Utility Value Plays a Powerful Role in the Development of Interest: Evidence from Laboratory and Field Experiments

by

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Abstract

When an instructor triggers students' interest, the instructor temporarily engages students' attention, promoting alertness, concentration, and positive emotions related to the relevant topic; for example, a chemical reaction could grab students' attention as well as excite them about the subject of chemistry. This type of interest often fades away when instruction ends, but sometimes students can begin to develop an individual interest in the topic that endures. How can educators trigger and maintain students' interest during instruction, and help students develop deeper, enduring interest? In a series of seven laboratory experiments and one field experiment I addressed this question, focusing on one factor that is theorized to play an important role in the development of longer-term interest: the belief that a topic has "utility value" (i.e., that it is useful to learn about). In laboratory experiments (Studies 1-7), I used a paradigm in which undergraduate participants were taught about a statistical topic (linear regression), and I manipulated whether participants were told about its usefulness for various careers. This utility value manipulation, which successfully promoted participants' beliefs about the usefulness of regression, also strengthened participants' interest in regression and made them more likely to request information about statistics resources on campus, an indicator of deeper interest. However, this manipulation had null effects on measures of triggered situational interest in the learning session, engagement with instructional materials, and performance on an end-of-study test. In Study 8, the field experiment, I tested a utility value intervention in several hundred middle and high school algebra classrooms via educational technology. As was the case in the laboratory studies, the intervention promoted students' beliefs about the usefulness of the topic (in this case, algebra) and promoted the development of maintained interest in the topic, but it did not improve engagement or performance. Overall, these studies suggest that although beliefs

about the usefulness of academic content may not be effective targets for educators who want to promote student engagement with instructional materials, they can play an important role in the development of deeper interest in a topic that endures beyond the immediate situation.

Utility Value Plays a Powerful Role in the Development of Interest: Evidence from Laboratory and Field Experiments

How do students make academic choices? What leads a college student to choose a particular major or decide to change majors? Why do some algebra students enthusiastically participate in class and go above and beyond what's asked of them on assignments, whereas other students are disengaged during lessons, neglect homework assignments, and even skip class altogether? These questions are important not only for psychologists who wish to understand human motivation, but also for educators who want their students to engage in learning, policy makers who aim to grow and diversify various fields, and for the many students who feel like school is a waste of their time.

The answers to these questions vary widely for different students, but for many they revolve around interest. When students are interested in particular courses or topics, they tend to engage with them voluntarily and happily, persisting even when obstacles arise (Renninger & Hidi, 2016). This is the type of motivation that educators want for students, but how can we encourage its development? The present research seeks to address this question, focusing on one particular factor that influences students' interest in a particular content area: their belief that the topic is useful to learn. This research is grounded in two theories of academic motivation: the four-phase model of interest development (Hidi & Renninger, 2006) and expectancy-value theory (Eccles & Wigfield, 2020).

Theoretical Background

Hidi and Renninger (2006) proposed a four-phase model of how a person's interest in a particular topic or content develops over time. In the first two phases, interest is a relatively transient psychological state that is characterized by alertness and concentration during task engagement. In these two phases, interest is considered "situational" because it is triggered (Phase 1), and then maintained (Phase 2) by external factors. For example, as tenth grade chemistry students fixate on a flashy chemical reaction at the start of a unit on combustion, they're experiencing situational interest. Whereas triggered situational interest is thought to primarily involve attention and engagement in a situation, maintained situational interest is thought to involve positive emotions (e.g., enjoyment or excitement about the content) and emerging beliefs that content is important or meaningful (Linnenbrink-Garcia et al., 2010; Schiefele, 1991). As their names imply, triggered and maintained situational interest are typically confined to a specific situation, fading when the environment no longer supports them, although they can deepen under the right circumstances (Linnenbrink-Garcia et al., 2010).

Over time, an individual can develop a more well-established, "individual" interest in a topic that lasts beyond a particular situation. In Hidi and Renninger's (2006) four-phase model, Phase 3 is "emerging individual interest," and Phase 4 is "well-developed individual interest." In these later two phases, interest becomes increasingly internalized. A person with an individual interest in a topic is less dependent on situational cues or supports to trigger or maintain their interest. In Phase 3, individuals are likely to independently re-engage with the topic as they begin to develop their own questions that facilitate further exploration. In Phase 4, individuals continue to independently re-engage and can persist through difficulty, guided by curiosity, knowledge about the topic, and goals that they've set for themselves (Renninger & Hidi, 2020).

Promoting Interest

In the four-phase model, individuals don't skip phases of interest development: interest must be triggered before it can be maintained, and it must be maintained before it can become internalized and persist beyond a situation (Hidi & Renninger, 2006). Accordingly, it's crucial to think about instructional practices that can help students at different phases of interest development advance to the next phase. Expectancy-value theory (Eccles & Wigfield, 2020; Wigfield & Eccles, 2000) provides insight about how interest in a task or topic can be triggered, maintained, and internalized. According to this theory, students choose to pursue academic tasks that they (*i*) expect to succeed in, and (*ii*) find personally valuable. For example, students should choose to invest their time and effort in courses that they value, and courses that they are confident about. Within the expectancy-value framework, there are three primary reasons that students can find value in an achievement-related task (e.g., learning about math). First, a task is said to have *intrinsic value* if students find it inherently enjoyable. Second, a task is said to have *utility value* if students believe that it is useful for achieving their goals. Third, a task is said to have *attainment value* if students come to view attainment of the task as part of their identities (e.g., self-identified "math people" see themselves as people who take and succeed in math courses).

Intrinsic value, utility value, and attainment value are thought to be implicated in different phases of interest development. Intrinsic value is important for triggering and maintaining situational interest: for instance, tasks that engage the senses and emotions (e.g., fun in-class activities) have intrinsic appeal and can catch and hold the attention of learners (Mitchell, 1993). Instructional conditions that include hands-on-activities, challenges, group work, puzzles, and computers have been found to trigger this early phase of interest (Mitchell, 1993; Renninger et al., 2019).

Instructional conditions that allow students to make choices should also trigger situational interest. When students are allowed to make choices about their learning experiences (e.g., a book that they will read, a piece of music they will learn, or a worksheet that they will complete), they experience a sense of autonomy. According to self-determination theory, autonomy is a basic psychological need, and individuals will be intrinsically motivated in contexts where this need is fulfilled (Ryan & Deci, 2000). When students are provided with choices, their experience of autonomy would therefore be expected to translate to a sense of intrinsic value and triggered situational interest.

When it comes to promoting deeper interest, utility value is theorized to play an important role (Harackiewicz et al., 2014; Hidi & Renninger, 2006; Hulleman et al., 2010; Mitchell, 1993). Dewey (1913) argued that a "genuine interest" in a topic emerges through an "identification" process, in which an individual comes to believe that engaging with the topic will confirm a valued aspect of the self. If this is the case, instructional practices that help students see the utility value of course material for important goals should promote identification and interest. Mitchell (1993) drew upon Dewey's ideas about identification to argue that students will experience maintained interest in a topic when they believe that it can empower them to achieve their personal goals. If students choose to take a chemistry class because it helps them understand climate change or another topic that is important to them, they will likely maintain their interest in chemistry even in the absence of flashy demonstrations. If instructors can help students see the utility value of course content for important personal goals, they should help students progress from triggered situational interest to deeper phases of interest development in which attention is maintained, positive emotions are experienced, tasks are valued, and students begin to reengage with course content without a situational trigger.

Whereas well-developed individual interest, the final phase of the four-phase model, is thought to emerge gradually as an individual independently engages with a topic or task over time (Hidi & Renninger, 2006), the first three phases of the model are important and reasonable targets for instructors. By promoting situational interest instructors can facilitate enthusiastic participation, an outcome that should improve students' experiences and improve learning. By giving students opportunities to think about personal value of course content, teachers may support the maintenance of situational interest and even create conditions that bring about the development of emerging individual interest.

Situational and individual interest are theorized to involve different processes, suggesting that educators may best support their students' interest development by combining different instructional practices that promote different phases of interest. Practices that engage students' attention should be most effective at triggering situational interest; practices that help students experience positive emotions and value what they are learning should help maintain situational interest, and practices that allow students to find value and meaning in what they are learning should be critical for supporting the development of emerging individual interest. In prior experimental work, researchers have tested the effects of instructional manipulations that provide students with choices (which support feelings of autonomy and promote early phases of interest development) separately from manipulations that emphasize the utility value of academic content (which should promote deeper interest). This work serves as the experimental basis for Studies 1-7 in the present research, which test the independent and combined effects of these manipulations, examining their impact on triggered situational interest, maintained situational interest, and emerging individual interest.

Choice Manipulations

In studies of academic choice, students are provided with opportunities to determine aspects of their educational experiences (Patall et al., 2008). Across studies, some of these choices have been instructionally relevant (e.g., about the topic of a lesson, or the theme for a writing assignment), whereas others have concerned peripheral aspects of the learning experience (e.g., the design of an avatar in an online learning environment, or the color of the pen to write with). Although instructionally relevant choices can tap into students' existing interest and goals, irrelevant choices still provide students with a sense of self-determination and can help them express their identities (Patall et al., 2008; Reber et al., 2018; Ryan & Deci, 2000). Thus, there is reason to expect that students can experience triggered situational interest if given either type of choice.

Research supports the link between choice and situational interest. For example, in a study of instructionally irrelevant choices, Cordova and Lepper (1996) manipulated whether or not students could customize several features of an online educational game. In a choice condition, students were allowed to select the in-game icon that represented them, and they could name both their character and their opponent. Compared to students who couldn't choose these features, those who were given choice reported that they enjoyed the computer game more, demonstrated deeper involvement with the game on several behavioral measures, and performed better during the session.

In studies of instructionally relevant choices, Høgheim and Reber (2015, 2017) manipulated whether students could choose the examples used in mathematical word problems, rather than having them simply assigned. This type of choice increased students' triggered situational interest, maintained situational interest in the learning material, and self-reported effort. In a larger follow-up study, Høgheim and Reber (2017) found an overall effect of an example choice manipulation on triggered situational interest, but no effects on maintained situational interest or effort.

In addition to the evidence from the studies discussed above, systematic reviews support the connection between the provision of choice and triggered situational interest and engagement during a lesson. In a meta-analysis of 41 studies, Patall, Cooper, and Robinson (2008) found that choice manipulations consistently affected participants' self-reported enjoyment, $d = .36$, their interest, $d = .18$, and their engagement in activities (indexed with measures such as time on task), $d = .30$. Taken together, these studies suggest that when students are provided with choices (instructionally relevant or not), this practice can trigger situational interest in topics and change how participants engage with academic work.

Utility-Value Manipulations

With the goal of promoting interest, researchers have tested two general strategies to help students realize and appreciate the utility value of specific academic content in laboratory studies: (1) directly-communicated utility value presentations, in which students are given information about the usefulness of the content, and (2) self-generated utility value writing activities, in which students are asked to reflect about the content's personal usefulness and write about those connections. Most of these studies have used a paradigm in which students are taught a technique for performing rapid mental multiplication of two-digit numbers, first developed by Barron and Harackiewicz (2001) as a means of studying motivation and learning in a laboratory setting. This paradigm was developed with ecological validity in mind; in it, students are taught a new math-related skill as they would be in a classroom learning experience.

Directly-Communicated Utility Value Manipulations. Durik and Harackiewicz (2007) gave students a presentation about the potential usefulness of the multiplication technique, discussing how knowledge of the technique could be useful for students in a variety of situations (e.g., personal banking, math coursework). This manipulation promoted beliefs that the technique was useful, and it also increased task involvement and maintained situational interest in the multiplication technique for participants who entered the session with higher levels of initial interest.

Durik and colleagues (2015) later raised the possibility that confidence, not interest, may be the more important moderator of directly-communicated utility value manipulations, and that their prior findings might have emerged because confidence and interest are highly correlated. Confident students might be more motivated by a presentation about utility value because a skill like the multiplication technique can only be seen as useful to the extent that someone believes they can perform it. Furthermore, directly-communicated utility value might even be threatening for less confident students (Durik, Hulleman, et al., 2015; Durik, Shechter, et al., 2015). Durik and colleagues (2015, Study 1) found evidence for this hypothesis when they tested both initial interest and confidence as moderators in the same regression model. In this analysis, the directly-communicated utility-value manipulation increased maintained situational interest and performance for confident students but decreased these outcomes for less confident students.

Self-Generated Utility-Value Manipulations. Hulleman and colleagues (2010) used the same mental math paradigm to test a *self-generated* approach for promoting utility value. Specifically, they replaced the presentation about utility value with a writing exercise in which participants were asked to reflect about the usefulness of the multiplication technique, coming up with their own ideas. In this study, participants who reflected on usefulness reported stronger perceptions of the technique's utility value, greater feelings of maintained situational interest in the technique, and were more likely to agree when asked if they might use the technique in the future. In contrast to the directly-communicated manipulation, the benefits of the self-generated utility value intervention were largest for those with lower levels of initial confidence.

Comparing and Combining the Two Manipulations. Canning and Harackiewicz (2015) compared self-generated and directly-communicated utility value approaches in a series of lab studies with the mental math paradigm. Across these studies, they found that the directly-communicated approach was only effective for students with higher levels of initial confidence, replicating the findings of Durik and colleagues (2007, 2015). After viewing a presentation about the math technique's usefulness, only the confident students reported higher perceptions of utility value, reported greater feelings of maintained situational interest in the multiplication technique, and performed better on a posttest. In contrast, the directly-communicated presentation decreased scores on these outcomes, possibly because it was experienced as threatening by less confident students. Like Hulleman and colleagues (2010), Canning and Harackiewicz found that self-generated utility value helped less confident students in terms of promoting utility value beliefs, situational interest in mental math, and performance. In addition, Canning and Harackiewicz found that a combination of self-generated and directly-communicated utility value had the largest benefits for less-confident students.

Finally, Hecht et al. (2021) tested utility-value manipulations with two studies in a new paradigm in which participants learned about the biology of fungi. In these studies, a directly-communicated utility value manipulation taught participants about the usefulness of fungi for beer making, baking, and gardening. In Study 1, this manipulation promoted beliefs about the usefulness of learning about fungi, with larger effects for students who reported higher levels of interest at baseline. They also found that the manipulation promoted triggered situational interest in the instructional materials and increased performance on an end-of-session test for those with higher levels of initial interest.

In Study 2, Hecht and colleagues combined their directly-communicated utility value manipulation with two different versions of self-generated utility value. Both versions asked students to write about "how fungi (or knowledge about fungi) might be useful." However, Hecht and colleagues varied the temporal focus of the reflection with: (1) a present-oriented version that encouraged students to write about usefulness for daily life, and (2) a future-oriented version that encouraged students to write about usefulness for the distant future (e.g., for potential careers). They found both the present- and future-oriented version promoted utility value perceptions and increased performance on a test, and they also found several distinct benefits of the future-oriented manipulation. For participants with higher levels of initial interest, the future-oriented manipulation increased situational interest in the instructional materials and in fungi as well as self-reported intentions to use knowledge about fungi in the future.

Considered together, what do these studies tell us about the effects of utility-value manipulations on interest development? First, they illustrate the challenges of convincing students that academic content is useful. In the studies reported above, most effects on beliefs about utility value were moderated; the utility-value manipulations rarely worked for everyone. Directly-communicated approaches typically worked better for more confident or more interested students than they did for less confident or less interested students. Self-generated or combined approaches, on the other hand, showed more promise for less confident students than they did for more confident students.

Second, when utility-value manipulations increased perceptions of utility value (either overall or for a subgroup), researchers typically found a corresponding benefit on measures of maintained situational interest in the content. In addition, two studies reported positive effects of utility-value manipulations on participants' intentions to use what they had learned in the future

(Hecht et al., 2021; Hulleman et al., 2010). By showing that beliefs about utility value are linked to maintained situational interest and intentions to reengage with content, these studies provide evidence that utility value may play a role in moving students past triggered situational interest toward more advanced phases of interest development.

Finally, it should be mentioned that all studies except Hecht et al. (2020) took place in the same context: a lab study paradigm in which students were taught a mental multiplication technique. It is possible that the specific patterns of effects in these studies may reflect characteristics of the mental math topic. Specifically, perceived competence may have emerged as a particularly important moderator in this context because the mental math paradigm teaches participants a skill that requires high levels of mastery to be useful. This point was raised by Hecht and colleagues (2020) as they speculated about why their manipulations were moderated by interest instead of confidence in a context where students were learning about the biology of fungus – a topic that might be perceived as useful even without high levels of mastery.

Promoting Interest Development: Current Evidence and Next Steps

The studies reviewed above demonstrate that choice and utility-value manipulations can support interest development. They also suggest that these manipulations may affect different types of interest and have different downstream consequences. Choice manipulations consistently triggered situational interest in instructional materials. Students who were given choices about their learning were more likely to report that the materials grabbed and held their attention (e.g., Cordova & Lepper, 1996; Høgheim & Reber, 2015). As for downstream consequences, when choice manipulations triggered situational interest, they also tended to affect measures of moment-to-moment engagement in learning sessions (e.g., self-reported task involvement and effort), and they sometimes affected performance as well.

In contrast, few laboratory studies of utility-value manipulations reported effects on triggered situational interest or engagement. Instead, these studies focused on maintained situational interest as a dependent variable. When students were told about utility value (in studies of directly-communicated utility value) or asked to reflect on personal value (in studies of self-generated utility value), they reported higher levels of maintained situational interest. Specifically, directly-communicated utility value promoted maintained situational interest for participants with higher levels of baseline interest and/or confidence, and self-generated utility value raised maintained situational interest for less confident students. Whereas few directly-communicated manipulations influenced learning (as assessed by test performance), self-generated manipulations often did.

The Need to Assess Emerging Individual Interest

In nearly every study reported above, researchers focused exclusively on the initial two phases of interest development: triggered and maintained situational interest, with different emphases. For example, in studies of utility-value manipulations, researchers did not examine whether beliefs about usefulness could promote triggered situational interest, effort, or engagement. In contrast, studies of choice manipulations often reported effects on these outcomes, but they did not often measure maintained situational interest or emerging individual interest, the type of interest that might continue beyond the session.

Given that choice manipulations are situational manipulations intended to change a student’s experience during a learning session, it makes sense to first examine effects on situational outcomes rather than longer-term interest. Utility-value manipulations, on the other hand, are far less situational. When students learn about (or write about) how academic content might relate to their lives and goals, it is reasonable to expect that this might promote interest that goes beyond the learning session (Dewey, 1913; Hidi & Renninger, 2006; Mitchell, 1993). However, the most common laboratory utility-value paradigm (in which participants learn a mental multiplication technique) makes it difficult to assess emerging individual interest, because connections to other activities or opportunities for deeper involvement are hard to establish in this laboratory paradigm. In an educational setting, a lecture or lesson is typically embedded in a course or curriculum and could therefore promote interest in a broader topic, but this is not the case with the stand-alone mental math paradigm. Thus, a new laboratory paradigm is needed to explore the effects of instructional practices that target the first three phases of interest development. This paradigm must afford measurement not only of students' triggered and maintained situational interest in a topic but also their authentic, longer-term intentions to re-engage with the topic (i.e., emerging individual interest).

The Present Research

In Studies 1-7 of the present research, I developed a new laboratory paradigm and used it to test the motivational consequences of utility value and choice manipulations, focusing on the development of interest during a learning situation. Whereas manipulations linked to intrinsic value may have particularly strong effects on triggered situational interest and engagement in academic work, the theory of interest development suggests that a utility-value manipulation should play a more powerful role in promoting deeper interest in academic content. Studying the independent and combined effects of choice and utility-value interventions, paired with assessment of different types of interest throughout a learning session, affords a careful analysis of interest development and motivational dynamics. Rather than reporting the results of the seven laboratory studies one at a time, I perform a meta-analysis to gain statistical power and precision.

In Study 8, a field experiment which took place in several hundred middle and high school algebra classrooms via educational technology, I examine whether the conclusions from the meta-analysis of Studies 1-7 generalize at scale in real math classrooms. In addition, I use Study 2 to examine issues related to dosage and timing when communicating the usefulness of content to students; namely, is there a benefit of repeating these messages, and how long do their effects on behavior and motivation endure?

Studies 1-7: Laboratory Experiments

With the goals of (1) reliably and powerfully manipulating participants' beliefs about the usefulness of academic content, (2) creating a context in which I could test educational practices such as directly communicated utility value and the provision of choice, and (3) assessing not only situational interest but also emerging individual interest, I developed a new laboratory paradigm built around a new topic: multiple regression. In this paradigm, all participants watch an approximately 19-minute instructional video that introduces the topic of linear regression and teaches participants how to conduct and interpret analyses involving one continuous predictor and one dichotomous predictor. This video can be viewed at https://osf.io/wvzsc. Because multiple regression is a genuinely useful topic for many careers, I reasoned that a utility-value manipulation could be powerful in this context. In addition, statistical analysis is a growing focus for undergraduate education (for example, in 2020 the university where this work took place introduced a new "data science" major), so a regression-based paradigm provided me with the opportunity to examine effects on emerging individual interest by assessing intentions to engage with related on-campus opportunities.

Using the multiple regression paradigm, I conducted seven laboratory experiments testing and comparing the effects of utility value and choice manipulations on the first three phases of interest development. The individual studies were sequentially designed to replicate each other and build upon one another, using the same paradigm and testing the same outcomes throughout.

Rather than reporting each study, one at a time, I will meta-analyze the studies to enable more powerful and precise tests of my research questions. If I were to report analyses of individual studies, one at a time, inconsistent findings would surely arise (i.e., *p* values that might be significant in larger studies and non-significant in smaller studies). By taking the extra step to formally synthesize these results, a meta-analysis can provide clarity. In addition, by meta-analyzing the effects of utility value and choice manipulations, I can answer research questions about the relative effects of each type of manipulation on different phases of interest development. For example, I can ask which kind of manipulation is more effective at promoting engagement and triggered situational interest, and which kind is more effective at promoting deeper interest. I will begin with a description of shared procedures, manipulations, and measures. After this, I will provide an overview of the individual studies, discussing how they evolved and differ.

Method

Participants

This study was approved by the Institutional Review Board at the University of Wisconsin-Madison. Table 1 displays an overview of demographic information for participants in each study. Participants were undergraduates enrolled in an introductory psychology course at a large Midwestern university. In total, Studies 1-7 consisted of 2,019 participants, 59.2% women, 40.6% men, and < 1% non-binary. In terms of race/ethnicity, 74.2% of participants identified as White, 20.8% as Asian, 8% as Hispanic, 3.6% as Black, and 1% as belonging to an indigenous group. The sample consisted of mostly first-year college students (84% of participants; average age = 18.7 years). All participants completed the studies for course credit.

Table 1. Information about Participants in Studies 1-7

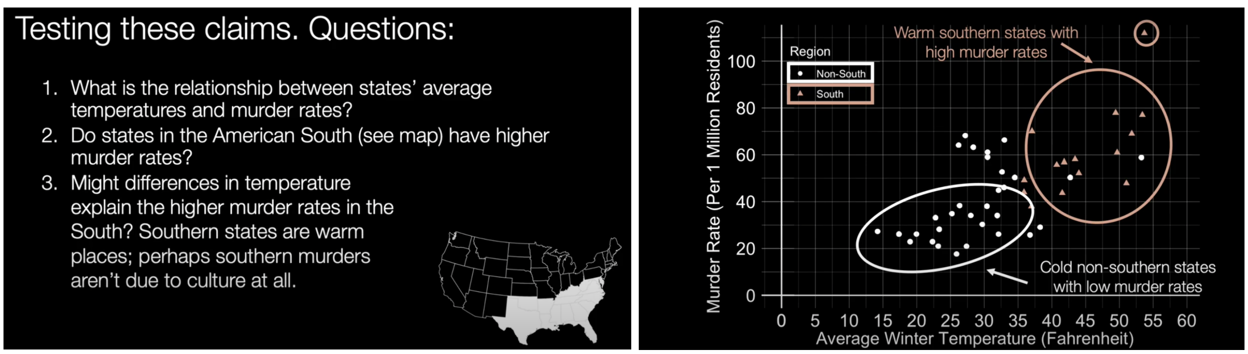
Study	Term	N	Pct. Women	Pct. White	Pct. Hisp.	Pct. Black	Pct. Asian	Pct. Indig.	Pct. First Year	Ave. Age
1	Spring	115	67.0%	72.2%	9.6%	2.6%	22.6%	0.0%	84.4%	19.1
2	Fall	256	54.7%	77.0%	10.6%	2.0%	17.6%	0.4%	77.3%	18.6
3	Spring	100	57.0%	81.0%	5.0%	1.0%	15.0%	1.0%	79.0%	19.1
4	Fall	673	59.6%	74.2%	6.5%	3.9%	21.3%	1.2%	86.2%	18.5
5	Spring	171	57.3%	77.2%	5.3%	2.3%	22.2%	1.2%	78.4%	18.9
6	Fall	377	58.4%	72.7%	7.7%	4.0%	22.3%	1.6%	85.4%	18.5
7	Fall	327	61.8%	71.3%	11.0%	5.8%	21.1%	0.9%	87.2%	18.7
All	--	2019	59.2%	74.2%	8.0%	3.6%	20.8%	1.0%	84.0%	18.7

Standard Procedures

The following procedures were held constant across all seven studies. Participants were run individually by an experimenter who greeted them, gave a brief overview of the study, and set them up with a Qualtrics session. To keep experimenters blind to condition and to ensure a standardized experience, the remainder of the study was delivered over the computer. Due to the COVID-19 pandemic, studies 2 and 3 were conducted over video calls; all other studies were conducted in person. Study 1 was conducted in the 2019-2020 academic year, Studies 2 and 3 were conducted in the 2020-2021 academic year, Studies 4 and 5 were conducted in the 2021-2022 academic year, and Studies 6 and 7 were conducted in the Fall of 2023.

Participants began the study by completing a baseline questionnaire that assessed their initial confidence in math and interest in statistics. Next, all participants watched the instructional video about multiple regression and were exposed to experimental manipulations. In all studies, the video was built around several research topics as it taught the basics of linear regression. For example, in Studies 1 and 2, the video (which can be viewed at https://osf.io/xdfuh) used a dataset of average temperatures and murder rates for the 48 continental U.S. states to examine the evidence for competing hypotheses about the causes of violence in the American South: southern culture (see the "culture of honor" hypothesis, Cohen et al., 1996) vs. warm temperatures. Figure 1 displays screenshots from the video. After the instructional video and manipulations, participants completed questionnaires and a posttest on multiple regression.

Figure 1. Screenshots from the Instructional Video about Regression



First, participants filled out a questionnaire assessing their triggered situational interest, distraction during the video, and feelings of maintained situational interest. After filling out these self-report measures, participants in all studies completed a behavioral measure of emerging individual interest in statistics. For this measure, participants were asked if they wanted to receive any of the following resources: a list of introductory statistics courses at the university, a list of courses at the university that cover regression in more detail, information about a new data science major on campus, or links to websites with resources about regression, statistics, and related careers. If participants were interested, they were asked to provide their email. This measure was designed to capture voluntary reengagement with statistics that extended beyond the situation (Hidi & Renninger, 2006).

Finally, participants completed a twelve-minute timed test on concepts from the instructional video.

Measures

Table 2 displays alphas for all scales in Studies 1-7 and Table 3 displays median correlations between measures. See Appendix A for descriptive statistics and correlation matrices for measures from each study and Appendix B for all items.

Baseline measures. In Studies 1-7, before watching the regression video or being exposed to any manipulation, participants reported their baseline confidence in math and their baseline interest in statistics. Confidence in mathematics was assessed with three items (e.g., "How comfortable do you feel doing math problems?"; α = .86 - .92, across the seven studies), as was initial interest in statistics (e.g., "To what extent do you find statistics interesting?"; α = .86-.95). These items were adapted from Durik et al. (2015) and Hecht et al., (2020).

Outcome measures. In Studies 1-7, participants' utility value for regression was assessed after the learning session with four items (e.g., "How useful do you think linear regression could be in your future?"; α = .79-.86), distraction was measured with three items (e.g., "I got distracted as I watched the regression video."; α = .86-.93), and feelings of maintained situational interest in regression were measured with three items (e.g., "To what extent do you find linear regression interesting?"; α = .80-.89). In Studies 1-6, triggered situational interest in the instructional video was measured with two items (e.g., "It was fun to watch the video"; α = .72-.80, across six studies), but this measure was replaced in study 7 by a six-item measure of triggered situational interest in the learning session (e.g., "This session has been fun"; α = .92). All interest-related measures were adapted from Linnenbrink-Garcia et al. (2010), Durik et al. (2015), and Hecht et al., (2020).

In Studies 3-7, I also assessed participants' perceived autonomy during the learning session. I did so with four items in Studies 3-6 (e.g., "I learned regression the way I wanted to";

α = .69-75, across four studies) and added two additional items for Study 7 (e.g., "I've been given a chance to think for myself in this study"; α = .86). The measure of perceived autonomy was adapted from the Intrinsic Motivation Inventory (Ryan, 1982). In all studies, for the behavioral measure of emerging individual interest in statistics, I recorded whether participants provided their email, requesting information about on-campus and online regression resources (a binary outcome). In all studies, end-of-session tests were scored on a 1-21 scale (M = 13.99-15.57, SD = 3.74-4.53) using a rubric.

Table 2. Alphas for All Scales in Studies 1-7

Study	BCM	BIS	Distraction	UVR	TSI	MSI	Autonomy
1	0.92	0.95	0.91	0.86	0.72	0.88	--
2	0.88	0.90	0.90	0.85	0.74	0.88	--
3	0.86	0.92	0.93	0.83	0.78	0.80	0.75
4	0.90	0.91	0.91	0.84	0.80	0.86	0.74
5	0.87	0.86	0.89	0.81	0.78	0.84	0.69
6	0.89	0.90	0.90	0.84	0.76	0.88	0.73
7	0.90	0.87	0.86	0.79	0.92	0.89	0.86

Note. BCM = baseline confidence in mathematics; BIS = baseline interest in statistics; UVR = utility value for regression; TSI = triggered situational interest in the learning session; MSI = maintained situational interest in regression.

Table 3. Median Correlations Between Measures in Studies 1-7

	1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**
1. Baseline Confidence in Math	1.00								
2. Baseline Interest in Statistics	0.45	1.00							
3. Distraction	-0.09	-0.18	1.00						
4. Utility Value for Regression	0.19	0.36	-0.24	1.00					
5. Triggered Situational Interest	0.07	0.31	-0.56	0.42	1.00				
6. Maintained Situational Interest	0.26	0.63	-0.38	0.61	0.63	1.00			
7. Perceived Autonomy	0.01	0.18	-0.37	0.32	0.51	0.46	1.00		
8. Requested Resources	0.07	0.23	-0.12	0.25	0.22	0.31	0.12	1.00	
9. Performance (Test Score)	0.44	0.24	-0.06	0.14	0.07	0.21	0.01	0.05	1.00

Manipulations in Each Study and Preliminary Results

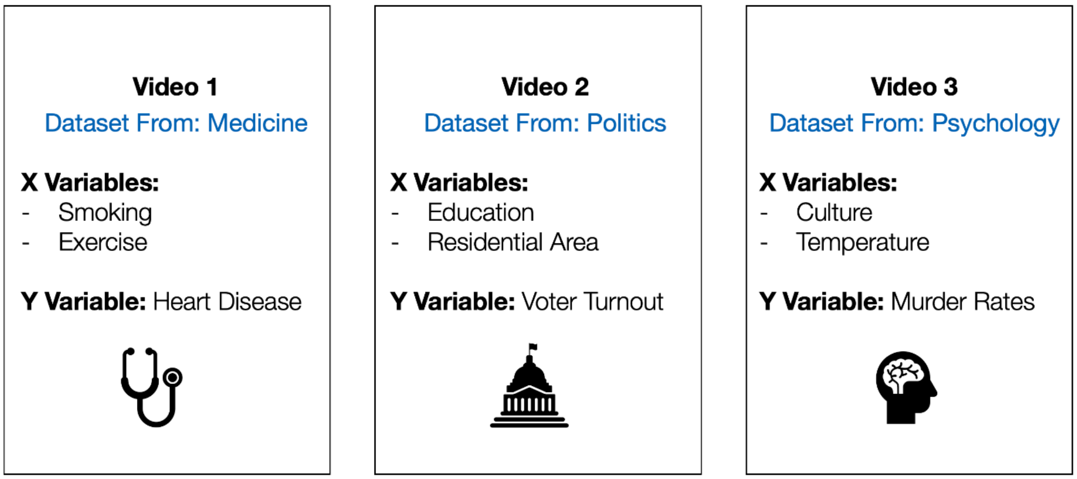
Directly-Communicated Utility Value Manipulation. In Study 1 (N = 115), I piloted the new, directly-communicated utility value manipulation, testing it against a control condition in a two-cell design. This manipulation took place before the instructional video, and it consisted of an approximately 3-minute video in which an instructor explains how linear regression has become a useful skill in many careers. Specifically, the instructor discusses how linear regression can be useful in medicine (for evaluating treatments and examining the causes of disease), politics (for forecasting voter behavior), and psychology (for answering questions about the causes of human behavior). I selected medicine and psychology as examples because these are common fields of interest for students who take introductory psychology, and I included politics because the study began in the Spring of 2020 when national news revolved around the upcoming presidential election. In the control condition, students watched a 3-minute video about the history of regression. In Study 1, I established that the utility-value manipulation was convincing: compared to control participants, those in the utility-value condition reported that regression was more useful, $d = .40$.

Study 2 (N = 256) was designed and preregistered (https://aspredicted.org/3zy5j.pdf) to replicate the effect of the utility value manipulation on utility value perceptions, to test if the utility-value manipulation was threatening for less-confident participants (replicating Canning & Harackiewicz, 2015), and to examine if any negative effects for this group of students could be mitigated by simultaneously bolstering students' confidence. Study 2 utilized a 2 x 2 design, crossing the utility-value manipulation with an attributional reframing message in which students learned that struggles with regression are common, unstable, and controllable (see Perry et al., 2014; Weiner, 1985). Specifically, participants were told that research shows "statistics classes are challenging for almost everyone at first, even those who wind up doing very well." And that "confusion is temporary, even for the most concerned students." Unlike Study 1, which used a video about the history of regression as a control activity for the utility value manipulation, control participants in Study 2 (and all subsequent studies) advanced to the instructional video without watching a video. This change was made to ensure that study results were not influenced by the content taught in the history of regression video.

Contrary to my predictions, I found no evidence that the utility-value manipulation threatened or undermined interest, engagement, or performance for less confident participants in Study 2. Instead, at all levels of confidence (on average), students who received the utility-value manipulation indicated stronger beliefs about the usefulness of regression, $d = .50$, and they also reported stronger feelings of maintained situational interest in regression, $d = .22$. In Study 2, the utility value manipulation didn't affect triggered situational interest in the instructional video, decrease distraction as students watched the video, or improve performance on the end-of-session test.

Choice Manipulation. To examine if I could more effectively promote interest, engagement, and performance by utilizing multiple triggers of interest, I crossed the directly-communicated utility value manipulation with a choice manipulation in Study 3 (N = 100) and Study 4 (N = 673). In the choice manipulation, which took place before the instructional video but after the utility-value manipulation (if applicable), participants were asked to choose between three versions of the regression video, each built around a different dataset. Figure 2 displays the choice that participants were given.

Figure 2. Choice Manipulation: The Three Videos that Participants Were Asked to Choose Between in Study 3



Video 1 was framed as involving a medical dataset, investigating the relationship between smoking rates, exercise, and heart disease in each U.S. state. Video 2 was framed as involving a political dataset, investigating links between education levels, residential area, and state-level voter turnout in presidential elections. Video 3 was identical to the video from Studies 1 and 2, and it was framed as being built around a dataset from psychological research, investigating causes of violence in U.S. states.

After making a choice, participants were shown the video that they selected. The three videos were built around different datasets and research questions, but all contained the same regression content in the same sequence, datasets with the same number of observations (48; one for each continental U.S. state), similar scripts, and stylistically identical animations and figures. To control for the possibility that the videos differed in their intrinsic appeal to participants, I utilized a yoked design to ensure that each video would be assigned in control conditions the same number of times as it was chosen in the utility-value conditions (see Patall et al., 2008).

Each participant who was randomly assigned to a choice condition was paired with a "no-choice" participant, and whatever video the first participant chose was assigned to the other.

Study 3 confirmed that the choice manipulation increased participants' perceived autonomy during the session, *d* = .66, and the results suggested that the choice manipulation may also have affected participants triggered situational interest in the instructional video, *d* = .34 (although this effect was not significant with 100 participants).

In a preregistered analysis for Study 4, see https://aspredicted.org/NZB_LP4 and Asher and Harackiewicz (2023), I (1) replicated the effects of choice from Study 3 (with significant results on perceived autonomy, *d* = .99, and triggered situational interest, *d* = .26), (2) found that the choice manipulation decreased self-reported distraction during the video, *d* = -.19, and increased maintained situational interest in regression, *d* = .18, (3) found that the utility-value manipulation affected participants' feelings of maintained situational interest in regression, *d* = .13, but not their triggered situational interest in the instructional video (replicating Study 2), and (4) learned that the utility-value manipulation increased the odds that participants requested resources about statistics opportunities on campus, *OR* = 1.63.

In Study 5 (N = 171), I attempted to disentangle the extent to which the choice manipulation triggered situational interest by promoting feelings of autonomy vs. by allowing participants to learn about regression in a context that matched their interests. I did so by introducing a "perceived choice" condition where participants were given a choice between only two regression videos, one of which had been pilot tested to be unappealing for most participants: a video with a "materials science" dataset involving the relationship between the density of embedded fibers, the type of the fibers, and the strength of plastic. Accordingly, I predicted that participants in this condition would experience a sense of autonomy, but they would be unable to pick a video that matched their interests to the same extent that participants in the standard choice condition could. Rather than running this study with a 2 (D-UV vs. Control) x 3 (No choice vs. perceived choice vs. standard choice) design, I opted to include only four cells to maximize power to address questions about the effects of D-UV and the two choice manipulations: the standard choice condition (to serve as a reference group), the perceived choice condition, a standard choice + directly-communicated utility value condition (to gather additional data about the effects of the utility-value manipulation), and a control condition (to gain more information about the effects of standard choice).

As predicted, the perceived choice manipulation worked: participants in the perceived choice condition were much less likely to opt for the materials science video than the alternative video offered. Participants in this condition also did not significantly differ from the standard choice condition on perceived autonomy or any other outcome. Given the relatively low number of participants in this study (with between 39 and 46 per condition), it was unclear if the null results indicated that effects of the choice manipulation are driven by perceived autonomy rather than interest matching, or if they simply reflect a lack of statistical power.

Combined Utility Value Manipulation. Finally, in Studies 6 and 7 I explored whether the directly-communicated utility value manipulation, administered at the beginning of the learning session, could be enhanced with a reflective writing activity that was administered at the end of the learning session, in a "combined" utility value manipulation. In these studies, a set of three quotes was added to the directly-communicated utility value manipulation. Each quote was attributed to a college student, and each discussed the usefulness of regression. These quotes, which are displayed in Table 4, were shown to participants after the instructional video concluded. Otherwise, the directly communicated manipulation was identical to the version in Studies 1-5.

Table 4. Quotes from the Reflective Writing Activity, Studies 6 and 7

How [University Name] Students Use Regression

Now that you've been introduced to linear regression, we want to show you how [university name] students are using it in their lives and work. We surveyed students on campus who are learning about or using regression, and we asked them about how they use or plan to use it. Here are three of their responses.

Quote 1: from a third-year psychology major:

I work in a lab where we study loneliness in children, looking at the factors that influence the way kids form friendships, and why some struggle with this. For instance, children have different expectations about what a friend should do, and we also consider factors like how many siblings they have. With regression we can test which particular factors are related to making close friends and which are associated with difficulties. I got involved because I want to be a school counselor and help kids who are struggling to make friends.

Quote 2: from a second-year business student:

I'm learning about regression now in some of my courses, and when I talked to my aunt about this, she told me that she uses regression at the real-estate company where she works. Each time they appraise a new house, she uses data from houses that sold in the same area, figuring out how things like neighborhood, number of bedrooms, square footage, and amenities are related to price. She can then use the regression model to predict the best sale price for the new house.

Quote 3: from a fourth-year health promotion and health equity major:

I started out as a pre-med student, but I switched to health promotion and health equity because I realized that lots of people don’t even have access to doctors, and I want to help make sure everyone can get medical care when they need it. In this work, it’s important to understand exactly which barriers are most critical. For example, poverty, education, and living in a rural area might all limit access. But these variables are all related to each other, so regression is needed to figure out which is the most important.

In the combined utility value condition, participants used these quotes as the starting point for a reflective writing activity. This quote-based approach for a self-generated utility value manipulation has been used in field experiments that have taken place in algebra classrooms, with the goal of providing students with structure (via the quotes) that allows them to meaningfully reflect about the usefulness of math (Gaspard et al., 2015, 2021). In Studies 6 and 7, participants were asked to select the quote that they related to most and explain why they selected this quote, and then write about how knowledge of regression might be useful in their own lives. Reflective writing is the central component of self-generated utility value manipulations, and I reasoned it might enhance the directly communicated manipulation for at least two reasons. First, the reflective writing activity could increase the extent to which participants focus on the *personal* usefulness of regression, strengthening effects of the directly-communicated utility value manipulation on maintained and emerging individual interest (phases of interest development that involve the appreciation of personal importance and meaningfulness). Second, like the choice manipulation, the reflective writing activity might promote a sense of self-determination. Because the reflective writing activity asks participants to make several meaningful choices (they select their preferred quote and then choose what to write about in a personal reflection), it could also promote feelings of autonomy and therefore triggered situational interest, engagement, and possibly performance.

In Study 6 (N = 377), participants in a directly-communicated condition (the reference group) were compared to those in the new combined condition. Two additional conditions were also run for the sake of replication: (1) a control condition (allowing for an additional test of directly-communicated utility value vs. control) and (2) a condition with both directly-communicated utility value and choice (allowing for an additional test of the combined effects of directly-communicated utility value and topic choice).

In Study 7 (N = 327), participants in a combined utility-value condition served as the reference group and were compared to a combined utility value condition with a brief version of the reflective writing activity. In this version, students read the three quotes, but I shortened the activity by removing the quote selection and evaluation portion; participants completed the personal reflection but were not asked to select the quote that they related to most and discuss why this was the case. The purpose of this condition was to test if any effects of reflection were in fact due to quote choice, as opposed to personal reflection. Two additional conditions were also run to test for replication of prior studies: (1) a directly-communicated utility value condition (enabling another test of combined utility value vs. directly-communicated only), and (2) a condition with directly-communicated utility value and task choice (providing an additional test of adding choice). Unlike Studies 1-5, Studies 6 and 7 were not analyzed prior to this meta-analysis.

Table 5 shows a summary of all conditions run in Studies 1-7.

Table 5. Conditions Run in Studies 1-7

Study	Condition 1	Condition 2	Condition 3	Condition 4
1	Control	D-UV	--	--
2	Control	D-UV	AR	D-UV + AR
3	Control	D-UV	Choice	D-UV + Choice
4	Control	D-UV	Choice	D-UV + Choice
5	Control	Choice*	D-UV + Choice	Perceived Choice
6	Control	D-UV *	D-UV + Choice	Combined UV
7	D-UV	Combined UV*	Combined UV (Brief Version)	Combined UV+ Choice

Note: "D-UV" refers to the directly-communicated utility value manipulation, in which participants watched a lecture about the usefulness of linear regression for different careers;

“Combined UV” refers to a manipulation that combines directly-communicated utility value and a reflective writing activity. In the reflective writing activity, participants selected a quote about the usefulness of regression, described why the selected the quote, and then discussed how regression could be useful for them. “Combined UV (Brief Version)” refers to a version of the combined manipulation with an abbreviated version of the reflective writing activity (with no quote selection or evaluation). “Choice” indicates that participants were allowed to select the dataset used for examples in the regression video. *For studies 5-7, an asterisk in in each row denotes the reference group.

Analysis Plan

Studies 1-7 were analyzed in two stages: first, an initial analysis of each study using a common model, and second, a meta-analysis of the results from stage one. This meta-analytic procedure, described below, was preregistered at https://osf.io/h953d. With it, I address five broad research questions and a number of corresponding, narrower hypotheses.

1. How did the two primary manipulations tested in studies 1-7 (directly-communicated utility value and choice) affect triggered situational interest and distraction (a related outcome) during a learning session? Prior research on the effects of choice led us to hypothesize that the choice manipulation should promote triggered situational interest and decrease distraction, and I wanted to explore how effects of the utility-value manipulation compared. Relatedly, I wanted to test if effects on interest and task engagement would translate to improved performance on an end of session test about regression.
2. How did the two primary manipulations affect measures of deeper interest (i.e., maintained situational interest and emerging individual interest)? Because beliefs about utility value and meaningfulness are thought to be strongly implicated in the development of these deeper interest, I expected that the directly-communicated utility value manipulation might have stronger effects on measures of maintained situational interest and emerging individual interest.

3. Did the directly-communicated utility value have stronger effects for more confident participants, and was it more impactful for participants who entered the session with higher levels of initial interest? I predicted that this manipulation might only benefit confident participants (because a skill only seems useful if you're confident that you can perform it), and I expected that utility value might only promote deeper interest for participants with higher levels of initial interest (because interest must first be triggered before it can be maintained and internalized).
4. Did the directly-communicated utility value and choice manipulations interact? On one hand, because both manipulations target interest, their combined effects may be weaker than the sum of their separate effects. On the other, if the two manipulations promote different types of interest or help different groups of students, the two manipulations might have additive benefits. I made no directional predictions for this research question.
5. Does adding reflective writing to the directly-communicated utility value manipulation make it more powerful at promoting deeper phases of interest development in regression (i.e., maintained situational interest and emerging individual interest), increasing perceived autonomy, decreasing distraction during the instructional video, or improving performance? A reflective writing activity could reinforce the message of the directly-communicated manipulation and make it more personally relevant. Thus, I hypothesized that the combined utility value manipulation might be more effective than the directly-communicated manipulation at promoting these outcomes.

Analysis Stage 1: Individual Study Regressions

In stage 1 of the analytic process, Studies 1-7 were analyzed with multiple regression, using a general linear model for continuous outcomes and logistic regression for the behavioral measure of maintained situational interest, a dichotomous outcome.

In all studies that allowed for an unconfounded test of the directly-communicated utility value manipulation (Studies 1-6), I regressed each outcome on a model that included (1) a unit-weighted "D-UV" contrast that compared the directly-communicated utility value condition(s) to a control condition (or conditions) that lacked the directly-communicated manipulation but were otherwise identical, and (2) interactions between the D-UV contrast and two moderators: participants' baseline interest in regression and perceived competence for math.

In all studies that allowed for an unconfounded test of the choice manipulation (Studies 3-7), I included a unit-weighted "choice" contrast. When the choice contrast was part of a 2 x 2 design (Studies 3-4), it was interacted with the D-UV contrast to address research question 4.

In Studies 6 and 7, which tested the effects of adding reflective writing to directly-communicated utility value manipulations, I (1) included a unit-weighted "combined UV" contrast that compared the combined utility value condition (including both directly-communicated utility value and reflection) to the directly-communicated condition, and (2) interactions between the reflection contrast and the two moderators: baseline interest in regression and perceived competence for math.

In addition, contrasts were included to test for effects of any additional experimental manipulations that were conducted in Studies 1-7. Because Study 2 had a 2 (Directly-communicated utility value vs. Control) x 2 (Attributional reframing vs. Control) design, the attributional reframing contrast in this study was interacted with the D-UV contrast.

Table 6 summarizes the regression model for each study.

Table 6. Regression Models for Initial Analysis of Studies 1-7

Study	Contrast 1	Contrast 2	Contrast 3
1	D-UV vs. Control (.5, -.5)*	--	--
2	D-UV vs. Control (.5, -.5)*	Attributional Reframing vs. Control (.5, -.5)	Contrast 1 x Contrast 2
3	D-UV vs. Control (.5, -.5)*	Choice vs. Control (.5, -.5)	Contrast 1 x Contrast 2
4	D-UV vs. Control (.5, -.5)*	Choice vs. Control (.5, -.5)	Contrast 1 x Contrast 2
5	D-UV + Choice vs. Choice (D-UV contrast) (1, 0)*	Control vs. Choice (choice contrast) (1, 0)*	Perceived Choice vs. Choice (extra contrast) (1, 0)*
6	Control vs. D-UV (D-UV Contrast) (1, 0)*	D-UV + Choice vs. D-UV (choice contrast) (1, 0)*	Combined UV vs. D-UV (combined UV contrast) (1, 0)*
7	Combined UV + Choice vs. Combined UV (choice contrast) (1, 0)*	D-UV vs. Combined UV (combined UV contrast) (1, 0)*	Combined UV (Brief) vs. Combined UV (extra contrast) (1, 0)*

Note: For each study, regression models contain the condition contrasts displayed in the table above. All contrasts marked with an asterisk* are interacted with participants' baseline confidence in math and interest in statistics. No additional predictors are included in the models. Cells highlighted in blue will be meta analyzed to test the effects of adding directly-communicated utility value, yellow cells will be meta analyzed to test effects of providing choice, green cells will be meta analyzed to test effects of adding reflective writing to a utility value manipulation, and red cells will be meta analyzed to test for effects of combining directly-communicated utility value and choice.

Prior to these initial analyses, all continuous dependent variables were standardized by subtracting their mean and then dividing the difference by their standard deviation after accounting for effects of experimental manipulations (i.e., the residual standard error from a model regressing the dependent variable on all condition contrasts from the study). This type of standardization, combined with the decision to unit weight all condition contrasts (either -.5/.5 or 0/1), means that regression coefficients for all condition contrasts can be interpreted as standardized mean differences between conditions. Full results from analyses of individual studies are provided in Appendix C.

Analysis Stage 2: Meta Regression

Random effects meta-regression models were fit using the "rma.mv" function in the "metafor" package in R (Viechtbauer, 2010). To address research questions 1 and 2, which involve the overall effects of each manipulation, I fit a set of models (one per outcome variable). Each model analyzed coefficients for the D-UV and Choice contrasts from the initial analyses of Studies 1-7, regressing them on a fixed intercept and a fixed effect for manipulation type (D-UV = 0 vs. Choice = 1), and including by-study random intercept and a by-study random slope for manipulation type. Each meta-regression model also used the variance-covariance matrices from the individual regressions as estimates of the sampling variances for each effect-size estimate and the covariances between non-independent effect size estimates in the same study (i.e., dummy codes).

To test for the average main effect of the directly-communicated utility value manipulation on each outcome, I examined the intercept of each model. To test if the choice manipulation had stronger or weaker effects on each outcome, I examined the fixed effect for manipulation type in each model. To explore if the choice manipulation had a significant effect on each outcome, I refit the models with the moderator recoded Choice = 0, D-UV = 1.

To test if the directly-communicated utility value manipulation was more effective for more interested or more confident students (research question 3), two meta-regression models were fit for each outcome: one meta-analyzing the D-UV x interest interaction coefficients and another meta-analyzing the D-UV x perceived competence interaction coefficients (from Studies 1-6). These models included a fixed intercept, a by-study random intercept, and no additional fixed or random effects. Like the previous models, they used the variance-covariance matrices from the individual regressions as estimates of the sampling variances for each effect-size estimate and the covariances between non-independent estimates in the same study.

To test if the directly-communicated utility value manipulation interacted with the choice manipulation (research question 4) I fit models meta-analyzing the D-UV x Choice contrasts from studies 3 and 4. To test if the directly-communicated utility value manipulation was improved by adding reflective writing (research question 5), I fit models meta-analyzing the "combined UV" contrasts from Studies 6 and 7 for each outcome. In addition, to test if effects of the reflective writing activity were moderated by participants' baseline interest or confidence, for each outcome I fit two additional models analyzing the "combined UV x interest" and "combined UV x perceived competence" interactions, respectively.

Results

Results from all meta-analyses are displayed in Table 7, and summarized in Figures 3, 4, 5, and 6.

Table 7. Results from Meta-Analyses of Studies 1-7.

	d	se	t	p
DV: Utility Value				
D-UV vs. Control	0.41	0.07	6.00	0.000
Choice vs. Control	0.09	0.07	1.18	0.239
Choice vs. D-UV	-0.33	0.08	-4.13	0.000
D-UV x Choice	0.17	0.29	0.58	0.561
Combined UV vs. D-UV	0.03	0.10	0.34	0.737
D-UV x Interest	-0.06	0.08	-0.72	0.474
D-UV x Confidence	0.02	0.06	0.30	0.764
Combined UV x Interest	0.05	0.19	0.25	0.805
Combined UV x Confidence	-0.13	0.12	-1.13	0.258
DV: Perceived Autonomy				
D-UV vs. Control	0.12	0.07	1.76	0.079
Choice vs. Control	0.92	0.06	15.01	0.000
Choice vs. D-UV	0.81	0.10	7.83	0.000
D-UV x Choice	0.04	0.14	0.26	0.795
Combined UV vs. D-UV	0.11	0.10	1.01	0.312
D-UV x Interest	-0.03	0.12	-0.24	0.813
D-UV x Confidence	0.06	0.07	0.89	0.376

Combined UV x Interest	0.13	0.16	0.85	0.398
Combined UV x Confidence	-0.03	0.22	-0.12	0.904
DV: Triggered S.I.				
D-UV vs. Control	0.08	0.05	1.67	0.094
Choice vs. Control	0.24	0.05	4.49	0.000
Choice vs. D-UV	0.16	0.08	2.03	0.043
D-UV x Choice	-0.28	0.50	-0.56	0.573
Combined UV vs. D-UV	0.11	0.10	1.12	0.264
D-UV x Interest	-0.09	0.06	-1.53	0.125
D-UV x Confidence	0.02	0.06	0.36	0.716
Combined UV x Interest	0.17	0.11	1.54	0.123
Combined UV x Confidence	0.01	0.11	0.09	0.930
DV: Distraction				
D-UV vs. Control	0.01	0.06	0.15	0.881
Choice vs. Control	-0.15	0.06	-2.39	0.017
Choice vs. D-UV	-0.16	0.08	-1.92	0.055
D-UV x Choice	0.07	0.14	0.46	0.644
Combined UV vs. D-UV	-0.05	0.11	-0.43	0.666
D-UV x Interest	-0.07	0.06	-1.20	0.231
D-UV x Confidence	0.17	0.06	2.76	0.006
Combined UV x Interest	-0.12	0.12	-1.06	0.289
Combined UV x Confidence	0.00	0.12	0.01	0.991
DV: Maintained S.I.				
D-UV vs. Control	0.15	0.04	3.79	0.000
Choice vs. Control	0.16	0.04	3.67	0.000
Choice vs. D-UV	0.01	0.06	0.15	0.881
D-UV x Choice	0.09	0.10	0.87	0.382
Combined UV vs. D-UV	0.06	0.09	0.72	0.471
D-UV x Interest	-0.08	0.05	-1.74	0.082
D-UV x Confidence	0.04	0.05	0.89	0.371
Combined UV x Interest	0.25	0.10	2.46	0.014
Combined UV x Confidence	-0.02	0.20	-0.08	0.933
DV: Requested Resource				
D-UV vs. Control	0.30	0.15	2.03	0.042
Choice vs. Control	0.02	0.15	0.14	0.885
Choice vs. D-UV	-0.28	0.19	-1.50	0.134
D-UV x Choice	0.24	0.31	0.76	0.450
Combined UV vs. D-UV	0.05	0.23	0.20	0.842
D-UV x Interest	0.04	0.19	0.22	0.824
D-UV x Confidence	0.01	0.14	0.05	0.962

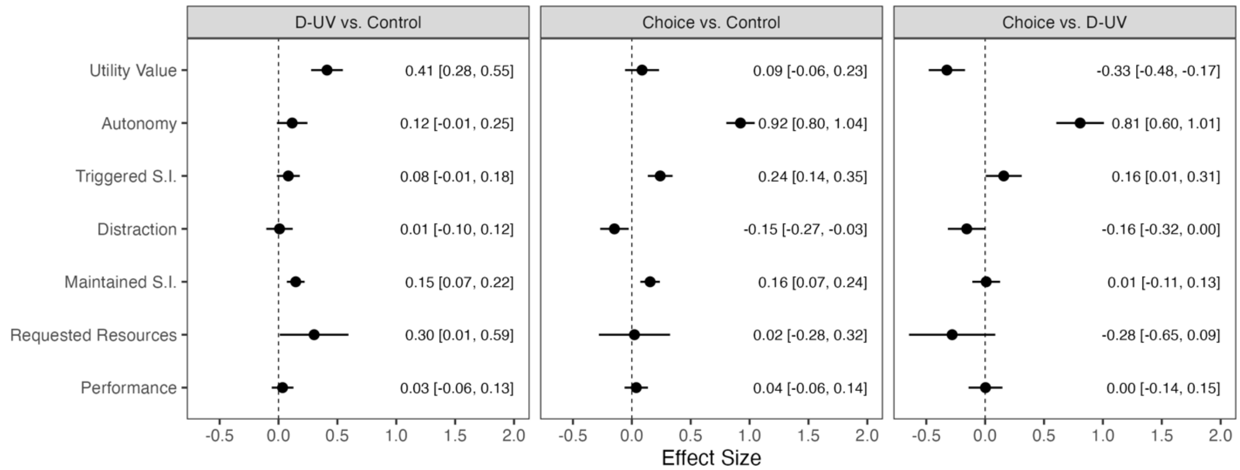
Combined UV x Interest	0.30	0.26	1.15	0.251
Combined UV x Confidence	-0.19	0.26	-0.73	0.468
DV: Performance				
D-UV vs. Control	0.03	0.05	0.73	0.463
Choice vs. Control	0.04	0.05	0.74	0.458
Choice vs. D-UV	0.00	0.07	0.04	0.967
D-UV x Choice	-0.05	0.13	-0.39	0.695
Combined UV vs. D-UV	-0.01	0.10	-0.10	0.920
D-UV x Interest	-0.01	0.11	-0.12	0.908
D-UV x Confidence	-0.04	0.06	-0.64	0.523
Combined UV x Interest	-0.17	0.11	-1.52	0.130
Combined UV x Confidence	0.00	0.11	0.01	0.992

Note. For each outcome, results from multiple meta-regressions are compiled in this table. Contrast names indicate the two groups being compared, and the level of the contrast that is coded "high" is listed first. Consequently, positive values of *d* indicate higher scores for the first group (e.g., positive values of *d* for "Choice vs. D-UV" indicate higher scores in the choice condition than the directly-communicated utility value condition).

Effects of the Directly-Communicated Utility Value and Choice Manipulations

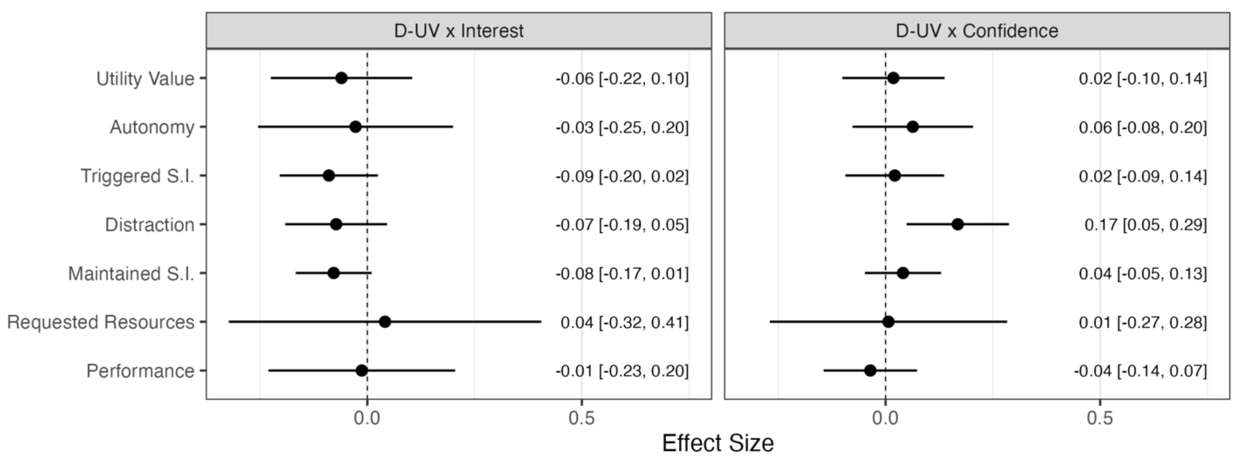
For each outcome, Figure 3 displays the overall effects of the directly-communicated utility value manipulation (left panel), the overall effects of the choice manipulation (center panel), and the difference between the two manipulations (right panel). Figure 4 displays the meta-analyzed coefficients for interactions between the directly-communicated utility value intervention and both baseline interest (left panel) and baseline confidence (right panel)

Figure 3. Effects of Directly-Communicated Utility Value and Choice Manipulations



Note. Points display effect size estimates and whiskers indicate 95% confidence intervals.

Figure 4. Moderation of Directly-Communicated Utility Value Effects by Participants' Baseline Interest and Confidence



Note. Points display effect size estimates and whiskers indicate 95% confidence intervals.

Utility Value for Regression. Across all studies, the directly-communicated utility value manipulation had a strong, positive effect on participants' beliefs about the usefulness of regression, boosting them by over .4 standard deviations relative to control, $d = .41$, $p < .001$. This overall effect was unmoderated; the directly-communicated utility value manipulation did not significantly interact with participants' baseline confidence in math or interest in statistics, *p*s

> .473. The choice manipulation was significantly less effective at promoting utility value beliefs; the average effect of the choice manipulation on this outcome was .33 standard deviations smaller than that of the directly-communicated utility value manipulation, $p < .001$. Relative to control, the effect of the choice manipulation on utility value beliefs did not significantly differ from zero, $d = 0.09$, $p = .239$.

Effects of Manipulations on Perceived Autonomy. The choice manipulation boosted participants' perceptions of autonomy during the learning session by .82 standard deviations relative to control, and by .81 standard deviations relative to the directly-communicated utility value manipulation, *p*s < .001. Relative to control, the effect of the directly-communicated utility value manipulation on perceived autonomy did not significantly differ from zero, $d = .12$, $p = .079$. The directly-communicated utility value manipulation did not interact with participants' baseline confidence in math or interest in statistics, *p*s > .376.

Triggered Situational Interest in the Instructional Video. Relative to control, there was no significant effect of the directly-communicated utility value manipulation on triggered situational interest in the instructional video, $d = .08$, $p = .094$. The choice manipulation increased triggered situational interest by .24 standard deviations relative to control, p < .001, and this effect size was significantly stronger than that of the directly-communicated utility value manipulation, $p = .043$. The directly-communicated utility value manipulation did not interact with participants' baseline confidence in math or interest in statistics, *p*s > .125.

Self-Reported Distraction. Participants in the directly-communicated utility value conditions reported comparable levels of distraction during the instructional video as those in the control condition, $d = .01$, $p = .881$. The choice manipulation decreased distraction by .15 standard deviations relative to control, $d = -.15$, $p = .017$, an effect size that was .16 units stronger than that of the directly-communicated utility value manipulation, p = .055. The directly-communicated utility value manipulation did not interact with participants' baseline interest in math, p = .289, but it did interact with their baseline interest in statistics, b =.17, p = .006, suggesting that a crossover interaction in which the directly-communicated utility value manipulation helped those with less initial interest focus during the regression video, but it increased distraction for those with higher levels of initial interest.

Maintained Situational Interest in Regression. The directly-communicated utility value manipulation boosted participants' feelings of maintained situational interest in regression by .15 standard deviations, relative to control, p < .001. Relative to control, the choice manipulation boosted maintained situational interest by .16 standard deviations, p < .001. These two effect sizes did not differ, p = .881. The effect of the directly-communicated utility value manipulation on maintained situational interest was not moderated by participants' baseline confidence in math p = .371. A negative but non-significant interaction between the directly-communicated utility value manipulation and baseline interest, b = -.08, p = .082, indicates that the manipulation may have been more effective at promoting maintained situational interest for participants who entered the session with lower levels of initial interest in statistics.

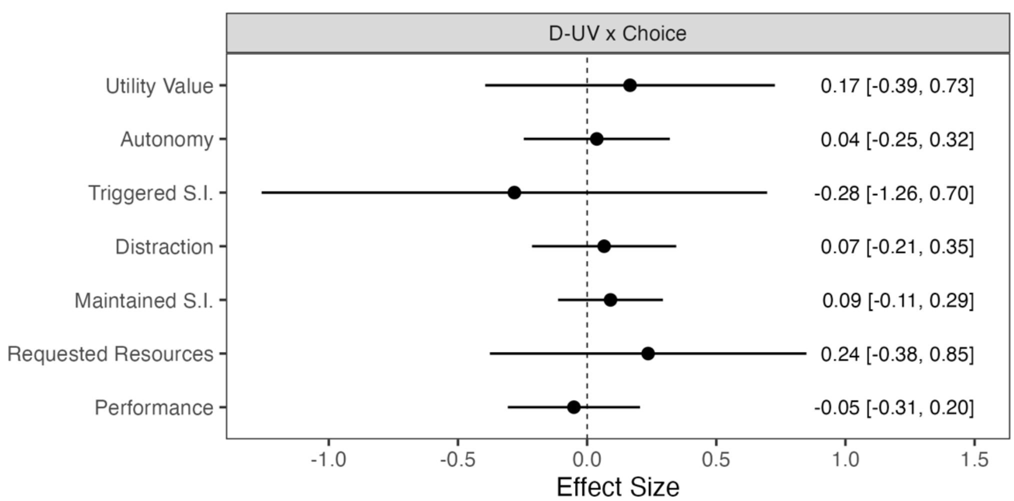
Requesting Regression Resources (a Behavioral Indicator of Emerging Individual Interest). On average, the directly-communicated utility value manipulation increased the odds that participants requested regression resources by 1.35x relative to control, OR = 1.35, p = .042, suggesting that the manipulation promoted emerging individual interest in statistics. In unadjusted percentages, 39.2% of participants in directly-communicated utility value conditions requested resources vs. 33.2% of participants in control conditions. In contrast, the choice manipulation had no influence on this outcome, OR = 1.02, p = .885. The difference between these two odds ratios was not significant, $p = .133$. The overall effect of the directly-communicated utility value manipulation was unmoderated; the manipulation did not significantly interact with participants' baseline confidence in math or interest in statistics, *p*s > .824.

Performance. Relative to control, there was no significant effect of the directly-communicated utility value manipulation on performance, $d = .03$, $p = .463$, nor was there an effect of the choice manipulation, $d = .04$, $p = .458$. The directly-communicated utility value manipulation did not interact with participants' baseline confidence in math or interest in statistics to influence performance, *p*s > .522.

Directly-Communicated Utility Value x Choice Interactions

Figure 5 displays the average directly-communicated utility value x choice interaction coefficient from Studies 3 and 4 for each outcome (combined N = 773). For all outcomes, there were no significant interactions between the two manipulations, *p*s > .382.

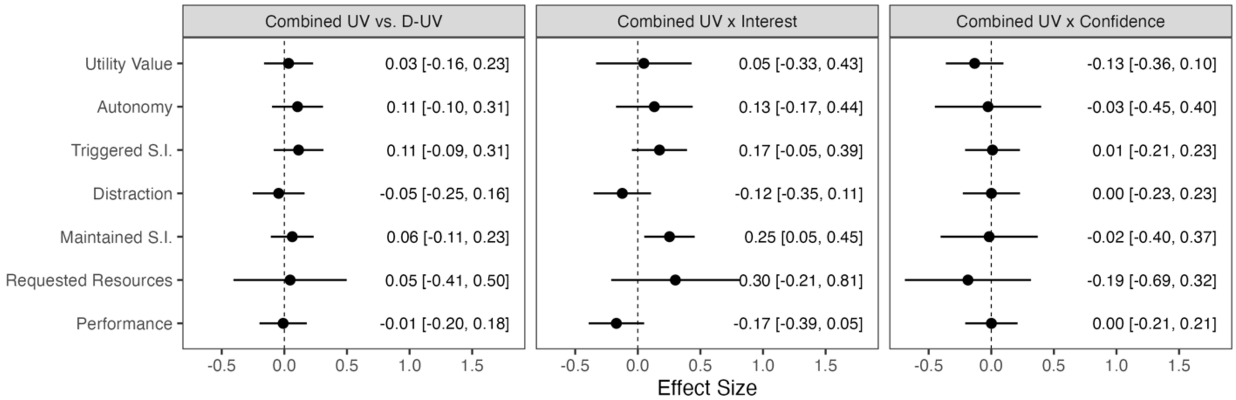
Figure 5. D-UV x Choice Interactions



Note. Points display effect size estimates and whiskers indicate 95% confidence intervals.

Effects of Adding Reflective Writing

Figure 6. Effects of Adding Reflective Writing

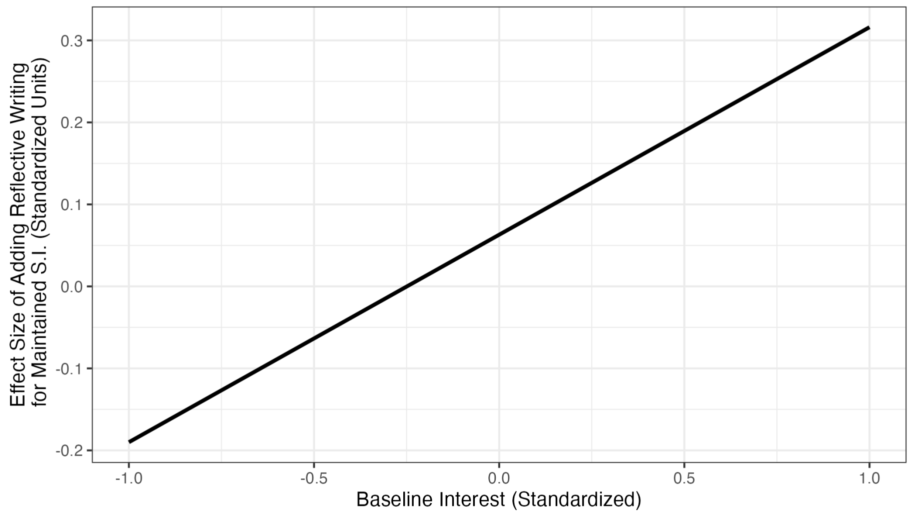


Note. Points display effect size estimates and whiskers indicate 95% confidence intervals.

In the left panel, Figure 6 displays average coefficients that show the effects of adding reflective writing to the directly-communicated manipulation in Studies 6 and 7 (combined N = 704). Although all effects of adding reflective writing were in the desirable and predicted direction—the manipulation was associated with higher levels of utility value, autonomy, all types of interest, and less distraction—effect sizes were small and non-significant. Standardized mean differences were less than .12 units for continuous outcomes, *ps > .26,* and the odds ratio for requesting resources was 1.05, p = .842.

In the center and right panels, Figure 6 displays tests of whether the effects of adding reflective writing varied as a function of participants' baseline interest in statistics or confidence in mathematics. Of the 12 interactions tested, only one was significant: a positive interaction between reflection and baseline interest when predicting maintained situational interest in statistics, b = .25, p = .014. When paired with the somewhat positive but non-significant main effect of reflective writing on maintained situational interest, this interaction suggests that reflecting on usefulness was more effective in promoting maintained situational interest for students with higher levels of initial interest, Figure 7.

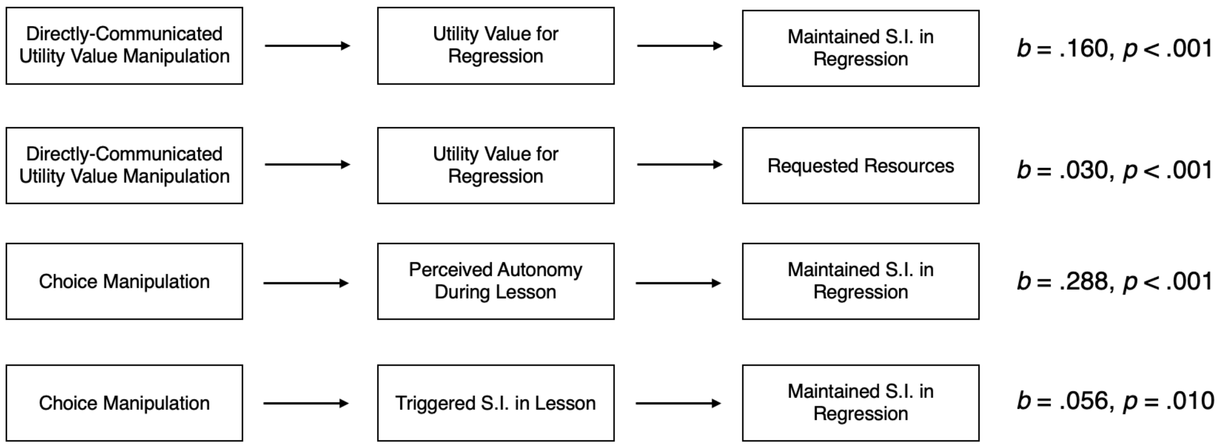
Figure 7. Effect Size of Adding Reflective Writing for Maintained Situational Interest, as a Function of Baseline Interest in Statistics



Mediation of Effects on Deeper Interest

In Studies 1-7, the utility value and choice manipulations promoted deeper interest in statistics. Both manipulations increased participants' self-reported maintained situational interest in regression, and the utility value manipulation also made it more likely that participants would request resources about statistics on campus, an indicator of emerging individual interest. To explore mechanisms by which these effects may have emerged, I tested the indirect effects depicted in Figure 8. I conducted this analysis in two stages. First, I fit path models in each of the seven studies to calculate the indirect effects, controlling for all terms from the regression model (i.e., baseline interest, baseline confidence, and all interactions) on both the "a" and "b" paths of the model, and using percentile bootstrapping to obtain standard errors. Second, I meta-analyzed these indirect effects, using their standard errors (squared) as estimates of their sampling variances.

Figure 8. Indirect Effects of Utility Value and Choice Manipulations on Measures of Deeper Interest



In this meta-analysis, I found evidence that perceived utility value mediated effects of the utility value manipulation on maintained situational interest in regression, $b = .16$, $p < .001$, and requesting regression resources $b = .030$, $p < .001$. I also found evidence that the choice manipulation may have promoted maintained situational interest via two different mediators: perceived autonomy, $b = .29$, $p < .001$, and triggered situational interest in the instructional video, $b = .06$, $p = .010$.

Discussion

First, meta-analysis of Studies 1-7 shows that a directly-communicated utility value manipulation discussing the career-based usefulness of linear regression can have strong, positive, and unmoderated effects on undergraduates' beliefs that regression is useful. Regarding the other outcomes in the study (i.e., the potential downstream consequences of changing this belief), an interesting pattern of results emerged. Overall, the directly-communicated utility value manipulation had no significant effect on participants' triggered situational interest in the instructional video, $d = .08$, and on average it did nothing to prevent participants from becoming distracted while watching the video, $d = .01$. However, the directly-communicated utility value manipulation did have significant and positive effects on the two measures of deeper interest.

Participants in directly-communicated utility value conditions reported stronger feelings of maintained situational interest in regression, *d* = .15, and their odds of requesting statistics-related resources were 1.35 times higher than those of participants in control conditions.

Taken together, these findings suggest that direct communications about usefulness may do little to trigger situational interest in a learning session (and therefore have small or null effects on situational effort, engagement, and learning), but at the same time directly-communicated utility value may be an effective means of promoting maintained situational interest, emerging individual interest, and longer-term engagement with content. Given the well-documented gap between individuals' intentions and subsequent actions (Webb & Sheeran, 2006), it is unlikely that effects of the directly-communicated utility value manipulation on requesting resources translated to differences in participants actually signing up for statistics courses. However, the fact that a 3-minute message about the usefulness of regression made participants more likely to disclose their email address and request resources 20 minutes later suggests that utility value can play a powerful role in promoting interest that begins to go beyond a situation.

The choice manipulation, on the other hand, had a strong effect on participants' feelings of autonomy during the learning session, *d* = .92, and had a different pattern of effects on the other outcomes. The choice manipulation increased triggered situational interest in the instructional video, *d* = .24, and it decreased self-reported distraction during the video, *d* = -.15. These two effects significantly differed from the null effects of the directly-communicated utility value manipulation on the same outcomes. Like the directly-communicated utility value manipulation, the choice manipulation promoted maintained situational interest in regression, *d* =

.16, but unlike the directly-communicated utility value manipulation, it had no impact on students requesting resources about regression and statistics, *OR* = 1.02.

The present research provides experimental evidence that supports several predictions from theories of interest development. In the four-phase model of interest development, triggered situational interest involves attention and engagement (Hidi & Renninger, 2006). By allowing participants to select a video that was intrinsically interesting to them, the choice manipulation grabbed participants' attention, thereby promoting engagement with the learning materials and triggering situational interest. The directly-communicated utility value manipulation, on the other hand, did little to appeal to attention—it simply informed participants that regression is useful and valuable—and it didn't promote engagement or trigger situational interest.

Both the utility value and choice manipulations promoted maintained situational interest in regression during the session, a construct that is theorized to involve two factors: (1) positive affect, and (2) the belief that content is important or meaningful (Linnenbrink-Garcia et al., 2010; Schiefele, 1991). Mediation analyses suggest that they may have done by each targeting a different factor—the choice manipulation brought about positive affect as participants learned (i.e., triggered situational interest), and the directly-communicated utility value manipulation influenced beliefs about importance and meaning. However, only the directly-communicated utility value manipulation affected interest in a manner that might go beyond the situation. Dewey (1913) and Mitchell (1993) both suggested that individual interest can emerge when individuals come to identify with content and believe that it can empower them to achieve their goals, and this finding provides evidence for this process. The findings from Studies 1-7 may suggest that utility value, more than positive affect and triggered situational interest, plays a role in promoting emerging individual interest.

But why did the directly-communicated utility value manipulation have unmoderated main effects on measures of maintained situational interest and emerging individual interest? The four-phase model of interest development suggests that students don't skip phases of interest; interest must be triggered before it can be maintained, and it must be maintained before it can become internalized and persist beyond a situation (Hidi & Renninger, 2006). Why then, wasn't the directly-communicated utility value manipulation more effective at promoting deeper interest for participants who entered the session with higher levels of individual interest? Or, put differently, why wasn't it ineffective for those with low levels of baseline interest?

There are several possibilities that might explain this finding. First, it could be the case that a large majority of participants had enough initial interest in statistics that the directly-communicated utility value manipulation could be effective for a large majority of participants. At first glance, this explanation for the lack of moderation appears to be unlikely; baseline interest in statistics was assessed on a 1-7 scale, and 53% of participants reported a level of baseline interest that was below 4, the scale's midpoint ($M = 3.64$, $SD = 1.29$). However, it could be the case that even a small amount of initial interest was sufficient for beliefs about value to promote deeper interest. Second, and more likely, is that even with over 2,000 participants, this study was underpowered to detect interactions with baseline interest or confidence. To investigate this possibility, I conducted a post-hoc power analysis with simulated data. The results of this analysis suggest that even when analyzed as a set, Studies 1-7 had less than 30% power to detect condition x interest (or condition x confidence) interactions on continuous outcomes like maintained situational interest. The assumptions, code, and results of this power analysis are detailed in Appendix D.

A lack of power to detect interactions could also have contributed to the finding that the directly-communicated utility value and choice manipulations combined in an additive (rather than interactive) manner to influence each outcome. However, it makes conceptual sense that the two manipulations can work independently of each other. Except for maintained situational interest, the two manipulations influenced different outcomes. And although they both promoted maintained situational interest, they likely did so via different mechanisms (utility value vs. autonomy and affect). This evidence suggests that multifaceted instructional approaches to promote interest via multiple mechanisms may be effective.

Finally, regarding the reflective writing activity, why did it provide so little benefit for promoting interest and engagement relative to the directly-communicated utility value manipulation, given the success of reflective writing in past studies of self-generated utility value manipulations? This finding may reflect differences between the regression paradigm and the mental multiplication paradigm. In nearly all prior laboratory studies involving self-generated utility value within the mental multiplication paradigm, benefits were concentrated among less confident students, who may have been threatened by directly-communicated utility value manipulations and benefitted from thinking about usefulness on their own terms (Durik, Hulleman, et al., 2015).

As suggested by Hecht and colleagues (2020) this threat-related finding may have emerged because math is a familiar and threatening subject for many students, and mental math techniques may require a high degree of mastery to be perceived as useful. Because linear regression is a new topic to most participants, and because of the way in which it was taught in the new paradigm (without complex mathematical jargon or symbols, and with a heavy emphasis on using computer software to do the math), it may have come across as relatively non-threatening. For these reasons, confidence may not have been as critical of a moderator.

In the study by Hecht and colleagues (2020), which involved a paradigm centered around the biology of fungus, baseline interest emerged as the most important moderator of utility value manipulations. Students with higher levels of initial interest benefitted most from learning about and reflecting on the usefulness of the content. I found a similar result involving the reflective writing activity in Studies 6 and 7; writing about the personal usefulness of regression was more effective at promoting maintained situational interest for participants who entered the session with higher levels of initial interest in statistics, and it may have undermined maintained situational interest for those who entered the session with lower levels of interest. This suggests that the act of reflecting about usefulness may only promote this deeper phase of interest development for students who already have some initial interest to inform and motivate their reflections.

Overall, Studies 1-7 demonstrate that beliefs about the usefulness of academic content can play an important and unique role in the development of deeper interest. Whereas meta-analyses showed that the choice manipulation was significantly more effective at promoting triggered situational interest and reducing distraction during the learning session (outcomes related to attention and engagement), the utility value intervention had benefits for promoting later phases of interest development, positively affecting a measure of emerging individual interest in statistics. This approach also points to the promise of conducting internal meta-analyses of a series of programmatic studies; with this approach I was able to gain considerable power, see through the noise that would arise from separately analyzing and verbally synthesizing seven studies, and even answer research questions about the relative magnitude of different manipulations for promoting different phases of interest development.

Study 8: A Utility-Value Intervention in an Online Algebra Tutor

Studies 1-7 suggest that utility value manipulations could be promising tools for instructors, helping move students deeper into the process of interest development. But do these laboratory findings hold in the real world? Can utility-value beliefs be promoted and sustained in authentic learning environments? In the present study, I was able to embed a series of utility-value interventions in MATHia, a software-based, online math tutor used by tens of thousands of algebra students (typically 13-15 years old) across the United States. In MATHia, students practice algebra concepts using an online curriculum that provides software-assisted guidance. The program is designed to be assigned by teachers during class time or for homework in lieu of traditional worksheets.

Online math tutors are becoming common in algebra classrooms because they can help teachers address students' diverse learning needs, and as such it's important to consider how they might best support students' engagement and motivation. Is it possible to embed interventions in this software that might change students' beliefs about the usefulness of algebra? And by doing so can an online utility value intervention make students more interested in mathematics, help students remain engaged as they practice within the tutor, and promote learning?

Because online math tutors track the individual actions that students' take as they solve problems, these platforms can provide researchers with insight into moment-to-moment intervention dynamics. By analyzing time-stamped keystrokes and mouse clicks, researchers can carefully explore how motivation interventions influence engagement over time. This enables fine-grained analyses of processes such as the fade-out of intervention effects over time.

Typically, this phenomenon has been documented over the course of weeks, months, and years (Bailey et al., 2020). However, online tutors allow researchers to track whether intervention effects fade out or persist across units of a curriculum or even from one problem to the next.

However, when designing a utility value intervention in an online algebra tutor, I faced challenges that did not arise in the laboratory studies discussed in Studies 1-7. First, for any intervention, contextual differences are critically important to consider. Without close knowledge of the context in which an intervention is implemented, it is difficult to anticipate whether an intervention's message will be received by students and supported by the environment (Harackiewicz & Priniski, 2018). Studies 1-7 took place in controlled, laboratory settings, keeping the general context similar across seven studies, minimizing distractions and ensuring a high degree of similarity between participants' experiences. Algebra courses, on the other hand, noisy, real-world settings with variability in factors including student characteristics (e.g., honors vs. remedial tracks), instructional practices (e.g., inquiry-based learning vs. direct instruction), and class size.

Features of classroom environments might undermine the success of an online utility-value intervention in many ways. For example, intervention messages may be easily missed by students who are distracted by their computers or peers (in contexts where classroom management is an issue) or their home lives (in contexts where the algebra tutor is assigned for homework). In addition, some algebra teachers might already emphasize the utility value of course content, making the intervention message redundant. Other teachers could undermine the message with their practices, perhaps by communicating that algebra has little real-world usefulness outside of the curriculum and assessments. Even if a utility value intervention is carefully designed and piloted with particular classrooms in mind, its efficacy will surely be limited in many contexts when it is deployed at scale (Bryan et al., 2021; Walton & Yeager, 2020).

Students' age, and how it affects their thinking, presents another challenge for utility value interventions in online algebra tutors. In Studies 1-7, which took place with college students, the directly-communicated utility value manipulation focused almost exclusively on the usefulness of regression for future careers. But will algebra students benefit from this thinking about usefulness for the distant future? Piaget (1955) suggested that the capacity to form abstract mental representations of time emerges during adolescence when formal operations develop. Over the course of adolescence, individuals begin to think more about the future, and extend their thoughts and plans further out in time (Nurmi, 1991). As such, compared to the college students who populated Studies 1-7 (mean age = 18.7 years), algebra students (typically aged 13-15) might think less about their future selves and future careers, and should be less motivated by the type of career-based utility value employed in the laboratory studies. For algebra students, it may be particularly important to focus on utility value for the near future, a period of time that is typically defined as 6 to 12 months for adolescents (Husman & Shell, 2008).

Another factor to consider is the familiarity of content**.** In Studies 1-7, the regression paradigm might have provided a particularly effective environment for promoting utility value beliefs because regression is a relatively new topic to most college students, and they haven't yet formed strong opinions about the utility value of statistics. In contrast, by the time a typical student reaches 9th grade algebra, they have accumulated over 1,000 hours of in-class math instruction, and many more hours outside of class thinking about math. Their interest in math and their beliefs about the utility value of math are informed by this experience, and it may be more difficult to change such strongly held beliefs with brief instructional manipulations.

To develop a utility-value intervention that might be able to successfully change students' motivation in the context of an online math tutor, I consulted prior literature on utility-value interventions.

Classroom Utility Value Interventions

Informed by laboratory work on utility-value manipulations, classroom utility value interventions have primarily been built around self-generated, reflective writing. In the first classroom test of this type of intervention, Hulleman & Harackiewicz (2009) randomly assigned ninth-grade science students to write about the usefulness of material that they were learning in class (the utility-value condition) or to summarize course material (the control condition). Students were given writing assignments every 3-4 weeks over the duration of a semester, so students completed up to eight essays each. Hulleman and Harackiewicz found that the utility value intervention helped students with low expectancies for success, boosting their interest and performance.

Gaspard and colleagues (2015) tested a utility value intervention across 82 German high school classrooms, consisting of a presentation about the usefulness of mathematics that was delivered by members of the research team, followed by activities that incorporated writing and reflection; thus, this intervention included a combination of directly-communicated and self-generated utility value. The research team reasoned that high school students may struggle to complete an open-ended utility value essay and that they may benefit from additional support to help them write about the usefulness of algebra. Consequently, they tested two different versions of a utility value writing activity: (1) a traditional, open-ended version in which students were asked to make a list of arguments for the personal relevance of mathematics, or (2) a more heavily scaffolded version in which students were asked to read and evaluate a set of six quotations from other students, describing ways that they use math in their lives. The presentation and writing exercises were delivered in a single, 90-minute session.

Compared to a waiting-list control condition, students in both utility-value conditions reported higher utility value for math, and those in the quote evaluation condition also reported higher intrinsic value. In a second study, the same research team (2021) attempted to scale up this procedure in an additional 78 classrooms, evaluating only the quote-evaluation condition against a waiting-list control condition, and training Masters' students and teachers to deliver the intervention in the place of the researchers. In this study, they replicated the positive effect on utility value, but not the effect on intrinsic value.

Utility value interventions have also been tested with college students in introductory biology. Harackiewicz and colleagues (2016) conducted a study in which biology students completed writing assignments about the usefulness of course content at three points during the semester. In this study, the utility value intervention improved course performance for all students on average, but it had an especially positive effect for students from underrepresented minority (URM) backgrounds, and the strongest effect for URM students who were also first-generation college students. When students were followed-up several years later, effects on persistence in the biomedical pipeline looked different: the intervention promoted persistence for more confident students, an effect that was mediated by the extent to which students wrote about personal themes in their utility value essays (Hecht et al., 2019).

The same team of researchers also conducted a series of studies testing individual features of the utility value intervention in college biology courses. For example, Canning and colleagues (2018) varied the number of utility value essays that biology students wrote and found that students who completed one or more essay benefitted, earning higher course grades and persisting in STEM majors at higher rates, on average. They also found that the timing of the essay was important when it came to promoting continuation: students who wrote a utility value essay in the third (and final) unit of the course were more likely to continue to the next course, but this wasn't the case for the first and second essay.

Most recently in this line of research, Harackiewicz and colleagues (2023) tested two versions of the utility value intervention in an introductory chemistry course: a "standard" utility value intervention in which students were asked to write about the personal utility value of course content, and a "prosocial-combined" utility value intervention in which students were asked to write about both personal and prosocial relevance (relevance for helping others). Results of the intervention varied for different subgroups of students. First-generation college students engaged more with the prosocial-combined essays, writing longer and better responses than their counterparts in other conditions. For confident first-generation students, this engagement translated to higher levels of end-of-semester interest in chemistry and better performance in the course. Among students from underrepresented and minoritized racial/ethnic (URM) groups (e.g., Black, Hispanic/Latino/Latina, and Indigenous students), the intervention strengthened intentions to major in the chemical and health sciences. When the research team followed up with students approximately 3 years later, students in both intervention conditions were 4 percentage points more likely to persist in STEM majors, and effects were larger for URM students, who were 14 percentage points more likely to persist after completing a utility value intervention (Asher et al., 2023).

These studies demonstrate that utility value interventions can support students' interest development, both at the high school and college level. Utility value interventions have promoted maintained situational interest in course material, increased engagement with course assignments, improved students' grades, and even shaped students' long-term individual interests (i.e., the courses that they took and majors that they pursued). This provides further experimental evidence that utility value is linked to the development of deep and enduring interest, shaping engagement and decision making over time. However, none of the intervention approaches taken in these studies can be easily transferred into an online algebra tutor at scale: all are too time consuming and would require significant buy-in and oversight from teachers.

To be successful in an online algebra tutor, a utility value intervention must be brief and unobtrusive, clearly communicate the usefulness of algebra in a manner that 8^{th} and 9^{th} grade students can relate to (e.g., without purely appealing to usefulness in the distant future) and ensure that students pay attention to and actively consider the intervention's message. As such, I chose to begin with the quote-evaluation approach taken by Gaspard and colleagues (2015, 2021). In this approach, participants were presented with quotes from former algebra students. In these quotes, the former students discussed how algebra had been useful to them in a variety of life domains, including both academic and non-academic activities. Students were then asked to rank the quotes based on how relevant each quote was to them and briefly write the reasoning for their rankings.

This intervention approach has promise in an online algebra setting. It can be administered in less than five minutes, quotes can come from students who have recently finished algebra (so that they seem relatable), quotes can be written to cover a range of topics and themes (e.g., usefulness for different interests, academic utility, personal utility, proximal utility, and future-based utility), and the ranking and reflection at the end of the exercise helps to ensure that students attend to the intervention materials. In the present study, I embedded quote-based utility value interventions in six units of MATHia's algebra curriculum.

The study, preregistered at https://aspredicted.org/D5Q_KD3, was designed to address the following research questions:

1. In the short-term, will the utility value intervention in an online math tutor affect students' maintained situational interest in relevant units of the algebra curriculum? Given the results of Studies 1-7, I predicted that the intervention should increase situational interest in algebra content.
2. Will the utility value intervention affect students' engagement with the online tutoring system? Will it help them remain on task and engage productively with the digital learning environment? And will the utility value intervention improve performance as students complete math problems? I found no evidence that utility value was linked to engagement or performance in Studies 1-7, and wanted to test if these null results would replicate with a larger sample and with behavioral measures of engagement.
3. What are the effects of dosage (i.e., number of interventions received) on motivation, engagement, and performance?
4. Do effects on engagement and performance fade out quickly over the duration of a learning session, or can they persist throughout?
5. Were effects of the intervention on motivation and performance maintained for several days following the intervention?

Method

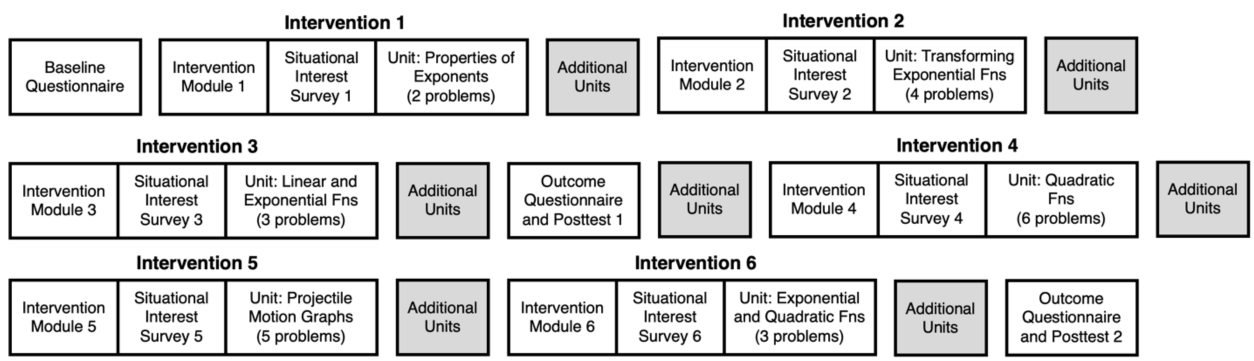
Participants

Data collection took place in 1,819 algebra classrooms at 491 U.S. schools between March and June of 2022. 12,824 students were assigned to a condition and make up the present sample. Demographic information about students (e.g., age, grade level, gender, and ethnicity) is not available due to data sharing agreements between MATHia and school districts.

Procedure

Six intervention modules were integrated into the beginning of six units in the MATHia curriculum. Figure 9 displays a timeline for the study.

Figure 9. Study Timeline



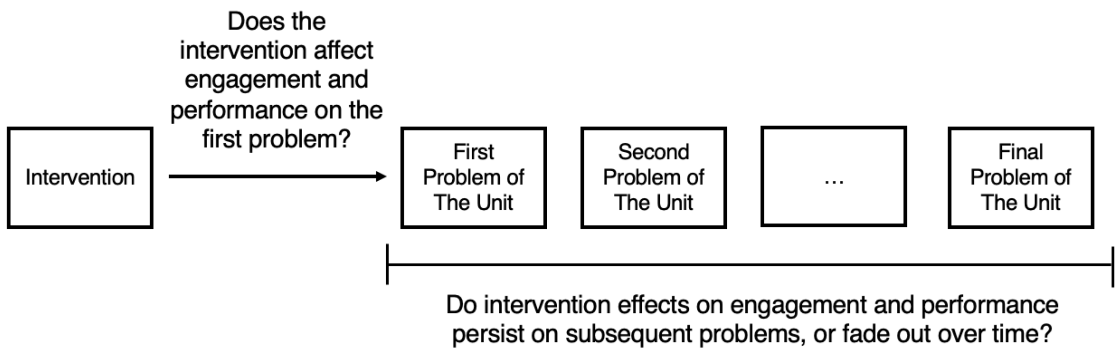
In the unit before the first intervention module, students completed a baseline questionnaire that assessed their beliefs about math's usefulness, their interest in math, and their confidence in their math abilities. Each time that a student reached an intervention module, they were randomly assigned to one of two conditions: the utility-value condition or a "business-as-usual" control condition. In the utility-value condition, students were presented with a video about the usefulness of algebra. Each video had a theme. In modules one and four the theme was using algebra outside of class, in modules two and five it was using algebra in other classes, and following modules three and six it was using algebra to help others. Each video consisted of three quotes from former or current algebra students that addressed this theme, and the quotes always revolved around specific content that was covered in the unit following each module. For example, the quote below was embedded in module 1, which took place during a unit on exponential growth and decay:

"I'm trying to become #instafamous. I started out by trying to post things that would get lots of likes, but I realized it's even more important if people share it or tag friends. So now I try to post things that will be interesting to a big group. If a few more people share my post, their friends see it too. Going viral is basically just exponential growth."

To facilitate student engagement and comprehension, the quotes in each video were narrated by a member of our research team. All quotes are displayed in Appendix E.

After watching the video, students were asked to select their favorite quote, discuss why they relate to this quote, and write a brief response about how they might use algebra in their own life. Students then completed a "situational interest survey," which assessed their situational interest in the relevant unit with a single item. In the control condition, students advanced straight to this survey without watching a video, being exposed to messages about algebra's usefulness, or completing any reflective writing activities. Immediately after the survey, students advanced to a problem set consisting of between two and six multi-step problems. As the students worked through these problems, the software collected a "log file" of time stamped actions taken by all students. These actions allow for measurement of whether students were engaged (i.e., on task and using the software productively) and answering problems correctly after each intervention, and they also afford me with the ability to examine whether effects on engagement and performance persisted or grew weaker on subsequent problems over time. Figure 10 provides a visual representation of how the study was designed to track short-term intervention effects on engagement and performance.

Figure 10. Tracking Short-Term Intervention Effects on Engagement and Performance



At two timepoints during the intervention, once following module three and again following module six, students completed an outcome questionnaire (assessing maintained situational interest in math, utility value beliefs, and confidence) and an assessment of the algebra content covered during the intervention. This questionnaire allows for an analysis of (1) longer-term change in motivation, and (2) the cumulative effects of receiving multiple doses of the intervention.

Study Completion

Although 12,824 students completed at least one intervention module, very few students completed the entire study. Students only completed all questionnaires and intervention modules if their teachers assigned all relevant units between March of 2022 and the end of the school year, and this was a rare occurrence. In MATHia, teachers move through the curriculum at different rates and in different sequences, and it is common for teachers to assign only a subset of the units to their students. As such, the module containing the baseline questionnaire was completed by only 27% of the sample (3,451 students), the module containing the first outcome questionnaire and posttest was completed by 9% of the sample (1,135 students), and the module containing the second outcome questionnaire and posttest was completed by 12% of the sample (1,563 students). Compounding this non-completion issue further, students who completed the baseline questionnaire were unlikely to advance far enough into the curriculum to complete an outcome questionnaire, making meaningful analyses of outcomes that involve baseline moderators or covariates impossible. Table 8 shows the distribution of participants who completed each questionnaire.

Table 8. Frequencies of Questionnaire Completion

Questionnaire Completion Pattern	N	%
Missing No Questionnaires	47	0.37%
Missing Baseline Only	45	0.35%
Missing Outcome T1 Only	82	0.64%
Missing Outcome T2 Only	483	3.77%
Missing Baseline and Outcome T1	1389	10.83%
Missing Baseline and Outcome T2	560	4.37%
Missing Outcome T1 and Outcome T2	2839	22.14%
Missing All Questionnaires	7379	57.54%

Non-completion was also common for the intervention modules. On average, students completed 1.7 out of six intervention modules, with 49% of students in the sample (N = 6,301) completing only one intervention module and 38% (N = 4,931) completing only two intervention modules. Table 9 displays the number of participants who completed 1-6 intervention modules, tabulated against the number of participants randomly assigned to an intervention condition 0-6 times.

Table 9. Distribution of Modules and Interventions Completed

	Interventions Completed							
Modules Completed	Zero	One	Two	Three	Four	Five	Six	Total
One	3261	3043						6304 (49%)
Two	1201	2516	1213					4930 (38%)
Three	128	376	373	112				989 (8%)
Four	19	80	109	70	16			294 (2%)
Five	7	35	69	79	34	1		225 (2%)
Six	2	8	19	28	16	7	2	82 (<1%)

Measures

Items for all measures are reported in Appendix F.

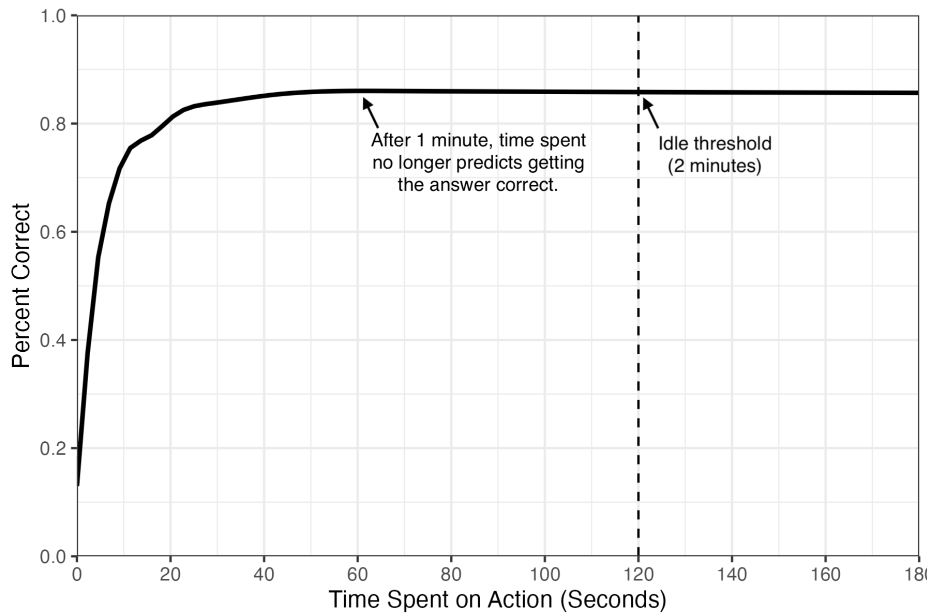
Baseline Questionnaire. Prior to the intervention, interest in mathematics was assessed with four items (e.g., "Math is interesting to me," $\alpha = .90$). Utility value for mathematics was assessed with three items (e.g., "Math will be important for my future," $\alpha = .73$) as was confidence in mathematics (e.g., "I'm good at math," $\alpha = .80$).

Situational Interest Survey for Each Unit. Following each assignment to condition and intervention (if applicable), students completed a one-item measure of maintained situational interest in the algebra unit ("The math I'm learning is interesting"). This measure was completed 22,047 times (1.7 times per student, on average): 10,864 times following an intervention, and 11,183 times in the control condition. I decided not to include an item assessing utility value for each module immediately after each intervention because of concerns that it would be subject to strong demand characteristics.

Behavioral Engagement. To be behaviorally engaged with an online learning environment, a student must be on task and using the system to learn or practice. As such, I computed two measures to assess students' behavioral engagement with the learning environment: a measure of "idle" behavior that flags when students have spent more than two minutes without interacting with the tutor, and a measure of "hint abuse" that flags when students are engaging unproductively with the system.

To determine the threshold for when a student was marked "idle," I consulted the distribution of time spent on each action in the tutor and I considered the relationship between time spent and correct responses. Across all actions, the median time spent was less than 7 seconds and the 75th percentile time spent was less than 17 seconds, indicating that typically, students were continuously engaged with the software. In addition, the relationship between time spent on an action and the probability of providing a correct response was strong and positive for this first 20 seconds, weak and positive between 20 and 60 seconds, and approximately flat after 60 seconds, Figure 11.

Figure 11. Relationship Between Time Spent on an Action and Probability of a Correct Response



Note. The smoothed curve summarizing the relationship between time spent and correct responses was produced with a generalized additive model.

As such, I set an idle threshold of 2 minutes, inferring that any action taking longer than this amount of time was unlikely spent engaged in productive work or help seeking (e.g., asking a teacher for assistance). With this threshold, students were flagged as idle on .8% of problem steps, students were flagged as idle at some point during 9% of problems, and 45% of students were flagged as going idle at least once during the study. Following each intervention module, I counted the number of times that a student was flagged as idle on each problem.

The measure of hint abuse was based on an approach taken by Paquette and colleagues (2014) to capture when students are progressing through an online tutor without meaningful engagement. When students request help from math tutoring software, initial hints are broad and conceptual, but with repeated requests the hints grow more and more specific. Eventually, the software reaches a "bottom-out" hint that provides the correct answer. Measures of hint abuse capture when students request hints to progress through the system without meaningful engagement (Baker et al., 2008). A problem was flagged as containing hint abuse if a student (1) submitted two hint requests for that problem with less than 2 seconds between them, and (2) solved the problem with a bottom-out hint. This behavior was common: 49% of students were flagged as abusing hints at least once and 21% of problems were flagged as being solved with hint abuse. Following each intervention module, I classified whether students abused hints to solve each problem, a dichotomous outcome.

Performance. Following each intervention, I tracked the proportion of correct steps taken by each student for each problem. A step was counted as correct if a student entered the correct answer on their first attempt, without first requesting a hint. Across all students and problems, 57% of first attempts were correct. In addition, at the end of the session 2,606 students completed a posttest of the material covered during the intervention. The average score on the test was a 58% (SD = 17%).

Outcome Questionnaire. Following two of the designated units, students completed a longer-term motivation questionnaire assessing maintained situational interest and utility value for math (rather than for a specific unit). The measure of utility value consisted of five items (e.g., "Math will be important to my future", $\alpha = .92$) and the measure of maintained situational interest in math consisted of four items (e.g., "I like doing math", $\alpha = .94$). 2,636 students completed at least one outcome questionnaire. Although approximately 19% of students completed this questionnaire on the same day as an intervention (or control) module, most students completed this questionnaire several days later (Median = 4.3 days). Table 10 displays the distribution of modules and interventions completed when students completed their final outcome questionnaire.

Table 10. Distribution of Modules and Interventions Completed Among Students with an Outcome Questionnaire

	Interventions Completed						
Units Completed	Zero	One	Two	Three	Four	Five	Six
One	525	498					
Two	292	554	309				
Three	44	110	125	34			
Four	3	12	19	16			
Five	1	13	23	17	17	0	
Six	0	4	6	7	5	1	1

Analysis

Short-Term Effects of Each Intervention. To assess the immediate, short-term impact of each intervention on maintained situational interest in each algebra unit, behavioral engagement, and performance, and to determine whether effects on these variables varied as a function of dosage, I fit a linear mixed effects model using the lme4 package in R (Bates et al., 2015). In this model, I included three fixed effects: a *Condition* contrast (Utility Value = .5, Control = -.5), a *Dosage* contrast indicating whether a student had received a previous intervention dose before (.5) or not (-.5), and an interaction term for these two contrasts. To account for the nested structure of the data (in which students are nested within classrooms and complete multiple interventions), I fit a three-level model that included by-classroom random effects and nested, by-student random effects. When I followed the recommendations of Barr et al. (2013) and included the "maximal" random effects structure (with by-classroom and by-student random slopes for all predictors) the model was too complex to converge. Accordingly, I followed the recommendations of Brauer & Curtin (2018) to investigate and remedy the non-convergence.

A primary cause of non-convergence was that 6,304 students completed only one intervention module. Because condition did not vary within these subjects, random slopes could not be estimated when these subjects were included in the dataset. To address this issue, I removed these subjects from analyses, resulting in a sample of 15,620 observations from 6,520 subjects who completed multiple intervention modules. The model with maximal random effects remained too complex to converge, so I sequentially removed the random slope for *Dosage* and removed covariances between the random effects. When the model still would not converge, I adopted an analytic strategy with two separate models.

First, to test the overall effects of the intervention, I used a model with *Condition* as the only predictor and the only random slope. Second, to test if the effects of the intervention changed as a function of prior dosage, I fit a second model with fixed effects for *Condition, Dosage*, and the *Condition* x *Dosage* interaction. In this model I followed the recommendations of Barr (2013) and only included a random slope for the interaction. Finally, when these models would not converge, I switched to a Bayesian approach, fitting the models specified above with weakly informative priors using the "brms" package in R (Bürkner, 2017). Using Bayesian models, I was able to achieve convergence.

To test if short-term intervention effects on idle behavior, hint abuse, and performance persisted or faded out as students worked through the problems following the intervention, I fit models regressing each of these three dependent variables on the *Condition* contrast, a "time" term (in which the first problem following the intervention was coded "0," the second problem was coded “1,” etc.)., and the *Condition* x time interaction. To get this model to converge, I included by-subject and by-classroom random intercepts, by-subject and by-classroom random slopes for the *Condition* x time interaction, and I used a Bayesian model with weakly informative priors.

Cumulative, Longer-Term Effects of Interventions. To assess the cumulative, longer-term impact of the utility value interventions on students’ end-of-study motivation and performance, I began with the sample of students who completed at least one intervention and an outcome questionnaire (N = 2,606). With these students, I fit linear mixed effects models regressing each longer-term outcome (utility value, maintained situational interest in mathematics, confidence, and performance) on a dosage term (i.e., the number of interventions completed by each student; 0-6), controlling for the number of modules that each student completed (1-6). I also included a quadratic term for dosage, testing whether longer-term effects of receiving a utility value intervention grew smaller or larger with each additional dose. Because students were nested in classrooms, I initially included a by-classroom random intercept and by-classroom random slopes for dosage, dosage-squared (the quadratic term), and the number of modules completed.

To get the model to converge, I sequentially (1) removed covariances between random effects, (2) removed the random slope for the covariate in the model (number of modules completed), and (3) fit Bayesian linear mixed effects models with weakly informative priors.

Results

Regression output for short term outcomes is presented in Table 11, regression output describing how short-term outcomes on performance and engagement changed over time is presented in Table 12, and regression output for longer term outcomes is presented in Table 13.

For all effects involving *Condition*, I present regression coefficients from the Bayesian models, 95% credible intervals, and the estimated probability that a given effect is greater than or less than zero (depending on the direction of the coefficient). Higher posterior probabilities indicate greater confidence than an effect differs from zero. I interpret all effects with 95% credible intervals that do not contain zero as strong evidence, and I also highlight several effects that have above an 80% posterior probability and are consistent with other findings.

To better understand the magnitude of effects reported below, I follow the recommendations of Muradoglu et al. (2023) for obtaining effect sizes that represent the standardized mean difference between conditions. All standardized effect size estimates must have a numerator, which represents the raw difference between conditions, and a denominator that scales the numerator by the amount of variability to be explained. For numerators, I use unstandardized regression coefficients, and for denominators I compute an estimate of the residual standard deviation in the dependent variable after accounting for the influence of the manipulation. Residual standard deviations are computed by regressing each dependent variable on a fixed effect for *Condition,* with by-classroom and nested by subject random intercepts, and then taking the square root of the summed variance components from each model.

Short-Term Effects

Table 11. Short Term Effects of Utility Value Interventions in Study 8

	Est	**LLCI**	**ULCI**	**pr**	**Est**	**LLCI**	**ULCI**	**pr**
DV: Maintained S.I. (Unit)								
Fixed Effects								
Intercept	3.11	3.074	3.143	> .999	3.15	3.113	3.186	> .999
Condition	0.04	0.010	0.071	.997	0.06	0.026	0.088	> .999
Dosage					0.11	0.073	0.139	> .999
Condition x Dosage					-0.07	-0.139	-0.009	.987
Random Effects								
By-Student SD (Condition)	0.16	0.022	0.307					
By-Class SD (Condition)	0.04	0.001	0.123					
By-Student SD (Cond. x Dosage)					0.23	0.016	0.528	

By-Class SD (Cond. x Dosage)					0.12	0.006	0.278	
DV: Hint Abuse on Problem 1								
Fixed Effects								
Intercept	0.17	0.158	0.176	> .999	0.18	0.174	0.193	> .999
Condition	0.00	-0.008	0.016	.708	0.00	-0.011	0.013	.581
Dosage					0.09	0.075	0.098	> .999
Condition x Dosage					-0.02	-0.041	0.008	.901
Random Effects								
By-Student SD (Condition)	0.03	0.001	0.071					
By-Class SD (Condition)	0.03	0.002	0.068					
By-Student SD (Cond. x Dosage)					0.08	0.004	0.197	
By-Class SD (Cond. x Dosage)					0.05	0.006	0.109	
DV: Idle (Count) on Problem 1								
Fixed Effects								
Intercept	0.18	0.168	0.190	> .999	0.18	0.171	0.194	> .999
Condition	-0.03	-0.046	-0.014	> .999	-0.03	-0.042	-0.010	.999
Dosage					0.02	0.003	0.036	.988
Condition x Dosage					0.01	-0.027	0.038	.633
Random Effects								
By-Student SD (Condition)	0.13	0.095	0.165					
By-Class SD (Condition)	0.02	0.001	0.057					
By-Student SD (Cond. x Dosage)					0.09	0.005	0.210	
By-Class SD (Cond. x Dosage)					0.06	0.003	0.136	
DV: Performance (Pct. Correct)								
Fixed Effects								
Intercept	0.48	0.470	0.484	> .999	0.47	0.464	0.479	> .999
Condition	0.00	-0.011	0.003	.844	0.00	-0.012	0.003	.883
Dosage					-0.03	-0.035	-0.019	> .999
Condition x Dosage					0.00	-0.012	0.018	.654
Random Effects								
By-Student SD (Condition)	0.03	0.001	0.068					
By-Class SD (Condition)	0.01	0.001	0.028					
By-Student SD (Cond. x Dosage)					0.03	0.002	0.078	
By-Class SD (Cond. x Dosage)					0.03	0.006	0.057	

Note. LLCI = Lower level of the 95% credible interval; ULCI = Upper level of the 95% credible interval. Pr = the probability that the associated estimate is greater or less than 0.

Table 12. How Short-Term Outcomes on Engagement and Performance Changed Over Time

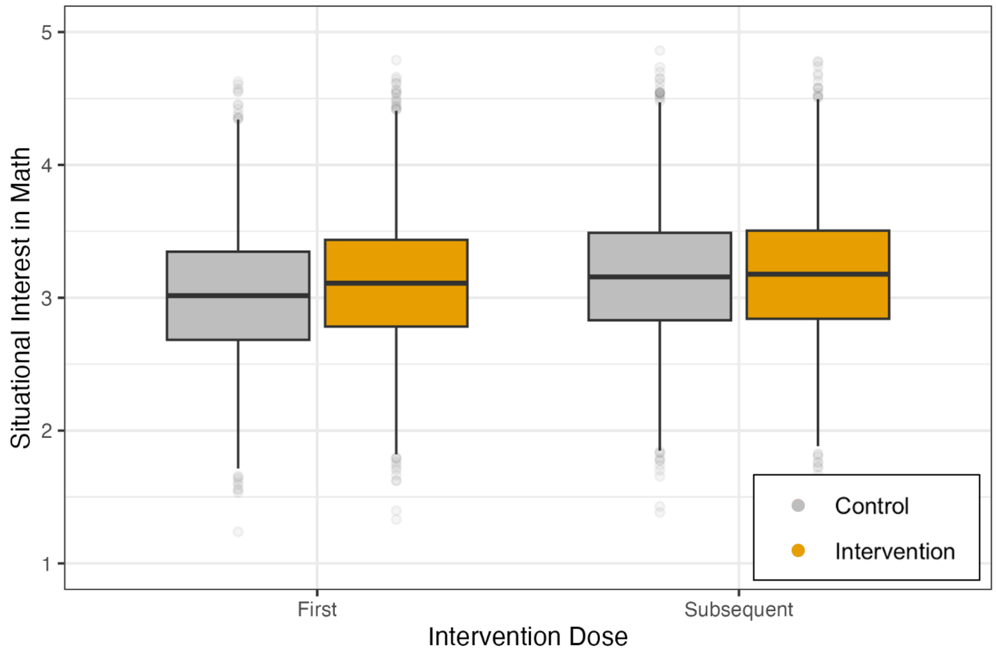
	Est	LLCI	ULCI	pr
DV: Hint Abuse				
Fixed Effects				
Intercept	0.18	0.166	0.186	> .999
Condition	0.00	-0.009	0.010	.523
Time	0.02	0.019	0.023	> .999
Condition x Time	0.00	-0.005	0.005	.526
Random Effects				
By-Student SD (Condition x Time)	0.07	0.068	0.078	
By-Class SD (Condition x Time)	0.01	0.000	0.016	

DV: Idle (Count) During Problem				
Fixed Effects				
Intercept	0.17	0.163	0.180	> .999
Condition	-0.03	-0.041	-0.019	> .999
Time	-0.01	-0.016	-0.011	> .999
Condition x Time	0.01	0.002	0.013	.994
Random Effects				
By-Student SD (Condition x Time)	0.04	0.027	0.045	
By-Class SD (Condition x Time)	0.01	0.002	0.016	
DV: Percent Correct				
Fixed Effects				
Intercept	0.47	0.466	0.479	> .999
Condition	0.00	-0.006	0.004	.622
Time	0.02	0.015	0.017	> .999
Condition x Time	0.00	-0.002	0.003	.658
Random Effects				
By-Student SD (Condition x Time)	0.05	0.048	0.053	
By-Class SD (Condition x Time)	0.00	0.000	0.010	

Note. LLCI = Lower level of the 95% credible interval; ULCI = Upper level of the 95% credible interval. Pr = the probability that the associated estimate is greater or less than 0.

Maintained Situational Interest in the Algebra Unit. Immediately following each intervention module, students in the utility-value condition reported higher levels of maintained situational interest in the unit than their counterparts in the control condition, $b = .04$, 95% CI [.026, .088], pr($b > 0$) = .997, on average. This effect was qualified by an interaction with dosage, $b = -.07$, 95% CI [-.139, -.009], pr($b < 0$) = .987, such that treatment effects were more than twice as large for students receiving their first dose compared to students receiving a subsequent dose, Figure 12. The residual standard deviation for triggered situational interest was estimated to be 1.24 units, and as such the effects reported above indicate that the intervention increased triggered situational interest, on average, by .03 SD, and that effects on the first dose were approximately .06 SD larger than subsequent effects. In summary, the interventions had small but positive effects on situational interest in each unit, with initial doses having substantially larger effects than subsequent ones (.07 SD, vs. .02 SD).

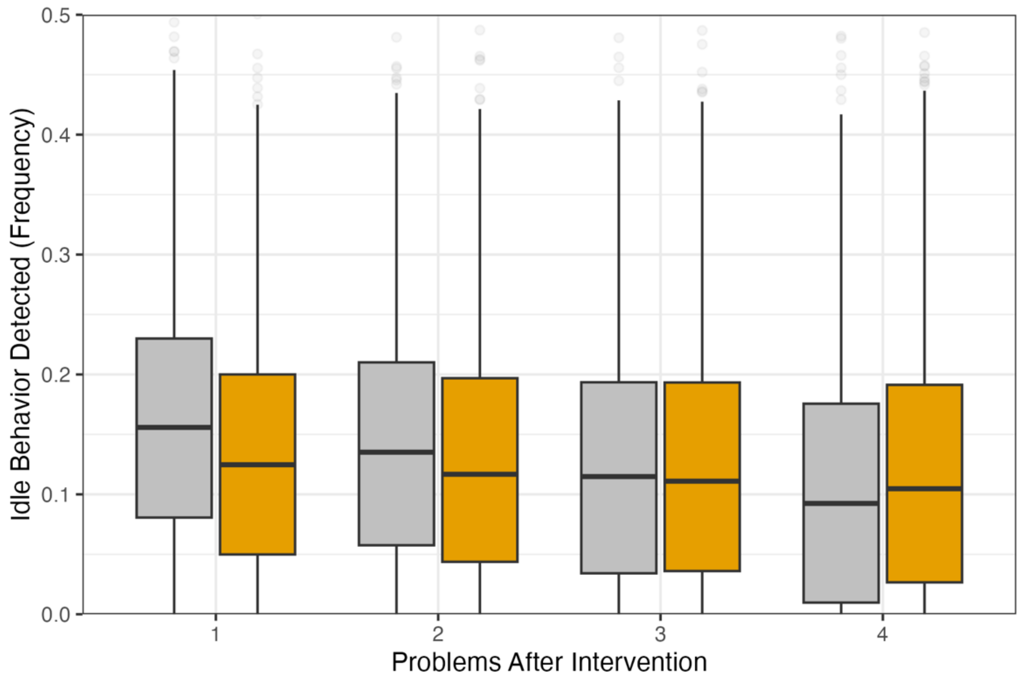
Figure 12. Condition x Dosage Interaction on Situational Interest in Mathematics



Note. Figure X was generated by sampling from the expectation of the posterior distribution from the interest model for each condition at each level of dosage. Boxes display the interquartile range of draws, whiskers display the interval from the 10th to 90th percentile of the posterior distribution, and points represent draws from the posterior distribution outside of that range.

Idle Behavior. On the first problem after an intervention module, participants in the utility-value condition went idle 3% less frequently than those in the control condition, $b = -.03$, 95% CI [-.046, -.014], pr($b < 0$) > .999. This effect was not moderated by dosage, $b = 0.01$, 95% CI [-0.020, 0.038], pr($b > 0$) = .633. In addition, intervention effects on idle behavior grew weaker over time as students attempted subsequent problems after completing the intervention, $b = .01$, 95% CI [.002, .013], pr($b > 0$) = .994, Figure 13. The residual standard deviation for idle behavior was estimated to be .51 units, and as such the intervention decreased the frequency of idle behavior .06 SD on average, with effects growing between .01 and .02 SD weaker on each subsequent problem following the intervention. In summary, the interventions decreased the probability of students going idle (i.e., spending more than 2 minutes without interacting with their computer) by three percentage points during the first problem post-intervention. This effect faded out as students completed subsequent problems, disappearing by the third problem after the intervention.

Figure 13. Effects of the Intervention on Idle Behavior Fade Out Over Time



Note. Figure 13 was generated by sampling from the expectation of the posterior distribution from the idle behavior model for each condition and for each problem after the intervention. Boxes display the interquartile range of draws, whiskers display the interval from the 10th to 90th percentile of the posterior distribution, and points represent draws from the posterior distribution outside of that range.

Hint Abuse. On the first problem after an intervention module there was no effect of the utility value intervention on hint abuse, b = .00, 95% CI [-.008, .016], pr(b > 0) = .708; participants in the utility-value condition were no less likely to solve these problems by abusing hints provided by the tutor than participants in the control condition. Although the *Condition* x dosage interaction was negative, b = -0.02, 95% CI [-0.041, 0.008], pr(b < 0) = .90, suggesting a possible crossover interaction (in which the initial intervention dose increased hint abuse and subsequent doses decreased hint abuse), the 95% credible interval for this interaction contained zero, suggesting it should be interpreted with caution. There was no interaction between

Condition and problem number, $b = 0.00$, 95% CI = [-0.005, 0.005], pr($b > 0$) = .526. The utility value intervention had no significant impact on hint abuse at any level of dosage or at any time after the intervention.

Performance on Math Tutor Problems. On the first problem after an intervention module there was no effect of the utility value intervention on performance, $b = .00$, 95% CI [-.011, .003], pr($b < 0$) = .883, nor was there an interaction between condition and dosage, $b = 0.00$, 95% CI [-.012, .018], pr($b > 0$) = .654, and no interaction between condition and problem number, $b = .00$, 95% CI [-.002, .003]. The utility value intervention had no significant impact on performance at any level of dosage or at any time after the intervention.

Longer-Term Effects

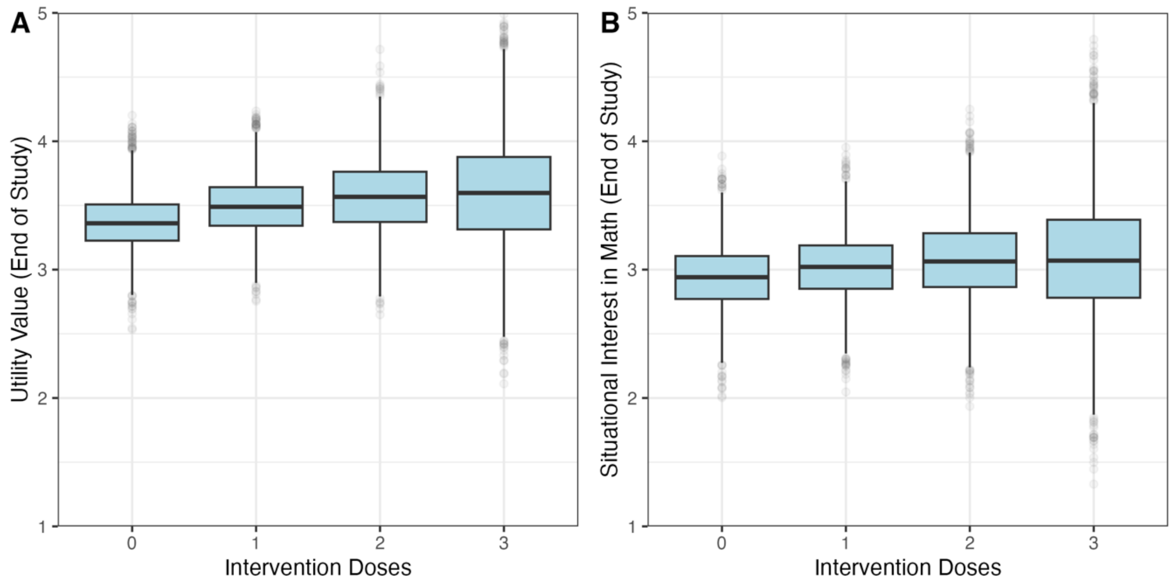
Table 13. Longer Term, Cumulative Intervention Effects

	Est	LLCI	ULCI	pr
DV: Utility Value for Mathematics				
Fixed Effects				
Intercept	3.27	3.176	3.378	> .999
Number of Interventions Completed	0.13	0.018	0.236	.990
Number of Interventions Squared	-0.02	-0.066	0.019	.872
Number of Modules Completed	0.05	0.003	0.105	.979
Random Effects				
By-Class SD (Num Ints)	0.09	0.007	0.175	
By-Class SD (Num Ints Squared)	0.03	0.001	0.061	
DV: Maintained Situational Interest in Math				
Fixed Effects				
Intercept	2.82	2.712	2.937	> .999
Number of Interventions Completed	0.08	-0.040	0.193	.906
Number of Interventions Squared	-0.02	-0.065	0.024	.817
Number of Modules Completed	0.12	0.064	0.185	> .999
Random Effects				
By-Class SD (Num Ints)	0.09	0.005	0.184	
By-Class SD (Num Ints Squared)	0.03	0.001	0.065	
DV: Posttest Performance				
Fixed Effects				
Intercept	0.54	0.518	0.554	> .999

Number of Interventions Completed	0.00	-0.014	0.018	.585
Number of Interventions Squared	0.00	-0.008	0.004	.748
Number of Modules Completed	0.02	0.007	0.024	.999
Random Effects				
By-Class SD (Num Ints)	0.01	0.000	0.026	
By-Class SD (Num Ints Squared)	0.00	0.000	0.008	

Post-Questionnaire Utility Value for Mathematics. There was a significant relationship between the number of interventions that students completed and students' self-reported beliefs about the utility value of mathematics at the end of the study, $b = .13$, 95% CI = [.018, .236], pr($b > 0$) = .990, suggesting that the first dose of the utility value intervention was associated with a .13 unit increase in participants' utility value beliefs, measured on the posttest. The quadratic term in the model was negative, with 87% posterior probability of it being less than zero, $b = -.02$, 95% CI = [-.066, .019], providing some evidence that intervention effects on end-of-study utility value may get smaller with each subsequent dose. The residual standard deviation for utility value beliefs was 1.01 units, and as such the coefficients reported above convert to a .13 SD effect of the initial intervention dose, with the incremental effectiveness of each subsequent dose growing .02 SD smaller, see Figure 14, Panel A.

Figure 14. Cumulative Effects of Interventions on End-of-Study Utility Value Beliefs (Panel A) and Maintained Situational Interest in Mathematics (Panel B)



Post-Questionnaire Maintained Situational Interest in Mathematics. The relationship between intervention dosage and maintained situational interest was positive, with 91% posterior probability of being greater than zero, b = .08, 95% CI [-.040, .193], pr(b > 0) = .906. As was the case for utility value beliefs, the quadratic term in the model for interest was negative, with 82% posterior probability of it being less than zero, b = -.02, 95% CI = [-.065, .024], pr(b < 0) = .817. The residual standard deviation for utility value beliefs was 1.15 units, and as such the coefficients reported above convert to a .07 SD effect of the initial intervention dose, with the incremental effectiveness of each subsequent dose growing .02 SD smaller, see Figure 14, Panel B.

Posttest Performance. On the posttest, there was a no relationship between the number of interventions that students completed and performance, b = .00, 95% CI = [-.014, .018], and the quadratic term did not differ from zero, b = .00, 95% CI = [-.008, .004], pr(b < 0) = .748.

Mediation of Effects on Idle Behavior

Because the intervention (1) promoted maintained situational interest in algebra units, and (2) made students less likely to go idle during the study, I considered whether the effects on idle behavior might be mediated those on interest. Accordingly, I followed Baron & Kenny's (1986) procedure for mediation analysis and tested whether triggered situational interest was negatively related to idle behavior, controlling for condition. Interest in mathematics was unrelated to idle behavior, $b = 0.00$, 95% CI [-.005, .009], pr($b > 0$) = .728, ruling it out as a mediator of the intervention effect on this outcome.

Discussion

This study showed that a utility value intervention in an online math tutor can have a small but statistically significant effect ($d = .03$ SD) on maintained situational interest in an algebra unit, measured immediately after the intervention. The intervention also promoted a measure of behavioral engagement with the online algebra environment, as students were less likely to be detected as "idle" immediately after finishing a utility value intervention. The effect on interest was moderated by dosage, such that the intervention was twice as powerful at triggering short-term situational interest in math when students were exposed to it the first time, compared to subsequent exposures. The effect on idle behavior was moderated by time, emerging on the first problem that students completed after the intervention but fading out over the course of subsequent problems. In the short term, there were no effects on hint abuse or algebra performance.

When students completed the final survey in the study (which was given, on average, over 4 days after students completed their final intervention), there was strong evidence for a relationship between students' self-reported utility value beliefs and the number of interventions that they had completed, establishing a relationship between intervention dosage and maintained motivation change. There was also evidence that the relationship between dosage and motivation change was non-linear; the first intervention was associated with a .13 SD increase in utility value beliefs, and the incremental effect of each subsequent dose grew .02 SD smaller, on average. However, the decreasing incremental value of an additional dose should not be used as evidence to argue against the importance of giving multiple doses, a practice that is common in utility value interventions. With three doses, intervention effects on utility value perceptions were twice as large as they were with a single dose.

There was weaker evidence for a similar, but smaller, relationship between intervention dosage and maintained situational interest in math. The first intervention was associated with a .07 SD increase in maintained situational interest in math, and the incremental effect of each subsequent dose grew .02 SD smaller, on average. There was no association between intervention dosage and performance on the posttest.

Limitations and Future Directions

Effects of the intervention on idle behavior were not mediated by effects on maintained situational interest. Although the intervention affected maintained situational interest, this measure was unrelated to idle behavior; students seemed to go idle for reasons that were unrelated to their interest in the algebra unit. So why did the intervention help students remain active when using the tutor? There are two major possibilities, both of which call attention to limitations of this study's design. First, it's possible that the intervention changed an aspect of other than maintained situational interest, the only motivational construct assessed after each intervention. The most conspicuous absence on the post-intervention survey was a measure of utility value beliefs, which I omitted due to concerns about demand characteristics. Given that the intervention changed beliefs about math's usefulness, measured several days later, it seems likely that it also had immediate, strong effects on this construct. However, given the results of Studies 1-7 (in which situational interest, but not utility value influenced engagement during a learning session), it seems unlikely that utility value perceptions could have mediated the intervention effects on idle behavior.

A second possibility is that the intervention affected idle behavior because of its format, rather than because of its message. To determine the added value of the intervention above and beyond "business as usual," I chose to compare it to a control condition in which students proceeded straight into a questionnaire without first watching a video, evaluating quotes, or doing any kind of reflective writing. As a consequence, the intervention might have served as a useful break from the algebra lesson—an experience not given to participants in the control condition. This could explain the positive intervention effects on idle behavior; participants may have remained on task more effectively following an intervention because they had just experienced a few extra minutes away from math practice.

In addition, this study had several limitations that involve implementation fidelity. Because few teachers assigned more than one or two units from the MATHia curriculum during the Spring of 2022 when the intervention was conducted, few students completed both baseline questionnaires and interventions, and many students completed only one intervention. Due to missing baseline questionnaires, it was not possible to analyze how student's baseline interest and confidence moderated intervention effects, and because so many students completed only one intervention module, nearly 50% of the sample had to be excluded in the primary analysis for models with random slopes to converge.

In the Fall of the 2022 school year, I began a second study in MATHia to address many of these limitations. I embedded the baseline survey at the beginning of the algebra curriculum to increase the proportion of students who receive it, and I included only three utility value interventions in the first two months of the curriculum to increase the proportion of students who would complete the entire intervention and posttest. I also included an additional item in the post-intervention surveys that assesses students' beliefs about the "importance" of what they're learning, which can serve as a subtly worded measure of utility value. Data collection is ongoing for this study and will finish at the end of the 2022-2023 school year.

It will also be important, using data from the present study as well as from the new study, to begin exploring the extent to which intervention effects differ between individuals and contexts, and attempt to understand the causes of any treatment effect heterogeneity that exists. For these analyses, it will be important to analyze students' written responses, and to examine whether observable classroom characteristics (e.g., average performance) moderate treatment effects.

Study 8 Conclusions

Overall, this intervention provides evidence that messages about the usefulness of algebra (in this case, in the form of quotes from other students) can have small effects on situational interest in mathematics and larger, more enduring effects on the belief that math is useful. In Study 8, effects on maintained situational interest were considerably smaller than those in laboratory studies 1-7, likely reflecting the difficulty of bringing about reliable change in real, noisy, and heterogeneous classroom environments with content that is already familiar to students.

Study 8 also highlights the benefits and potential limitations of repeated appeals to utility value. When exposed to multiple intervention modules, each of which was designed to incorporate different quotes and revolve around different themes, students reported stronger beliefs about the usefulness of mathematics when surveyed several days after the final intervention, compared to those who were only exposed to a single intervention. The same pattern of findings was also true for maintained situational interest. However, there were clear signs that the first exposure to the intervention's message was the most impactful, and that subsequent exposures had diminishing benefits.

General Discussion

In the present research, I examined how changes to students' beliefs about the utility value of academic content can influence their situational interest in a learning session, their engagement as they learn and practice, their performance, and their emerging individual interest in the content itself. In both lab and field experiments, manipulations that emphasized the usefulness of academic content successfully changed participants' utility value beliefs, and a consistent set of findings emerged. Utility value manipulations did little to promote triggered situational interest during learning sessions, a phase of interest characterized by attention and engagement. In addition, utility value interventions had no effects on performance.

Instead, these manipulations had consistent, positive effects on deeper phases of interest development. In both lab and field studies, utility value manipulations promoted maintained situational interest in academic content, which is characterized by positive feelings about the content (e.g., enjoyment, excitement). In lab studies in which participants learned about linear regression, utility value manipulations also made students more likely to request information about statistics-related resources on campus, a measure of emerging individual interest in the topic. These effects contrasted sharply with those of another potential trigger of interest that I explored in the lab: providing students with meaningful choices about how material was taught. Unlike the utility value manipulation, the choice manipulation successfully triggered participants' situational interest in the instructional video about regression, and it also increased self-reported engagement with the video. However, choice manipulations had no effect on the measure of emerging individual interest in statistics.

These findings suggest that distinct strategies may be required to trigger situational interest, promoting engagement and learning during a learning session, and to foster enduring, individual interest that motivates students to willingly revisit the content after a learning session concludes. Whereas instructional practices that increase the intrinsic appeal of a lesson can promote triggered situational interest and engagement, appeals to students' beliefs about utility value appear to play a critical role in promoting the development of individual interest. This evidence is consistent with theories of interest development put forth by Dewey (1913), Mitchell (1993), and Hidi & Renninger (2006), which suggest that beliefs about the meaningfulness of content are critical for the development of deep interest.

Limitations and Future Directions

The present laboratory and field studies have several important limitations that should be addressed with future research. First, although I placed a large focus on later phases of interest development in these studies, there was much more measurement of maintained situational interest (phase 2 of the four-phase model) than there was of emerging individual interest (phase 3). Emerging individual interest was only assessed in lab studies, and it was assessed with a single, dichotomous measure--whether participants requested resources about statistics opportunities on campus. Future field studies should incorporate similar, behavioral tasks to assess emerging individual interest in mathematics (e.g., if students explore resources about advanced math courses at their schools), and laboratory studies could incorporate continuous measures of interest in on-campus statistics courses.

Second, this laboratory research was also limited by a reliance on self-report measures for triggered situational interest. Triggered situational interest is largely characterized by attention and engagement, but the laboratory studies had no behavioral measures of engagement with the lesson. In future studies, it will be important to assess behaviors that indicate engagement (e.g., whether students pause and rewind the video during dense sections or take notes as they watch) or disengagement (e.g., whether students navigate to additional browser tabs). It could also be beneficial to incorporate a problem set into the regression paradigm, perhaps using online tutoring software so log-file data can be analyzed and measures of behavioral engagement can be developed.

The field experiment (Study 8) had several important limitations that should be addressed in future work. I will begin by discussing two comparatively minor issues, the control condition and the strength of the manipulation, before concluding with the largest issue: attrition and non-completion.

Regarding the control group, because this study was the first in this line of research, I wanted to examine how the effects of a utility value manipulation differed from standard instruction in the online algebra tutor. However, this meant that students in the intervention condition were able to take a break from math practice to watch a video and complete a brief writing activity, whereas students in the control group were not. This makes it difficult to infer whether the observed effects of the intervention involving on-task behavior are due to changes in beliefs about usefulness or differences in the format of the intervention and control conditions. In future field experiments, it will be important include an active control group in which students watch a video and complete a writing assignment, just as the those in the intervention condition do.

Regarding the strength of the manipulation, the field experiment had much weaker effects on participants' utility value beliefs, $d = .13$, than the lab study did, $d = .41$. This difference in effect size likely reflects (1) the difficulty of changing students' beliefs about the usefulness of math in real-world classrooms and (2) the difficulty of changing the perceived value of a broad domain (math) rather than a specific topic (linear regression). Future work should focus on ways to make this manipulation stronger. For example, the videos of narrated quotes could be replaced with videos in which actual students talk about the usefulness of math. Such videos could be recorded in targeted interviews or focus groups with former algebra students, and they could be much more memorable and compelling than the current approach for communicating the usefulness of algebra.

Finally, as for attrition and non-completion, of the 12,824 students who were enrolled in the field study, only 3,451 completed the baseline questionnaire, only 2,606 completed an outcome questionnaire, and less than 5% completed both questionnaires. In addition, although this study was designed to consist of six intervention modules, only 6,304 students (49% of the sample) completed more than one module.

These issues limited the potential of the study in several important ways. First, missing questionnaire data severely limited the sample size (and representativeness of the sample) for all analyses of longer-term change in maintained situational interest and utility value. Second, missing questionnaire data also made it impossible to test for moderation of intervention effects by baseline interest and confidence. Third, short-term analyses of intervention effects on situational interest, engagement, and performance were severely limited by non-completion of intervention modules. This issue made it difficult to examine the relationship between dosage and intervention effectiveness, and even more importantly this issue caused model convergence issues that forced me to drop over 51% of the sample for analyses of short-term outcomes. Finally, missing data severely limited the extent to which I was able to explore how and why intervention effects might have varied from classroom to classroom. The field study took place in 1,819 algebra classrooms, but because so few students completed baseline questionnaires it was impossible to examine how these classrooms differed in students' average levels of interest and confidence, or how these differences might have moderated the effectiveness of the intervention.

To solve issues related to missing data, it will be important to conduct studies in ways that maximize the proportion of students who complete questionnaires and multiple intervention modules. If baseline questionnaires could be completed when students first log in to the algebra tutor, this would eliminate all missing data on associated measures. To increase the proportion of students who complete the full intervention, future studies could also be designed to span fewer units and placed earlier in the curriculum, ideally in commonly-assigned units that large numbers of students complete. It would also be helpful to gather additional measures that can be collected without students completing a survey; for example, measures of baseline performance could be computed from students' pre-intervention performance in the tutor. Such a measure would be helpful for testing if student-level intervention effects vary as a function of baseline algebra skill, and it would also enable tests of how intervention effects vary between high-achieving classrooms vs. classrooms with large proportions of struggling students.

Conclusion

In school environments, it is critically important that educators think about interest development. Schools should be places that not only prepare students with the skills they will need for the future, but also places that help students develop interests in topics that can become careers and passions someday. Moreover, if educators can help students develop interest in course material, students will be more likely to engage deeply and enthusiastically over time. The present research suggests that beliefs about the utility value of course content can play an important role in the development of deeper interest, and it indicates that brief utility-value messages can help to change these beliefs, at least for a short period of time. Future work should continue exploring how to effectively harness these beliefs to promote meaningful change in students' experiences and outcomes.

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Appendices

Appendix A

Correlation Matrices and Descriptive Statistics for Studies 1-7

Study 1

	1	2	3	4	5	6	7	8
1. Baseline Confidence in Math								
2. Baseline Interest in Statistics	0.45*							
3. Distraction	-0.15	-0.19*						
4. Utility Value for Regression	0.17	0.37*	0.03					
5. Triggered Situational Interest	0.32*	0.38*	-0.50*	0.27*				
6. Maintained Situational Interest	0.51*	0.75*	-0.28*	0.50*	0.57*			
7. Requested Resources	0.21*	0.26*	-0.12	0.21*	0.27*	0.26*		
8. Performance (Test Score)	0.37*	0.24*	-0.02	0.09	0.12	0.27*	0.05	
Mean	4.78	3.89	4.41	4.91	3.25	3.89	0.22	15.57
SD	1.26	1.42	1.49	1.13	1.21	1.22	0.41	3.97

Study 2

	1	2	3	4	5	6	7	8
1. Baseline Confidence in Math								
2. Baseline Interest in Statistics	0.49*							
3. Distraction	-0.09	-0.21*						
4. Utility Value for Regression	0.26*	0.37*	-0.26*					
5. Triggered Situational Interest	0.08	0.31*	-0.51*	0.42*				
6. Maintained Situational Interest	0.31*	0.65*	-0.38*	0.63*	0.63*			
7. Requested Resources	0.11	0.27*	-0.13*	0.32*	0.30*	0.38*		
8. Performance (Test Score)	0.46*	0.30*	-0.03	0.16*	0.07	0.29*	0.10	
Mean	4.80	3.81	3.62	5.15	3.79	4.11	0.45	14.85
SD	1.14	1.31	1.55	1.14	1.35	1.33	0.50	3.74

Study 3

	1	2	3	4	5	6	7	8	9
1. Baseline Confidence in Math									
2. Baseline Interest in Statistics	0.32*								
3. Distraction	-0.04	0.04							
4. Utility Value for Regression	0.20*	0.49*	-0.24*						
5. Triggered Situational Interest	-0.02	0.22*	-0.71*	0.28*					
6. Maintained Situational Interest	0.12	0.63*	-0.31*	0.65*	0.54*				
7. Perceived Autonomy	-0.04	0.16	-0.37*	0.14	0.45*	0.35*			
8. Requested Resources	0.01	0.27*	-0.09	0.25*	0.27*	0.35*	0.18		
9. Performance (Test Score)	0.41*	0.13	-0.20	0.25*	0.19	0.21*	0.02	-0.01	
Mean	4.46	3.69	3.41	5.20	4.25	3.98	4.73	0.61	14.10
SD	1.15	1.23	1.58	1.05	1.30	1.04	1.20	0.49	4.53

Study 4

	1	2	3	4	5	6	7	8	9
1. Baseline Confidence in Math									
2. Baseline Interest in Statistics	0.50*								
3. Distraction	-0.13*	-0.18*							
4. Utility Value for Regression	0.28*	0.36*	-0.19*						
5. Triggered Situational Interest	0.14*	0.30*	-0.58*	0.33*					
6. Maintained Situational Interest	0.37*	0.70*	-0.36*	0.55*	0.61*				
7. Perceived Autonomy	0.05	0.08*	-0.38*	0.26*	0.51*	0.32*			
8. Requested Resources	0.07	0.20*	-0.13*	0.20*	0.20*	0.31*	0.11*		
9. Performance (Test Score)	0.44*	0.26*	-0.13*	0.19*	0.09*	0.21*	0.01	0.05	
Mean	4.51	3.57	4.26	4.97	3.58	3.63	4.41	0.36	14.18
SD	1.23	1.31	1.50	1.11	1.31	1.24	1.17	0.48	4.15

Study 5

measure	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**
1. Baseline Confidence in Math									
2. Baseline Interest in Statistics	0.39*								
3. Distraction	-0.11	-0.18*							
4. Utility Value for Regression	0.19*	0.36*	-0.30*						
5. Triggered Situational Interest	0.00	0.37*	-0.56*	0.43*					
6. Maintained Situational Interest	0.17*	0.53*	-0.44*	0.58*	0.75*				
7. Perceived Autonomy	0.06	0.21*	-0.33*	0.32*	0.48*	0.47*			
8. Requested Resources	-0.08	0.07	-0.24*	0.12	0.21*	0.26*	0.04		
9. Performance (Test Score)	0.45*	0.34*	-0.01	0.13	0.04	0.18*	0.09	-0.05	
Mean	4.44	3.59	4.07	5.04	3.83	3.60	4.54	0.26	14.51
SD	1.20	1.18	1.53	1.03	1.31	1.20	1.06	0.44	4.35

Study 6

	1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**
1. Baseline Confidence in Math									
2. Baseline Interest in Statistics	0.37*								
3. Distraction	-0.01	-0.24*							
4. Utility Value for Regression	0.18*	0.36*	-0.25*						
5. Triggered Situational Interest	0.07	0.35*	-0.60*	0.45*					
6. Maintained Situational Interest	0.19*	0.58*	-0.45*	0.61*	0.71*				
7. Perceived Autonomy	0.01	0.20*	-0.43*	0.36*	0.57*	0.46*			
8. Requested Resources	0.08	0.19*	-0.10	0.28*	0.22*	0.33*	0.12*		
9. Performance (Test Score)	0.41*	0.19*	-0.07	0.06	-0.01	0.08	-0.09	0.07	
Mean	4.49	3.56	4.32	5.21	3.70	3.63	4.23	0.33	13.99
SD	1.14	1.28	1.51	1.06	1.30	1.21	1.15	0.47	4.31

Study 7

	1	2	3	4	5	6	7	8	9
1. Baseline Confidence in Math									
2. Baseline Interest in Statistics	0.49*								
3. Distraction	-0.02	-0.10							
4. Utility Value for Regression	0.19*	0.35*	-0.20*						
5. Triggered Situational Interest	0.06	0.31*	-0.53*	0.49*					
6. Maintained Situational Interest	0.26*	0.57*	-0.40*	0.61*	0.78*				
7. Perceived Autonomy	-0.05	0.18*	-0.34*	0.40*	0.69*	0.60*			
8. Requested Resources	0.05	0.23*	-0.08	0.25*	0.22*	0.31*	0.14*		
9. Performance (Test Score)	0.46*	0.24*	-0.06	0.14*	-0.12*	0.08	-0.21*	0.03	
Mean	4.56	3.71	4.30	5.43	4.44	4.06	4.41	0.38	14.31
SD	1.24	1.23	1.64	0.88	1.21	1.23	1.19	0.49	4.39

Appendix B

Scales, Studies 1-7

Measure	Items
Baseline Confidence in Math	1. How good at math are you? 2. How comfortable do you feel doing math problems? 3. How strong is your background in math?
Baseline Interest in Statistics	1. How interesting do you find statistics? 2. How much do you enjoy learning about statistics? 3. How excited are you excited to learn about statistics?
Distraction	1. I got distracted as I watched the regression video. 2. My mind wandered as I watched the video. 3. I had trouble concentrating on the regression video.
Utility Value for Regression	1. How useful do you think it is to learn about linear regression? 2. How useful do you think linear regression could be in your future? 3. How useful is linear regression for helping others? 4. To what extent could linear regression be useful for finding solutions to problems people face in their everyday lives? 5. How useful do you think knowledge of linear regression could be for you? (S7 only) 6. How helpful would knowledge of regression be for achieving your goals? (S7 only)
Triggered Situational Interest in Learning Session	1. It was fun to watch the video. (S1-S6) 2. The regression video was boring. (S1-S6) 1. I've enjoyed this session. (S7 only) 2. This session has been interesting (S7 only) 3. I've enjoyed learning new things (S7 only) 4. This session has been fun (S7 only) 5. This session has been boring (S7 only) 6. It's been fun to learn about statistics this way (S7 only)
Maintained Situational Interest in Regression (feelings)	1. How excited are you about linear regression? 2. How interesting do you find linear regression? 3. How interesting do you find statistics (S1-S4) 3. How much have you enjoyed learning about linear regression? (S5-S7)
Perceived Autonomy	1. I felt I had some choice about how to learn regression today. 2. There wasn't much choice involved in today's session. 3. I learned regression the way I wanted to. 4. I felt like an active participant in my learning. 5. I've had some choice(s) about how to think about regression (S7 only) 6. I've been given a chance to think for myself in this study (S7 only)

Appendix C

Results from Studies 1-7

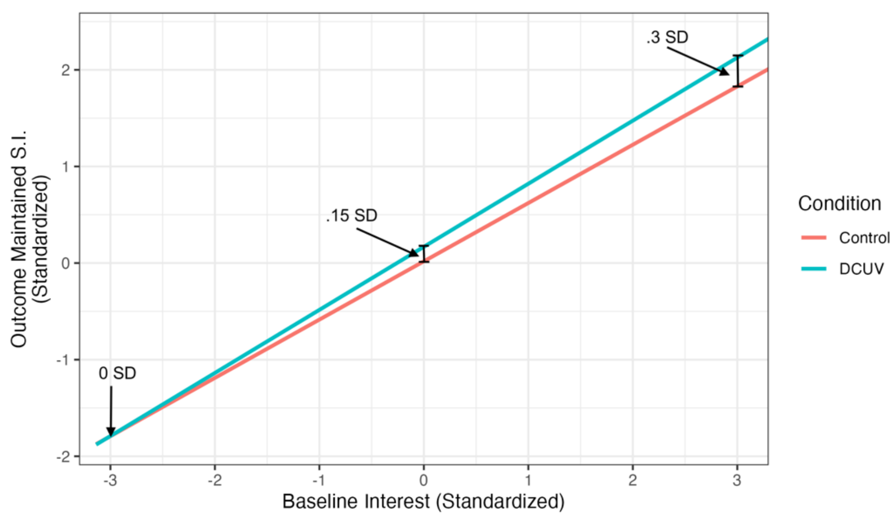
Regression output for all analyses of studies 1-7 can be found at https://osf.io/hnptv/

Appendix D

Post-Hoc Power Analysis for Condition x Interest Interaction

Figure D1, below, shows the relationship that I simulated between condition, baseline interest in statistics (standardized), and the outcome measure of maintained situational interest in regression (also standardized). In this figure, I assume no treatment effect of the directly-communicated utility value manipulation for participants at the lowest levels of baseline interest (-3 SD) and a treatment effect of *d* = .3 for participants with the highest levels of baseline interest (3 SD). As such, the average treatment effect in the simulated sample is *d* = .15, the same magnitude that I observed in the study.

Figure D1. Simulated Relationship Between Condition, Baseline Interest, and Maintained Situational Interest



Using the code presented in below, I simulated the results of 1,000 studies. Each study consisted of 2,019 participants (the combined N from Studies 1-7) with varying levels of baseline interest (normally distributed with a mean of 0 and standard deviation of 1). Maintained situational interest scores were generated to correlate .6 with the baseline interest scores (the approximate correlation between the two variables across studies). I then randomly assigned participants to condition. If a simulated participant was assigned to the directly-communicated utility value condition, their maintained situational interest score was increased by between 0 and .3 SD, depending on their initial interest level (as is depicted in Figure D1). For each study, I then fit a model regressing the outcome interest scores on condition (D-UV = .5, Control = -.5), baseline interest (standardized), and the interaction between these two variables. After simulating and analyzing all 1,000 studies, I counted the number of studies with a significant main effect of directly-communicated utility value and the number of studies with a significant D-UV x baseline interest interaction. The main effect of condition was .15 SD on average, and it was significant in 98.2% of simulated studies. However, the D-UV x interest interaction (b = .05) was only significant 28.7% of the time. Given the effect sizes I observed in this study for main effects, even with an N of more than 2,000 participants I was substantially underpowered to reliably detect interactions between directly-communicated utility value and baseline measures of motivation.

Code for Post-Hoc Power Analysis

```
library(tidyverse)

sims <- 1000 # number of simulations
alpha <- .05 # alpha (type 1 error rate)
n <- 2019 # sample size
tau <- .3 # hypothesized effect size for treatment effect (standardized)
r <- .6 # correlation between baseline moderator and outcome in control

results <- data.frame()

set.seed(1234)

for (i in 1:sims) {

  # control potential outcome
  y0 <- rnorm(n = n, mean = 0, sd = 1)

  # simulate baseline interest scores, correlated with y0 scores
  e1 <- rnorm(n, 0, 1)
  int_b <- y0 * r + (1 - r**2)**.5 * e1

  # treatment potential outcomes
  treatment_effect <- ((int_b + 3) / 6) * tau
```

```
  y1 <- y0 + treatment_effect

  cond <- rbinom(n = n, size = 1, prob = .5) # do a random assignment
  int_f <- y1 * cond + y0 * (1 - cond) # reveal outcomes

  # run a regression
  d <- data.frame(int_f, int_b, cond)
  d$int_b_z <- scale(d$int_b) %>% as.numeric() # z score baseline interest
  d$cond_c <- d$cond - .5 # center condition
  fit.sim <- lm(int_f ~ cond_c * int_b_z, data = d)

  # extract and store coefficients and p values
  p_cond <- summary(fit.sim)$coefficients["cond_c", 4] # effect of condition
  est_cond <- summary(fit.sim)$coefficients["cond_c", 1]
  p_x <- summary(fit.sim)$coefficients["cond_c:int_b_z", 4] # cond x interest
  est_x <- summary(fit.sim)$coefficients["cond_c:int_b_z", 1]

  results <- rbind(results, data.frame(p_cond, est_cond, p_x, est_x))
}

# summarise and report results of power analysis

results$sig_cond <- results$p_cond <= .05 # are condition efffects
significant?
results$sig_x <- results$p_x <= .05 # are interactions significant?

power_x <- mean(results$sig_x) # proportion of significant interactions
power_cond <- mean(results$sig_cond) # prop. of significant main effects
list(power_x, power_cond) # return results
```

Appendix E: Utility-Value Intervention Quotes for Study 8

Intervention 1. Theme: "How do you use math in your spare time?"

Student 1. I love cats, and I volunteer with the animal shelter in my town. Learning about exponential growth helped me understand why there are so many stray cats.
Cats can have five kittens at a time, every six months. This way, one cat can have five kittens, and each of those kittens can have five more kittens just six months later, and so on. It's exponential. When I volunteer, I help trap stray cats so we can neuter and then release them so they can't have more kittens.

Student 2. I'm trying to become #instafamous. I started out by trying to post things that would get lots of likes, but I realized it's even more important if people share it or tag friends. So now I try to post things that will be interesting to a big group. If a few more people share my post, their friends see it too. Going viral is basically just exponential growth.

Student 3. I like to play video games. My favorite one is a Battle Royale game, and I realized that one character does damage based on exponential growth. His main ability does just a bit of damage at first, but then it does a bit more damage each second, and then way more if you can't escape quickly. I didn't think I would use algebra stuff outside of class, but I guess I do!

Intervention 2. Theme: "How do you use math in your other classes?"

Student 1. Math has helped me in my chemistry class. We learned all about exponential decay for different chemicals, basically just like we did in algebra. I didn't realize that exponents would keep coming up in my other classes, but they do.

Student 2. Math has helped me in my chemistry class. We learned all about exponential decay for different chemicals, basically just like we did in algebra. I didn't realize that exponents would keep coming up in my other classes, but they do.

Student 3. I don't use math much in my other classes right now, but I really want to go to college. My cousin studied medicine and programming, and she told me that high school math helped her in a lot of her college courses. I don't know yet what I want to study in college, but it seems like my math classes might help me in my own college classes someday.

Intervention 3. Theme: "How do you think you might use what you're learning to help someone?"

Student 1. In my algebra class, we learned about exponential growth, and my teacher told us this is how COVID spreads. If one person gets two people sick, and they each get two people sick, and then they each get two people sick, it's exponential growth. But if people wear masks or stay home when they feel sick, then COVID can decrease with exponential decay. Now I know why it's so important to do things like wearing masks.

Student 2. In my math class, I just did a problem where I used an equation to figure out what grade I need on my test to pass the class. Now that I know how to do this, it can help me and my friends figure out what we need to do to get the grades we want in all of our classes, instead of just hoping for the best.

Student 3. I have two little sisters and sometimes they want help with their homework. I can usually help them because I already know what they're learning. Also, I just learned in class that if you start saving money early, you get compound interest, which is exponential growth of your money. I'm telling my sisters to start saving as soon as they can.

Intervention 4. Theme: "How do you use math in your spare time?"

Student 1. Every day on my way to school, my bus goes over a bridge. I've noticed that the bridge is held up by a bunch of arches. I just learned in math class that these shapes are one of the strongest ways to keep a bridge up, and that the people who made the bridge had to use quadratic equations to figure out how to build it. It was cool learning how what we're learning in class actually gets used in real life.

Student 2. I helped my aunt build a doll house for my little cousin last summer, and we were using a lot of math to figure out all sorts of things. Like how much wood we'd need for the whole thing, how long we'd need to cut the different boards, and what sorts of angles we'd need to use so everything lined up. When my aunt showed me what she was doing, I was actually able to understand it and help because of what I learned in math class.

Student 3. I play a bunch of different sports, and I know that every time I shoot, throw, or kick a ball, I'm basically solving a quadratic equation. I have to figure out the right angle and exactly how much of my strength I need to use so the ball comes down in the exact right place. It's not really like I'm solving an equation in class but it's like my brain is doing it without me even thinking about it. It's pretty cool.

Intervention 5. Theme: "How do you use math in your other classes?"

Student 1. I want to be a musician, so I'm taking piano right now as an elective. We're working on writing our own pieces and I've definitely needed some math to figure out how all of the different rhythms fit into measures, and how to write everything out on the page. My music teacher says he needed more and more math the further he got with music in school.

Student 2. I didn't know I'd need algebra for my other classes, but we've been using a lot of it in physics class. Like, right now we're building catapults, and we're using quadratic equations to figure out how far they'll launch rocks through the air, because the catapult makes the rock go up at first and then gravity pulls it back down. It's good I paid attention in algebra.

Student 3. I'm not really using math right now in other classes because I'm just in ninth grade, but I know I'll need it later. I think I might want to study computer programming or design video games. I know all of that means I'll be taking a bunch of classes in college where I'll be using what I'm learning now in math.

Intervention 6. Theme: "How do you think you might use what you're learning to help someone?"

Student 1. It's kind of a funny example, but I just helped my friends order a pizza using what I learned in math class. We were trying to figure out if we should get three mediums or two larges, and I used the area of a circle formula to figure out exactly how much pizza we'd get with each option, to get the most for our money. If you don't know how to do the math to figure out what's a good deal, all kinds of people can take advantage of you.

Student 2. In my algebra class, we learned about quadratic equations, and my teacher told us this is how disease spreads. Like, if each person gets two other people sick, it's exponential growth. This happens at the start of every flu season. But after people get infected, they get immunity. Then the disease runs out of people to infect and stops spreading. It's all like a quadratic equation. Now I know why it helps to get a flu shot—it can help turn the flu around faster without you getting sick.

Student 3. My mom tells me that she has to use math all the time to help with paying the bills—like figuring out a budget each month and paying taxes too. I bet I'll use what I'm learning in algebra right now too when I'm out of high school and need to make my own budget or maybe even to help my family someday.

Appendix F

Measures, Study 8

Questionnaire	Measure	Items
Baseline Questionnaire	Baseline Interest in Mathematics	1. Math is interesting to me. 2. I look forward to the next time I'll get to do math. 3. I like doing math. 4. I'm looking forward to taking more math classes in the future.
	Baseline Utility Value for Mathematics	1. I think what we are learning in this math course is important. 2. Math will be important for my future. 3. Math can be useful in my everyday life.
	Baseline Confidence in Mathematics	1. I am confident that I will do well in this math class. 2. I'm good at math. 3. I'm good at learning new things in math.
Situational Interest Survey	Maintained Situational Interest in Unit	1. The math I'm learning is interesting.
Outcome Questionnaire	Utility Value for Mathematics	1. I think what we are learning in this math course is important. 2. Math can be useful for helping others. 3. Math will be important for my future. 4. Math can be useful in my everyday life. 5. Math can be useful for finding solutions to important real-world problems.
	Maintained Situational Interest in Math	1. Math is interesting to me. 2. I look forward to the next time I'll get to do math. 3. I like doing math. 4. I'm looking forward to taking more math classes in the future.