got my grade, I could put the situation in perspective a lot better than right when I first got the exam back. I realized that this was just one part of the course, and I could study for the other two exams differently. I was less actively frustrated after that."

- Nick, age 19, major: *Computer Science*

"The biggest challenge my friends and I had in physics was studying for the exams. On the first exam it seemed like there was so much material to learn, and we were overwhelmed talking about whether we would be able to learn everything in time. On the other two exams we would remind ourselves that the week or two before any exam is especially busy, but that's not what the class is like every day of the semester. Obviously we still had a ton to do for other classes and for physics between exams, so we couldn't just sit around. But after an exam things would go back to a "normal" workload that seemed more manageable. Thinking about that made studying less overwhelming."

- Yi, age 20, major: *Biochemistry*

We'd like to know what you think of each example. Please answer the following questions about each student quote.

1. How much is this quote similar to your own experiences?
2. How interesting is this quote to read?

Next, we'd like you to rank the quotes according to which you liked the most. You can click on each quote and move it up and down on the list. The top quote should be the one that you liked the most, and the bottom quote should be the one that you liked the least.

For the quote you ranked as the one you liked most, what about it made you rank it first?

Now that you've had a chance to read what other students had to say, what would you tell another student about a challenge you experienced in physics and how you addressed it? Please write 3-5 sentences.

Appendix C: Text of Control Summary Condition

Learning physics can be fun, but it can also be challenging. We have spent time thinking about what strategies help students learn better in physics classes, and today we want to share one of those strategies with you.

One way to improve your learning in physics is to actively review topics that you have already learned in class. For example, you might review important vocabulary or concepts from one lesson before starting your assignment.

In today's activity we will give you a chance to review some of your recent Physics course material. We'd like your feedback on this type of activity because it's possible that we could use it to help physics students in the future.

Take a moment to think about one topic or concept you've been learning about in your Physics class recently. Try to think about a topic or concept that has been challenging, or one that has tripped you up a bit.

In the space below, type a 3 - 4 sentence summary of the concept, focusing on *defining the concept in your own words.*

Next, describe the calculations or formulas required to solve problems related to the concept you chose. Try to type a brief summary (3 - 4 sentences) describing those calculations or formulas.

Finally, think of a problem involving the topic or concept you selected above, that you might be asked to solve on a homework assignment or exam. Write the problem and the correct answer, as well as a brief description of how to solve the problem, here.

What did you like about this activity? Is there anything you would have changed?

Appendix D: Text of Refresher Activity

Today, we would like you to think about the activity you completed a few weeks previously.

What do you remember about that activity?

[Utility value intervention condition:]

Just to remind you, last time we asked you to think about how physics related to your life. Here are some of the examples you read last time:

"I want to be an engineer so I want a good grade in physics to get a job. But what I'm actually learning in class will also be helpful for doing my job later. A lot of engineers improve the design of different machines using equations or programs from electrical or mechanical physics. I could end up in a job where I need to know how to reduce a machine's friction, or change its structure so it uses less energy. The machines then might last longer or cost less, and I would be able to help do that. That process is cool to me, and I also like that the job could help the environment. More efficient machines waste less, and if I used physics to improve machines like windmills or solar panels that can make a difference for renewable energy."

- Alex, age 19, major: *Mechanical Engineering*

"I can connect my life to physics if I think about it, although to be honest, I mostly think of physics as a hurdle to get through so I can get into a good grad school. But there are these scenes in action movies where people survive a far jump, or a really high fall. Sometimes I ignore whether or not the scene's realistic, because it's fun to watch. But other times I can't help but wonder whether what I'm watching could actually happen in the real world. I have thought about physics when doing that. For instance it isn't believable if people don't roll when they land a long jump or fall in movies, because you have to do that to minimize force on any one part of your body. And if cars jump over a big gap, they would have to have a lot of weight in the back of the car to reduce torque, or the car would actually nose dive straight into the ground."

- Jenna, age 20, major: *Chemical Engineering*

Has your thinking about how physics relates to your life changed since you completed the activity? Please explain why or why not.

During the last activity, we asked you to write to another student about how what you're learning in physics relates to your life. Today we would like you to write about the same topic again. We want your feedback a second time because as a current student, you are the best source of information about what students experience while taking Physics 161. We would like to know how you think different parts of your course material relate to your life so we can use it to help students who take Physics 161 during later semesters.

In the box below, please write 3-5 sentences about how what you're learning in your physics course relates to your life. You can choose to write more about the same topic that you discussed during the first activity, or you can write about a different topic.

[Cost intervention condition:]

Just to remind you, last time we asked you to think about overcoming challenges you experienced in physics.

Have the challenges you experienced in your physics course changed since you completed the last activity? Please explain why or why not?

We would like you to read some more examples from students about overcoming challenges in their physics courses. These examples are more relevant to experiences later in the semester than the examples that you read last time. Again, we want to know your feedback on these students' thoughts so we can understand how to help other physics students in the future.

"In the beginning the physics workload was manageable and some of the material was even a review from high school. But after a while, juggling work for my other courses and physics made me feel burned out. There was one week when I got really overwhelmed thinking about all the work required in all my classes that week. Something that helped was telling myself that it was just one week out of the entire semester. It's a lot of effort but the very busy weeks don't last forever. Everyone has a few times like that during every semester. Reminding myself of all that helped me put the workload in perspective. It made me less overwhelmed during other busy weeks."

-Anna, 20, Computer Science

"Over time I had to put more and more effort into physics homework. That's because the topics got more advanced and the homework questions were really different than the questions we went over in the lectures. This made me frustrated at first because I figured the homework would require less work if I put more effort in during class. I know a lot of other people who put in a lot of work on the homework, and it helped to know I wasn't alone. But what helped more was that I managed my time better and got better about using outside resources as the semester went on. The homework was never different, but after a while I didn't react strongly to it like I did at first because I knew what it would be like. I didn't get as frustrated about it towards the end of the semester."

-Dan, 19, Bioengineering

Are these statements similar to your own experiences? Why or why not?

Which of these statements do you like more? Why?

During the last activity, we asked you to write to another student about a challenge you experienced in physics and how you overcame it. Today we would like you to write about the same topic again. We want your feedback a second time because as a current student, you are the best source of information about what students experience while taking Physics 161. We would like to know how you think about course challenges so we can use it to help students who take Physics 161 during later semesters.

In the box below, please write 3-5 sentences explaining how you overcame a challenge in your physics course. You can choose to write more about the same topic that you discussed during the first activity, or you can write about a different topic.

[summary condition:]

Just to remind you, last time we asked you to summarize a topic from your physics class.

Do you remember which topic you summarized?

> [if yes]
> Which topic did you choose to summarize, and why?
>
> Has your thinking about the topic changed in any way since the last session? Please explain why or why not.
>
> [if no]
> What is a topic that you remember learning about in class anytime before your first physics exam this semester?
>
> Did your thinking about that topic change over the course of the semester? Please explain why or why not.

Appendix E: Measures

Category	Measure	Details
Pre-intervention motivation	Baseline competence-related beliefs in physics	Average of five items from Eccles and Wigfield (1995): 1. How good in Physics are you? (*Not at all good, Very good*) 2. If you were to list all the students in your class from the worst to the best in Physics, where would you put yourself? (*One of the worst, One of the best*) 3. Compared to other subjects, how good are you in Physics? (*Not at all good, Very good*) 4. How well do you expect to do in Physics this year? (*Not at all good, Very good*) 5. How good would you be at learning something new in Physics? (*Not at all good, Very good*) Response scale: 7 point scale (anchors noted above)
	Belonging uncertainty in physics	Average of two items from Harackiewicz et al. (2016): 1. When something bad happens, I feel that maybe I don't belong at the University of Maryland. 2. Sometimes I feel that I belong at the University of Maryland, and sometimes I feel that I don't belong at the University of Maryland. Response scale: 6 point scale (*Strongly Disagree*, *Strongly Agree*)
	Beliefs about the malleability of intelligence	Average of three items from Dweck, Chiu, & Hong (1995): 1. You have a certain amount of intelligence and you really can't do much to change it. 2. Your intelligence is something about

		you that you can't change very much. 3. You can learn new things, but you can't really change your basic intelligence. Response scale: 6 point scale (*Strongly Disagree*, *Strongly Agree*)
Pre-intervention achievement	Quiz scores	Average proportion correct across the first two quizzes in Physics 161
Post-intervention motivation	Utility value for physics	Average score on 2 items from Eccles and Wigfield (1995): 1. In general, how useful is what you learn in Physics? 2. Compared to most of your other activities, how useful is what you learn in Physics? Response scale: 7 point scale (*Not at all useful, Very useful*)
	Perceived cost in physics	Average score for all 15 items and average score for each sub-scale (4-6 items each) from Flake et al. (2015): Task Effort Cost: 1. This class demands too much of my time. 2. I have to put too much energy into this class. 3. This class takes up too much time. 4. This class is too much work. 5. This class requires too much effort. Outside Effort Cost: 1. I have so many other commitments that I can't put forth the effort needed for this class. 2. Because of all the other demands on my time, I don't have enough time for this class. 3. I have so many other responsibilities that I am unable to put in the effort that is necessary for this class. 4. Because of other things that I do, I don't have time to put into this class.

		Emotional Cost: 1. I worry too much about this class. 2. This class is too exhausting. 3. This class is emotionally draining. 4. This class is too frustrating. 5. This class is too stressful. 6. This class makes me feel too anxious. Response scale: 7 point scale (*Strongly disagree, Strongly agree*)
	Competence-related beliefs in physics	Same items as used above
	Intrinsic value for physics	Average score on 2 items from Eccles and Wigfield (1995): 1. In general, I find working on Physics assignments (*Very boring, Very interesting*) 2. How much do you like doing Physics? *(Not at all, Very much*) Response scale: 7 point scale
	Attainment value for physics	Average score on 2 items from Eccles and Wigfield (1995): 1. For me, being good in Physics is … 2. Compared to most of your other activities, how important is it for you to be good at Physics? Response scale: 7 point scale (*Not at all important, Very important*)
Post-intervention achievement	Course grade	Overall percent grade earned in Physics 161 during the Fall, 2016 semester
	Quiz scores	Average proportion correct across Quizzes 5 – 8 given in Physics 161
	Exam scores	Average proportion correct across exams 2 and 3 in Physics 161; Proportion correct on exam 2 and exam 3 given in Physics 161
Post-	Discussion section	Average rate of attendance in discussion

intervention course participation	attendance	sections (out of 1-4 randomly selected discussion section sessions) in Physics 161
	Homework time	Average amount of time students spent logged into the MasteringPhysics homework website across the final six homework assignments in the course
Post-intervention course-taking patterns	Enrollment in next physics course	Whether or not students enrolled in the next course in the physics sequence following Physics 161 during the Spring, 2017 semester From university registrar
	Number of STEM courses taken in Spring 2017	Number of STEM courses in which students enrolled during the Spring, 2017 semester From university registrar

Appendix F: Output for All Moderation Analyses using the Survey Control Condition as a Reference Group

Output for Non-Focal Motivational Outcomes (RQ 2): Moderating Variable of Baseline Competence-Related Beliefs

	Utility Value		Effort Cost		Outside Effort Cost		Emotional Cost		Total Cost	
	B	S.E.	B	S.E.	B	S.E.	B	S.E.	B	S.E.
Session 1										
Intercept	5.20	0.23	3.13	0.22	3.01	0.24	3.19	0.24	3.12	0.21
Cost Intervention (Cost)	-0.14	0.28	0.20	0.27	0.46	0.30	0.26	0.30	0.29	0.26
Utility Value Intervention (UV)	-0.10	0.27	0.18	0.26	0.29	0.30	0.15	0.29	0.19	0.25
Summary (Sum)	-0.22	0.33	0.39	0.32	0.80*	0.35	0.34	0.35	0.49	0.31
Competence-Rel. Beliefs (CB)	0.58*	0.26	-1.03**	0.25	-0.94**	0.28	-1.52**	0.28	-1.18**	0.24
Cost x CB	-0.01	0.30	0.29	0.29	0.50	0.33	0.69*	0.32	0.54$^+$	0.28
UV x CB	0.08	0.30	0.22	0.29	0.17	0.33	0.46	0.32	0.29	0.28
Sum x CB	-0.02	0.34	0.63*	0.32	0.22	0.36	0.79*	0.36	0.56$^+$	0.31
Model R^2	.24**		.37**		.27**		.43**		.41**	
Session 2										
Intercept	4.85	0.26	3.14	0.23	3.08	0.25	3.16	0.24	3.13	0.22
Cost	0.18	0.32	-0.02	0.28	-0.05	0.32	-0.17	0.30	-0.09	0.28
UV	0.35	0.31	0.12	0.27	0.17	0.31	0.12	0.29	0.13	0.27
Sum	-0.22	0.42	0.01	0.37	0.50	0.41	0.35	0.39	0.28	0.36
CB	0.93**	0.29	-1.05**	0.26	-1.00**	0.29	-1.27**	0.28	-1.13**	0.26
Cost x CB	-0.53	0.35	0.53$^+$	0.31	0.47	0.35	0.51	0.33	0.50	0.31
UV x CB	-0.54	0.34	0.40	0.30	0.43	0.34	0.46	0.32	0.43	0.30
Sum x CB	0.03	0.44	0.25	0.39	-0.03	0.44	0.17	0.42	0.15	0.39
Model R^2	.20**		.31**		.25**		.39**		.35**	
Session 3										
Intercept	4.96	0.25	3.12	0.22	3.13	0.25	3.34	0.21	3.21	0.20
Cost	-0.07	0.31	0.30	0.27	0.26	0.31	-0.08	0.26	0.14	0.25
UV	0.17	0.31	0.47$^+$	0.27	0.46	0.31	0.11	0.26	0.32	0.25
Sum	0.32	0.40	1.02**	0.34	0.97*	0.40	0.87*	0.34	0.95**	0.32
CB	0.92**	0.30	-0.93**	0.26	-1.04**	0.30	-1.59**	0.26	-1.22**	0.25
Cost x CB	-0.88*	0.35	0.29	0.30	0.65$^+$	0.35	0.74*	0.30	0.57*	0.29
UV x CB	-0.37	0.35	0.48	0.30	0.66$^+$	0.35	0.97**	0.30	0.73*	0.28
Sum x CB	-0.38	.44	0.22	0.37	0.47	0.43	0.71$^+$	0.37	0.48	0.35
Model R^2	.18**		.29**		.19**		.45**		.36**	

Note: ** $p < .01$; * $p < .05$; $^+$ $p < 0.10$. All intercepts are significant in the models at $p < .05$.

Output for Non-Focal Motivational Outcomes (RQ 2): Moderating Variable of Baseline Competence-Related Beliefs

	Competence-Related Beliefs		Intrinsic Value		Attainment Value	
	B	S.E.	B	S.E.	B	S.E.
Session 1						
Intercept	4.86	0.09	4.17	0.22	5.42	0.24
Cost Intervention (Cost)	-0.05	0.11	0.19	0.27	-0.12	0.29
Utility Value Intervention (UV)	0.00	0.11	0.06	0.27	0.13	0.29
Summary (Sum)	-0.12	0.13	0.06	0.32	-0.10	0.35
Competence-Rel. Beliefs (CB)	1.24**	0.10	1.15**	0.25	0.14	0.28
Cost x CB	-0.05	0.12	-0.56	0.30^{+}	0.39	0.32
UV x CB	-0.10	0.12	-0.39	0.30	0.07	0.32
Sum x CB	0.04	0.13	-0.31	0.33	-0.01	0.36
Model R^2	.89**		.36**		$.09^{+}$	
Session 2						
Intercept	4.81	0.16	4.31	0.26	5.01	0.27
Cost	0.23	0.20	0.11	0.33	0.36	0.34
UV	0.23	0.19	-0.17	0.32	0.50	0.33
Sum	0.00	0.26	-0.15	0.43	0.75^{+}	0.44
CB	1.30**	0.18	1.10**	0.30	0.61^{+}	0.31
Cost x CB	-0.27	0.22	-0.85*	0.36	-0.39	0.37
UV x CB	-0.40^{+}	0.21	-0.48	0.35	-0.44	0.36
Sum x CB	-0.08	0.27	-0.20	0.46	-0.55	0.47
Model R^2	.67**		.23**		.08	
Session 3						
Intercept	5.00	0.17	4.26	0.27	5.16	0.26
Cost	-0.13	0.21	-0.10	0.33	-0.03	0.32
UV	-0.10	0.20	-0.10	0.33	0.26	0.32
Sum	-0.37	0.26	0.04	0.42	0.33	0.42
CB	1.25**	0.20	1.12**	0.32	0.86**	0.32
Cost x CB	-0.38	0.23	-0.98*	0.37	-0.92*	0.37
UV x CB	-0.34	0.23	-0.36	0.37	-0.46	0.37
Sum x CB	-0.02	0.29	-0.38	0.46	-0.66	0.45
Model R^2	.61**		.23**		.11*	

Note: ** $p < .01$; * $p < .05$; $^{+}$ $p < 0.10$. All intercepts are significant in the models at $p < .05$.

Output for Focal Motivational Outcomes (RQ 1): Moderating Variable of Prior Achievement

	Utility Value		Effort Cost		Outside Effort Cost		Emotional Cost		Total Cost	
	B	S.E.	B	S.E.	B	S.E.	B	S.E.	B	S.E.
Session 1										
Intercept	5.15	0.26	3.26	0.25	3.03	0.27	3.28	0.26	3.20	0.23
Cost Intervention (Cost)	-0.15	0.32	0.11	0.30	0.40	0.33	0.13	0.32	0.15	0.28
Utility Value Intervention (UV)	-0.05	0.32	0.06	0.30	0.28	0.33	0.06	0.32	0.09	0.28
Summary (Sum)	0.001	0.40	0.17	0.39	0.84*	0.41	0.32	0.41	0.39	0.36
Prior Achievement (Ach)	0.41$^+$	0.24	-0.78**	0.23	-0.61*	0.25	-1.26**	0.25	-0.92**	0.21
Cost x Ach	-0.39	0.30	0.37	0.28	0.15	0.30	0.64*	0.30	0.41	0.26
UV x Ach	-0.06	0.31	0.11	0.29	-0.05	0.32	0.39	0.31	0.15	0.27
Sum x Ach	-0.17	0.42	0.54	0.40	-0.16	0.43	0.29	0.43	0.25	0.36
Model R^2	.06		.22**		.21**		.36**		.33**	
Session 2										
Intercept	4.87	0.29	3.26	0.24	3.19	0.27	3.20	0.27	3.22	0.24
Cost	0.09	0.36	-0.25	0.30	-0.28	0.33	-0.31	0.34	-0.28	0.30
UV	0.37	0.35	-0.07	0.29	0.02	0.32	0.05	0.33	0.001	0.29
Sum	-0.22	0.49	-0.19	0.41	0.43	0.45	0.36	0.46	0.20	0.41
Ach	0.56$^+$	0.31	-1.10**	0.26	-0.94**	0.29	-1.37**	0.29	-1.17**	0.26
Cost x Ach	-0.47	0.37	0.65*	0.31	0.32	0.34	0.75*	0.35	0.60*	0.31
UV x Ach	-0.25	0.37	0.60$^+$	0.31	0.51	0.34	0.77*	0.35	0.64*	0.31
Sum x Ach	0.53	0.56	0.22	0.47	-0.49	0.52	-0.05	0.53	-0.08	0.47
Model R^2	.11$^+$		.28**		.27**		.34**		.33**	
Session 3										
Intercept	4.94	0.27	3.22	0.23	3.26	0.26	3.49	0.24	3.34	0.22
Cost	-0.02	0.34	0.14	0.29	0.07	0.33	-0.27	0.30	-0.04	0.28
UV	0.21	0.34	0.43	0.29	0.35	0.32	0.05	0.30	0.26	0.28
Sum	0.26	0.46	0.73$^+$	0.40	0.81$^+$	0.45	0.48	0.42	0.65$^+$	0.38
Ach	0.45$^+$	0.25	-0.81**	0.22	-0.81**	0.25	-1.20**	0.23	-0.96**	0.21
Cost x Ach	-0.54$^+$	0.32	0.26	0.28	0.33	0.31	0.62*	0.29	0.42	0.26
UV x Ach	-0.14	0.32	0.43	0.28	0.43	0.31	0.68*	0.29	0.53*	0.26
Sum x Ach	0.35	0.52	0.37	0.45	0.34	0.50	0.59	0.47	0.45	0.43
Model R^2	.10$^+$		.23**		.18**		.31**		.28**	

Note: ** $p < .01$; * $p < .05$; $^+$ $p < 0.10$. All intercepts are significant in the models at $p < .05$.

Output for Non-Focal Motivational Outcomes (RQ 2): Moderating Variable of Prior Achievement

	Competence-Related Beliefs		Intrinsic Value		Attainment Value	
	B	S.E.	B	S.E.	B	S.E.
Session 1						
Intercept	4.86	0.20	4.18	0.27	5.37	0.23
Cost Intervention (Cost)	-0.06	0.25	0.22	0.34	-0.02	0.29
Utility Value Intervention (UV)	-0.02	0.25	0.07	0.33	0.20	0.29
Summary (Sum)	-0.15	0.32	0.13	0.43	0.32	0.36
Prior Achievement (Ach)	0.66**	0.19	0.52*	0.26	0.08	0.22
Cost x Ach	0.09	0.23	-0.17	0.31	0.10	0.27
UV x Ach	0.14	0.24	-0.19	0.32	-0.06	0.28
Sum x Ach	0.71*	0.33	0.06	0.45	-0.51	0.38
Model R^2	.46**		.10*		.04	
Session 2						
Intercept	4.79	0.22	4.30	0.30	4.98	0.29
Cost	0.20	0.27	0.18	0.38	0.41	0.36
UV	0.25	0.27	-0.13	0.37	0.53	0.35
Sum	-0.13	0.37	-0.16	0.52	0.72	0.49
Ach	1.01**	0.23	0.52	0.33	0.45	0.31
Cost x Ach	-0.39	0.28	-0.27	0.39	-0.26	0.37
UV x Ach	-0.31	0.28	-0.15	0.39	-0.43	0.37
Sum x Ach	0.64	0.42	0.45	0.60	-0.18	0.56
Model R^2	.43**		.10*		.06	
Session 3						
Intercept	4.95	0.22	4.21	0.30	5.14	0.28
Cost	-0.09	0.27	0.04	0.38	-0.03	0.34
UV	-0.06	0.27	-0.05	0.38	0.26	0.34
Sum	-0.37	0.37	-0.11	0.52	0.22	0.47
Ach	0.75**	0.21	0.67*	0.28	0.63*	0.26
Cost x Ach	-0.11	0.26	-0.61^{+}	0.36	-0.79*	0.33
UV x Ach	-0.02	0.26	-0.46	0.36	-0.32	0.33
Sum x Ach	0.75^{+}	0.42	0.36	0.58	-0.28	0.53
Model R^2	.39**		.09		.09	

Note: ** $p < .01$; * $p < .05$; $^{+}$ $p < 0.10$. All intercepts are significant in the models at $p < .05$.

Output for Focal Motivational Outcomes (RQ 1): Moderating Variable of Belonging Uncertainty

	Utility Value		Effort Cost		Outside Effort Cost		Emotional Cost		Total Cost	
	B	S.E.	B	S.E.	B	S.E.	B	S.E.	B	S.E.
Session 1										
Intercept	5.21	0.25	3.19	0.24	3.01	0.27	3.25	0.27	3.15	0.24
Cost Intervention (Cost)	-0.24	0.31	0.30	0.29	0.56⁺	0.33	0.40	0.34	0.37	0.29
Utility Value Intervention (UV)	-0.08	0.30	0.06	0.29	0.21	0.32	0.01	0.33	0.05	0.29
Summary (Sum)	-0.09	0.36	0.15	0.35	0.54	0.38	0.01	0.40	0.19	0.34
Belonging Uncertainty (BU)	-0.75*	0.31	0.81**	0.30	0.10	0.33	0.92**	0.34	0.69*	0.30
Cost x BU	0.77*	0.37	-0.52	0.36	0.23	0.40	-0.41	0.41	-0.39	0.35
UV x BU	0.60⁺	0.35	-0.16	0.33	0.41	0.37	-0.09	0.38	0.01	0.33
Sum x BU	0.78⁺	0.41	-0.43	0.39	0.39	0.43	-0.32	0.45	-0.19	0.38
Model R^2	.05		.20**		.12*		.24**		.23**	
Session 2										
Intercept	4.92	0.27	3.19	0.25	3.09	0.28	3.07	0.28	3.12	0.25
Cost	0.01	0.34	0.03	0.31	0.05	0.34	0.10	0.35	0.06	0.32
UV	0.32	0.33	-0.01	0.31	0.09	0.34	0.10	0.34	0.06	0.31
Sum	0.11	0.43	-0.35	0.40	0.01	0.44	-0.24	0.44	-0.21	0.40
BU	-0.53	0.34	0.74*	0.31	0.52	0.35	0.97**	0.35	0.77*	0.32
Cost x BU	0.32	0.41	-0.65⁺	0.38	-0.35	0.41	-0.50	0.42	-0.51	0.38
UV x BU	0.20	0.38	-0.29	0.35	-0.04	0.39	-0.44	0.39	-0.28	0.35
Sum x BU	0.69	0.49	-0.95*	0.45	-0.53	0.50	-0.57	0.50	-0.69	0.46
Model R^2	.07		.13*		.08		.16**		.13*	
Session 3										
Intercept	4.98	0.26	3.23	0.23	3.25	0.25	3.07	0.28	3.34	0.22
Cost	-0.06	0.32	0.29	0.29	0.16	0.31	0.10	0.35	0.12	0.28
UV	0.18	0.32	0.33	0.29	0.31	0.31	0.10	0.34	0.16	0.28
Sum	0.51	0.39	0.49	0.36	0.53	0.38	-0.24	0.44	0.38	0.34
BU	-0.56⁺	0.33	0.74*	0.30	0.92**	0.32	0.97**	0.35	0.85**	0.28
Cost x BU	0.70⁺	0.40	-0.62⁺	0.36	-1.04**	0.38	-0.50	0.42	-0.70*	0.35
UV x BU	0.12	0.37	-0.37	0.33	-0.43	0.35	-0.44	0.39	-0.37	0.32
Sum x BU	0.72	0.45	-0.33	0.41	-0.66	0.43	-0.57	0.50	-0.40	0.39
Model R^2	.10*		.13*		.15*		.16**		.18**	

Note: ** $p < .01$; * $p < .05$; $^+$ $p < 0.10$. All intercepts are significant in the models at $p < .05$.

Output for Non-Focal Motivational Outcomes (RQ 2): Moderating Variable of Belonging Uncertainty

	Competence-Related Beliefs		Intrinsic Value		Attainment Value	
	B	S.E.	B	S.E.	B	S.E.
Session 1						
Intercept	4.89	0.24	4.25	0.26	5.45	0.24
Cost Intervention (Cost)	-0.33	0.30	-0.01	0.33	-0.25	0.30
Utility Value Intervention (UV)	0.03	0.29	0.03	0.32	0.10	0.30
Summary (Sum)	0.23	0.35	0.18	0.38	-0.11	0.35
Belonging Uncertainty (BU)	-0.42	0.31	-0.53	0.33	-0.13	0.31
Cost x BU	0.00	0.36	0.28	0.40	0.06	0.37
UV x BU	-0.06	0.34	0.23	0.37	0.12	0.34
Sum x BU	0.20	0.40	0.72	0.44	0.18	0.41
Model R^2	.13**		.06		.02	
Session 2						
Intercept	4.90	0.25	4.39	0.29	5.06	0.27
Cost	-0.11	0.31	-0.04	0.36	0.27	0.34
UV	0.22	0.30	-0.19	0.35	0.46	0.33
Sum	0.45	0.39	0.11	0.46	0.70	0.43
BU	-0.61^{+}	0.31	-0.43	0.36	-0.49	0.34
Cost x BU	0.02	0.37	0.22	0.43	0.35	0.40
UV x BU	0.25	0.34	0.12	0.40	0.56	0.38
Sum x BU	0.72	0.45	0.67	0.52	0.60	0.48
Model R^2	.14*		.06		.05	
Session 3						
Intercept	4.98	0.24	4.30	0.29	5.16	0.26
Cost	-0.31	0.30	-0.18	0.36	0.02	0.33
UV	-0.02	0.30	-0.10	0.36	0.28	0.32
Sum	0.22	0.37	0.27	0.44	0.35	0.40
BU	-0.52^{+}	0.30	-0.51	0.36	-0.88**	0.33
Cost x BU	0.04	0.37	0.37	0.44	1.06**	0.40
UV x BU	-0.03	0.34	0.12	0.41	0.70^{+}	0.37
Sum x BU	0.52	0.42	0.80	0.50	1.16*	0.45
Model R^2	.15**		.07		.09	

Note: ** $p < .01$; * $p < .05$; $^{+}$ $p < 0.10$. All intercepts are significant in the models at $p < .05$.

Output for Focal Motivational Outcomes (RQ 1): Moderating Variable of Beliefs about Malleability of Intelligence

	Utility Value		Effort Cost		Outside Effort Cost		Emotional Cost		Total Cost	
	B	S.E.	B	S.E.	B	S.E.	B	S.E.	B	S.E.
Session 1										
Intercept	5.31	0.27	3.02	0.28	2.89	0.29	2.99	0.32	3.01	0.28
Cost Intervention (Cost)	-0.34	0.33	0.47	0.33	0.67^{+}	0.35	0.66^{+}	0.39	0.52	0.33
Utility Value Intervention (UV)	-0.18	0.32	0.25	0.33	0.32	0.35	0.28	0.38	0.20	0.33
Summary (Sum)	-0.14	0.38	0.43	0.39	0.73^{+}	0.41	0.35	0.45	0.42	0.39
Intelligence Beliefs (Mind)	0.35	0.31	-0.48	0.31	-0.38	0.33	-0.65^{+}	0.37	-0.48	0.31
Cost x Mind	-0.41	0.36	0.90*	0.36	0.77*	0.39	1.24**	0.43	0.92*	0.36
UV x Mind	-0.41	0.35	0.62^{+}	0.35	0.55	0.37	0.93*	0.42	0.67^{+}	0.35
Sum x Mind	-0.61	0.40	0.17	0.41	0.38	0.43	0.67	0.48	0.41	0.40
Model R^2	.02		.07		.07		.09^{+}		.07	
Session 2										
Intercept	5.05	0.29	2.91	0.27	2.84	0.29	2.84	0.30	2.86	0.27
Cost	-0.08	0.35	0.34	0.33	0.33	0.35	0.30	0.37	0.32	0.33
UV	0.22	0.35	0.28	0.32	0.34	0.35	0.33	0.37	0.32	0.33
Sum	0.09	0.43	-0.07	0.40	0.24	0.43	0.17	0.45	0.11	0.40
Mind	0.42	0.32	-0.49^{+}	0.29	-0.55^{+}	0.32	-0.72*	0.33	-0.60*	0.30
Cost x Mind	-0.43	0.38	1.01**	0.35	1.20**	0.38	1.29**	0.40	1.17**	0.36
UV x Mind	-0.72^{+}	0.37	0.64^{+}	0.34	0.79*	0.37	1.02*	0.39	0.83*	0.35
Sum x Mind	-.79	.48	-0.06	0.44	0.64	0.47	0.48	0.50	0.34	0.45
Model R^2	.05		.12*		.11*		.11*		.12*	
Session 3										
Intercept	5.03	0.30	3.04	0.27	3.05	0.30	3.21	0.31	3.11	0.27
Cost	-0.12	0.36	0.52	0.33	0.45	0.36	0.21	0.37	0.38	0.32
UV	0.15	0.36	0.52	0.32	0.50	0.35	0.19	0.36	0.38	0.32
Sum	0.50	0.42	0.78*	0.38	0.80^{+}	0.42	0.60	0.43	0.71^{+}	0.38
Mind	0.33	0.36	-0.37	0.33	-0.38	0.36	-0.60	0.37	-0.47	0.33
Cost x Mind	-0.16	0.41	0.81*	0.38	0.79^{+}	0.41	0.98*	0.43	0.87*	0.37
UV x Mind	-0.56	0.41	0.57	0.37	0.57	0.40	0.87*	0.42	0.69^{+}	0.37
Sum x Mind	-0.70	0.48	0.27	0.44	0.35	0.48	0.53	0.49	0.39	0.44
Model R^2	.06		.09		.07		.07		.09	

Note: ** $p < .01$; * $p < .05$; $^{+}$ $p < 0.10$. All intercepts are significant in the models at $p < .05$.

Output for Non-Focal Motivational Outcomes (RQ 2): Moderating Variable of Beliefs about Malleability of Intelligence

	Competence-Related Beliefs		Intrinsic Value		Attainment Value	
	B	S.E.	B	S.E.	B	S.E.
Session 1						
Intercept	5.10	0.26	4.30	0.28	5.51	0.26
Cost Intervention (Cost)	-0.54^{+}	0.31	-0.07	0.34	-0.34	0.31
Utility Value Intervention (UV)	-0.19	0.31	-0.03	0.34	0.07	0.31
Summary (Sum)	0.06	0.36	0.19	0.39	-0.14	0.36
Intelligence Beliefs (Mind)	0.79**	0.29	0.40	0.32	0.35	0.29
Cost x Mind	-1.34**	0.34	-0.79*	0.38	-0.69*	0.34
UV x Mind	-0.92**	0.33	-0.53	0.36	-0.53	0.33
Sum x Mind	-1.27**	0.38	-0.62	0.42	-0.51	0.38
Model R^2	.15**		.05		.06	
Session 2						
Intercept	5.08	0.27	4.52	0.30	5.14	0.28
Cost	-0.24	0.32	-0.15	0.37	0.19	0.34
UV	0.06	0.32	-0.28	0.36	0.43	0.34
Sum	0.33	0.39	0.14	0.45	0.75^{+}	0.42
Mind	0.57^{+}	0.29	0.42	0.33	0.26	0.31
Cost x Mind	-1.08**	0.35	-0.41	0.39	-0.36	0.37
UV x Mind	-0.87**	0.34	-0.84*	0.38	-0.57	0.36
Sum x Mind	-0.74^{+}	0.44	-.96^{+}	0.49	-.94*	0.46
Model R^2	.13*		.08		.09	
Session 3						
Intercept	5.12	0.29	4.27	0.33	5.15	0.30
Cost	-0.39	0.34	-0.12	0.39	0.01	0.36
UV	-0.15	0.34	-0.05	0.39	0.31	0.35
Sum	0.07	0.40	0.36	0.46	0.43	0.42
Mind	0.51	0.35	0.22	0.40	0.11	0.36
Cost x Mind	-0.82*	0.40	-0.16	0.45	0.06	0.41
UV x Mind	-0.75^{+}	0.39	-0.62	0.44	-0.42	0.41
Sum x Mind	-0.72	0.46	-0.59	0.53	-0.69	0.48
Model R^2	.07		.06		.07	

Note: ** $p < .01$; * $p < .05$; $^{+}$ $p < 0.10$. All intercepts are significant in the models at $p < .05$.

Output for Focal Motivational Outcomes (RQ 1): Moderating Variable of Gender

	Utility Value		Effort Cost		Outside Effort Cost		Emotional Cost		Total Cost	
	B	S.E.	B	S.E.	B	S.E.	B	S.E.	B	S.E.
Session 1										
Intercept	5.55	0.28	2.94	0.31	2.85	0.32	2.85	0.35	2.86	0.31
Cost Intervention (Cost)	-0.58^{+}	0.35	0.37	0.38	0.60	0.40	0.64	0.44	0.49	0.38
Utility Value Intervention (UV)	-0.15	0.34	0.32	0.37	0.27	0.39	0.28	0.43	0.23	0.37
Summary (Sum)	-0.46	0.40	0.21	0.44	0.45	0.45	0.28	0.51	0.28	0.44
Gender (Female)	-1.22*	0.57	0.89	0.60	0.61	0.63	1.40*	0.71	1.04^{+}	0.59
Cost x Female	1.22^{+}	0.68	-0.47	0.72	-0.39	0.76	-1.12	0.85	-0.66	0.71
UV x Female	0.21	0.68	-0.78	0.72	-0.18	0.76	-0.71	0.85	-0.54	0.71
Sum x Female	1.38^{+}	0.81	-0.01	0.85	0.64	0.90	-0.58	1.01	-0.05	0.84
Model R^2	.09^{+}		.04		.07		.06		.07	
Session 2										
Intercept	5.36	0.30	2.87	0.29	2.78	0.31	2.57	0.32	2.72	0.29
Cost	-0.14	0.38	0.18	0.37	0.19	0.40	0.37	0.41	0.26	0.37
UV	-0.03	0.37	0.28	0.36	0.39	0.39	0.51	0.40	0.40	0.36
Sum	-0.13	0.48	-0.45	0.47	-0.19	0.50	-0.13	0.52	-0.25	0.47
Female	-1.76**	0.64	1.13^{+}	0.62	1.17^{+}	0.67	1.84**	0.69	1.42*	0.62
Cost x Female	1.00	0.76	-0.65	0.74	-0.75	0.79	-1.36	0.82	-0.96	0.74
UV x Female	1.40^{+}	0.75	-0.88	0.73	-1.00	0.79	-1.39^{+}	0.81	-1.11	0.73
Sum x Female	1.37	0.90	-0.15	.88	0.24	0.94	-0.33	0.97	-0.12	0.88
Model R^2	.10^{+}		.08		.07		.11*		.10*	
Session 3										
Intercept	5.31	0.30	2.93	0.28	2.81	0.29	2.88	0.29	2.88	0.27
Cost	-0.38	0.39	0.45	0.35	0.44	0.37	0.42	0.38	0.44	0.34
UV	0.00	0.38	0.58^{+}	0.35	0.69^{+}	0.37	0.42	0.37	0.55	0.33
Sum	0.23	0.47	0.59	0.43	0.66	0.45	0.44	0.46	0.55	0.41
Female	-1.22*	0.61	1.10*	0.55	1.65**	0.59	2.29**	0.59	1.72**	0.53
Cost x Female	1.04	0.74	-0.79	0.67	-1.14	0.71	-2.13**	0.72	-1.42*	0.65
UV x Female	0.74	0.74	-0.92	0.67	-1.44*	0.71	-1.89**	0.71	-1.45*	0.64
Sum x Female	1.18	0.88	-0.16	0.80	-0.45	0.85	-0.75	0.85	-0.47	0.77
Model R^2	.07		.08		.12*		.17**		.14*	

Note: ** $p < .01$; * $p < .05$; $^{+}$ $p < 0.10$. All intercepts are significant in the models at $p < .05$.

Output for Non-Focal Motivational Outcomes (RQ 2): Moderating Variable of Gender

	Competence-Related Beliefs		Intrinsic Value		Attainment Value	
	B	S.E.	B	S.E.	B	S.E.
Session 1						
Intercept	5.30	0.28	4.76	0.29	5.53	0.27
Cost Intervention (Cost)	-0.61^{+}	0.35	-0.26	0.36	-0.48	0.34
Utility Value Intervention (UV)	-0.17	0.35	-0.26	0.36	-0.08	0.34
Summary (Sum)	-0.11	0.41	-0.23	0.42	-0.47	0.40
Gender (Female)	-1.56**	0.57	-2.01**	0.59	-0.28	0.56
Cost x Female	1.34^{+}	0.69	1.28^{+}	0.71	0.80	0.67
UV x Female	0.70	0.68	1.15	0.70	0.63	0.67
Sum x Female	1.17	0.82	1.73*	0.84	1.38^{+}	0.79
Model R^2	.10*		.13*		.06	
Session 2						
Intercept	5.38	0.28	4.86	0.31	5.28	0.30
Cost	-0.50	0.36	-0.25	0.40	0.07	0.39
UV	-0.13	0.34	-0.53	0.39	0.18	0.38
Sum	0.53	0.45	-0.18	0.50	0.36	0.49
Female	-1.90**	0.59	-1.96**	0.67	-0.78	0.65
Cost x Female	1.89**	0.70	1.28	0.79	0.75	0.77
UV x Female	1.40*	0.70	1.45^{+}	0.79	1.00	0.76
Sum x Female	0.42	0.84	1.70^{+}	0.94	1.23	0.91
Model R^2	.15*		.11^{+}		.04	
Session 3						
Intercept	5.41	0.28	4.83	0.32	5.50	0.31
Cost	-0.50	0.36	-0.40	0.41	-0.36	0.39
UV	-0.26	0.35	-0.40	0.40	-0.11	0.38
Sum	0.16	0.43	-0.26	0.50	-0.08	0.47
Female	-1.68**	0.56	-2.08**	0.64	-1.25*	0.61
Cost x Female	1.37*	0.68	1.22	0.78	1.18	0.74
UV x Female	0.99	0.68	1.29^{+}	0.78	1.40^{+}	0.74
Sum x Female	0.48	0.81	2.26*	0.93	1.74^{+}	0.89
Model R^2	.13*		.14*		.06	

Note: ** $p < .01$; * $p < .05$; $^{+}$ $p < 0.10$. All intercepts are significant in the models at $p < .05$.

Output for Course Outcomes (RQ 3-5): Moderating Variable of Baseline Competence-Related Beliefs

Course Achievement										
	Quiz Average		Exam 2		Exam 3		Exam Av.		Final Course Grade	
	B	S.E.	B	S.E.	B	S.E.	B	S.E.	B	S.E.
Intercept	0.85	0.03	0.86	0.02	0.80	0.03	0.83	0.02	87.49	1.91
Cost Intervention (Cost)	-0.03	0.04	0.01	0.03	0.02	0.04	0.01	0.03	-0.09	2.33
Utility Value Intervention (UV)	0.00	0.04	0.03	0.03	0.05	0.04	0.04	0.03	0.72	2.32
Summary (Sum)	-0.06	0.05	-0.04	0.03	0.02	0.05	-0.01	0.03	-3.11	2.86
Competence-Related Beliefs (CB)	0.08*	0.04	0.10**	0.02	0.10**	0.03	0.10**	0.03	9.57**	2.12
Cost x CB	-0.03	0.04	-0.07**	0.03	-0.03	0.04	-0.05^{+}	0.03	-4.03	2.50
UV x CB	-0.05	0.04	-0.08**	0.03	-0.06	0.04	-0.07*	0.03	-5.48*	2.50
Sum x CB	0.01	0.05	-0.03	0.03	-0.01	0.05	-0.02	0.03	-0.77	2.88
Model R^2	.14*		.23**		.20**		.25**		.36**	

Course Participation and Course-Taking								
	Homework Time		Discussion Section Attendance		Enrollment in Next Physics Course		Enrollment in Future STEM Courses	
	B	S.E.	B	S.E.	Log-Odds	S.E.	B	S.E.
Intercept/Constant	126.31	10.88	2.92	0.23	-0.13	0.44	2.99	0.23
Cost	26.01^{+}	13.49	-0.12	0.28	0.39	0.54	0.01	0.28
UV	7.53	13.33	-0.01	0.28	0.30	0.53	0.26	0.28
Sum	0.54	16.61	-0.33	0.35	0.46	0.68	-0.25	0.33
CB	14.42	12.51	0.09	0.26	0.77	0.54	0.20	0.26
Cost x CB	-3.68	14.76	0.05	0.31	-0.61	0.62	-0.29	0.30
UV x CB	-18.37	14.75	0.06	0.31	-0.83	0.61	-0.29	0.30
Sum x CB	-8.83	17.02	0.08	0.36	0.14	0.74	0.16	0.34
Model R^2/ χ^2	$R^2 = .06$		$R^2 = .02$		$\chi^2 = 7.28$		$R^2 = .05$	

Note: ** $p < .01$; * $p < .05$; $^{+}$ $p < 0.10$. All intercepts are significant in the models at $p < .05$, except for the log-odds of the constant in the model testing enrollment in the next physics course.

Output for Course Outcomes (RQ 3-5): Moderating Variable of Prior Achievement

Course Achievement

	Quiz Average		Exam 2		Exam 3		Exam Av.		Final Course Grade	
	B	S.E.	B	S.E.	B	S.E.	B	S.E.	B	S.E.
Intercept	0.83	0.03	0.86	0.02	0.80	0.02	0.83	0.02	86.92	1.21
Cost Intervention (Cost)	0.001	0.04	0.01	0.02	0.04	0.03	0.02	0.02	1.76	1.50
Utility Value Intervention (UV)	0.01	0.04	0.03	0.02	0.04	0.03	0.04	0.02	0.84	1.48
Summary (Sum)	-0.07	0.05	-0.06*	0.03	-0.01	0.04	-0.04	0.03	-5.32**	1.89
Prior Achievement (Ach)	0.08**	0.03	0.10**	0.02	0.14**	0.02	0.12**	0.02	10.75**	1.14
Cost x Ach	-0.004	0.03	-0.07**	0.02	-0.03	0.03	-0.05*	0.02	-2.48^+	1.39
UV x Ach	-0.03	0.03	-0.07**	0.02	-0.06*	0.03	-0.07**	0.02	-3.96**	1.44
Sum x Ach	0.07	0.05	0.02	0.03	0.02	0.04	0.02	0.03	4.18*	1.99
Model R^2	.26**		.38**		.50**		.54**		.73**	

Course Participation and Course-Taking

	Homework Time		Discussion Section Attendance		Enrollment in Next Physics Course		Enrollment in Future STEM Courses	
	B	S.E.	B	S.E.	Log-Odds	S.E.	B	S.E.
Intercept/Constant	125.29	10.44	2.85	0.24	-0.20	0.47	2.94	0.23
Cost	20.36	12.95	-0.07	0.29	0.53	0.56	0.12	0.28
UV	7.38	12.79	0.03	0.29	0.38	0.56	0.34	0.28
Sum	3.89	16.34	-0.29	0.37	0.29	0.88	-0.25	0.36
Ach	6.08	9.83	0.23	0.22	0.81	0.50	0.31	0.21
Cost x Ach	-4.26	12.05	-0.08	0.27	-0.40	0.58	-0.18	0.26
UV x Ach	-13.37	12.47	-0.07	0.28	-0.84	0.59	-0.34	0.27
Sum x Ach	-1.02	17.2	0.08	0.39	1.02	1.17	0.29	0.37
Model R^2/ χ^2	$R^2 = .03$		$R^2 = .04$		$\chi^2 = 12.64^+$		$R^2 = .07$	

Note: ** $p < .01$; * $p < .05$; $^+$ $p < 0.10$. All intercepts are significant in the models at $p < .05$, except for the log-odds of the constant in the model testing enrollment in the next physics course.

Output for Course Outcomes (RQ 3-5): Moderating Variable of Belonging Uncertainty

Course Achievement

	Quiz Average		Exam 2		Exam 3		Exam Av.		Final Course Grade	
	B	S.E.	B	S.E.	B	S.E.	B	S.E.	B	S.E.
Intercept	0.84	0.03	0.87	0.02	0.81	0.03	0.84	0.02	87.34	2.09
Cost Intervention (Cost)	-0.02	0.04	-0.003	0.03	0.000	0.04	-0.002	0.03	-1.07	2.56
Utility Value Intervention (UV)	0.01	0.04	0.03	0.03	0.04	0.04	0.04	0.03	1.20	2.56
Summary (Sum)	-0.02	0.05	-0.01	0.03	0.05	0.05	0.02	0.03	0.24	3.15
Belonging Uncertainty (BU)	-0.08$^+$	0.04	-0.09**	0.03	-0.12**	0.04	-0.11**	0.03	-10.45**	2.59
Cost x BU	0.05	0.05	0.06*	0.03	0.09$^+$	0.05	0.08*	0.03	7.97*	3.09
UV x BU	0.06	0.05	0.07*	0.03	0.08$^+$	0.04	0.07*	0.03	6.68*	2.88
Sum x BU	0.10$^+$	0.06	0.03	0.04	0.09	0.05	0.06	0.04	8.18*	3.59
Model R^2	.05		.19**		.16**		.20**		.19**	

Course Participation and Course-Taking

	Homework Time		Discussion Section Attendance		Enrollment in Next Physics Course		Enrollment in Future STEM Courses	
	B	S.E.	B	S.E.	Log-Odds	S.E.	B	S.E.
Intercept/Constant	122.05	10.84	2.84	0.22	-0.21	0.43	2.94	0.21
Cost	28.19*	13.47	-0.10	0.28	0.39	0.53	0.04	0.26
UV	11.60	13.37	0.10	0.27	0.39	0.52	0.30	0.26
Sum	6.55	16.61	-0.2	0.34	0.57	0.64	-0.14	0.32
BU	-16.53	13.71	-0.08	0.28	-0.76	0.57	-0.63*	0.27
Cost x BU	13.37	16.45	-0.34	0.34	-0.03	0.69	0.05	0.32
UV x BU	16.86	15.28	-0.16	0.31	0.61	0.63	0.70*	0.30
Sum x BU	17.04	18.86	0.06	0.39	1.36$^+$	0.80	0.62$^+$	0.37
Model R^2/ χ^2	R^2 = .05		R^2 = .07		χ^2 = 9.43		R^2 = .13**	

Note: ** $p < .01$; * $p < .05$; $^+$ $p < 0.10$. All intercepts are significant in the models at $p < .05$, except for the log-odds of the constant in the model testing enrollment in the next physics course.

Output for Course Outcomes (RQ 3-5): Moderating Variable of Beliefs about Malleability of Intelligence

Course Achievement

	Quiz Average		Exam 2		Exam 3		Exam Av.		Final Course Grade	
	B	S.E.	B	S.E.	B	S.E.	B	S.E.	B	S.E.
Intercept	0.86	0.03	0.87	0.03	0.81	0.04	0.84	0.03	88.51	2.39
Cost Intervention (Cost)	-0.05	0.04	-0.002	0.03	-0.01	0.04	-0.01	0.03	-2.48	2.85
Utility Value Intervention (UV)	-0.02	0.04	0.03	0.03	0.04	0.04	0.04	0.03	-0.08	2.87
Summary (Sum)	-0.04	0.05	-0.02	0.04	0.04	0.05	0.01	0.04	-1.04	3.40
Intelligence Beliefs (Mind)	0.03	0.04	0.01	0.03	0.02	0.04	0.02	0.03	3.01	2.74
Cost x Mind	-0.09*	0.05	-0.03	0.03	-0.08^{+}	0.05	-0.05	0.04	-7.22*	3.19
UV x Mind	0.004	0.05	-0.02	0.03	-0.04	0.05	-0.03	0.03	-3.54	3.09
Sum x Mind	-0.07	0.05	-0.06	0.04	-0.09^{+}	0.05	-0.07^{+}	0.04	-9.00*	3.62
Model R^2	$.10^{+}$		.07		$.10^{+}$		$.09^{+}$		.11*	

Course Participation and Course-Taking

	Homework Time		Discussion Section Attendance		Enrollment in Next Physics Course		Enrollment in Future STEM Courses	
	B	S.E.	B	S.E.	Log-Odds	S.E.	B	S.E.
Intercept/Constant	128.29	11.47	2.93	0.23	-0.12	0.44	3.02	0.24
Cost	21.8	13.93	-0.19	0.29	0.33	0.53	-0.01	0.29
UV	7.44	13.89	0.02	0.28	0.27	0.53	0.22	0.29
Sum	2.26	16.63	-0.33	0.34	0.57	0.64	-0.21	0.34
Mind	6.46	13.02	0.06	0.26	-0.14	0.50	0.07	0.27
Cost x Mind	-10.34	15.47	-0.29	0.32	-0.06	0.59	-0.23	0.32
UV x Mind	-16.67	14.84	-0.23	0.30	0.29	0.58	0.02	0.31
Sum x Mind	-18.7	17.20	-0.73^{+}	0.37	-0.32	0.69	-0.27	0.37
Model R^2/ χ^2	$R^2 = .06$		$R^2 = .08$		$\chi^2 = 2.42$		$R^2 = .04$	

Note: ** $p < .01$; * $p < .05$; $^{+}$ $p < 0.10$. All intercepts are significant in the models at $p < .05$, except for the log-odds of the constant in the model testing enrollment in the next physics course.

Output for Course Outcomes (RQ 3-5): Moderating Variable of Gender

Course Achievement

	Quiz Average		Exam 2		Exam 3		Exam Av.		Final Course Grade	
	B	S.E.	B	S.E.	B	S.E.	B	S.E.	B	S.E.
Intercept	0.86	0.04	0.92	0.03	0.86	0.04	0.89	0.03	91.39	2.57
Cost Intervention (Cost)	-0.06	0.05	-0.05	0.03	-0.04	0.05	-0.04	0.04	-4.25	3.20
Utility Value Intervention (UV)	-0.01	0.05	-0.01	0.03	-0.02	0.05	-0.02	0.03	-2.63	3.16
Summary (Sum)	-0.05	0.06	-0.06	0.04	-0.01	0.06	-0.03	0.04	-4.13	3.83
Gender (Female)	-0.09	0.08	-0.17**	0.05	-0.19*	0.07	-0.18**	0.05	-14.39**	5.03
Cost x Female	0.14	0.09	0.17**	0.06	0.18*	0.09	0.17*	0.07	13.64*	6.08
UV x Female	0.07	0.09	0.13*	0.06	0.22*	0.09	0.18**	0.07	12.73*	6.11
Sum x Female	0.13	0.11	0.11	0.07	0.17	0.11	0.14^+	0.08	13.71^+	7.47
Model R^2	.03		.13*		.07		$.10^+$		.07	

Course Participation and Course-Taking

	Homework Time		Discussion Section Attendance		Enrollment in Next Physics Course		Enrollment in Future STEM Courses	
	B	S.E.	B	S.E.	Log-Odds	S.E.	B	S.E.
Intercept/Constant	127.18	12.07	2.79	0.25	-0.22	0.47	3.11	0.25
Cost	11.52	15.34	-0.36	0.32	0.29	0.60	-0.27	0.32
UV	-2.10	15.16	0.05	0.31	0.41	0.59	-0.02	0.31
Sum	-4.05	18.18	-0.29	0.38	0.51	0.72	-0.25	0.38
Female	-16.93	24.65	0.21	0.51	0.22	0.94	-0.61	0.50
Cost x Female	56.57^+	29.91	0.97	0.62	0.52	1.18	1.31*	0.62
UV x Female	44.47	29.60	0.02	0.61	-0.27	1.13	1.12^+	0.60
Sum x Female	39.40	36.69	0.29	0.76	0.18	1.39	0.42	0.73
Model R^2/ χ^2	R^2 = .10*		R^2 = .10		χ^2 = 2.47		R^2 = .08	

Note: ** $p < .01$; * $p < .05$; $^+$ $p < 0.10$. All intercepts are significant in the models at $p < .05$, except for the log-odds of the constant in the model testing enrollment in the next physics course.

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