AMERICAN PSYCHOLOGICAL ASSOCIATION

Psychological Bulletin

ISSN: 0033-2909

https://doi.org/10.1037/bul0000443

The Unpleasantness of Thinking: A Meta-Analytic Review of the Association Between Mental Effort and Negative Affect

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Influential theories in psychology, neuroscience, and economics assume that the exertion of mental effort should feel aversive. Yet, this assumption is usually untested, and it is challenged by casual observations and previous studies. Here, we meta-analyze (a) whether mental effort is generally experienced as aversive and (b) whether the association between mental effort and aversive feelings depends on population and task characteristics. We meta-analyzed a set of 170 studies (from 125 articles published in 2019–2020; 358 different tasks; 4,670 unique subjects). These studies were conducted in a variety of populations (e.g., health care employees, military employees, amateur athletes, college students; data were collected in 29 different countries) and used a variety of tasks (e.g., equipment testing tasks, virtual reality tasks, cognitive performance tasks). Despite this diversity, these studies had one crucial common feature: All used the NASA Task Load Index to examine participants' experiences of effort and negative affect. As expected, we found a strong positive association between mental effort and negative affect. Surprisingly, just one of our 15 moderators had a significant effect (effort felt somewhat less aversive in studies from Asia vs. Europe and North America). Overall, mental effort felt aversive in different types of tasks (e.g., tasks with and without feedback), in different types of populations (e.g., university-educated populations and non-university-educated populations), and on different continents. Supporting theories that conceptualize effort as a cost, we suggest that mental effort is inherently aversive.

Public Significance Statement

In practice, employers and educators often stimulate employees and students to exert mental effort. On the surface, this seems to work well: Employees and students are indeed often observed to opt for mentally effortful activities. One may be tempted to conclude from this observation that employees and students may readily learn to enjoy mental effort. Our results suggest that this conclusion would be false: Our meta-analysis shows that mental effort feels unpleasant across a wide range of populations and tasks. This insight is important for professionals (e.g., engineers, educators) who design tasks, tools, interfaces, materials, and instructions. When employees and students are required to exert substantial mental effort, it is sensible to support or reward them (e.g., by providing structure, by balancing demanding tasks with tasks that foster engagement, or by highlighting achievements).

Keywords: effort paradox, feeling of effort, subjective effort, cognitive effort, NASA Task Load Index

Supplemental materials: https://doi.org/10.1037/bul0000443.supp

Across scientific disciplines, people are often assumed to be effort avoiders. In psychology, this assumption is embodied in the classic "law of less work." Rooted in classic work on animal learning (Hull, 1943; Tsai, 1932), the "law of less work"—or the general assumption that people minimize their expenditure of effort—has had an immense influence on modern psychology. For example, it is now widely accepted that people often rely on heuristics and stereotypes, allowing them to expend less mental effort (Shah & Oppenheimer, 2008). Also,

Blair T. Johnson served as action editor.

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The authors thank Deepshikha Prasad and Fenna Andriessen for their help with data processing. Preregistration, coding protocol, data, code, and analysis scripts are available at https://osf.io/mktbr/.

Louise David played an equal role in conceptualization, data curation, formal analysis, investigation, methodology, project administration, writing–original draft, and writing–review and editing. Eliana Vassena played an equal role in conceptualization, investigation, methodology, supervision, and writing–review and editing. Erik Bijleveld played an equal role in conceptualization, data curation, formal analysis, investigation, methodology, supervision, visualization, writing–original draft, and writing–review and editing.

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it is now widely accepted that people exert mental effort strategically: They refrain from exerting mental effort unless their effort is compensated by a sufficiently valuable reward (Shenhav et al., 2017). More broadly, the "law of less work" is a cornerstone assumption in the biological and social sciences. For example, in neuroscience and economics (Holmstrom & Milgrom, 1994; Silvetti et al., 2023), effort is often modeled as a *cost*: a quantity that people try to minimize.

The underlying assumption shared across these domains is that effort, including mental effort, is inherently aversive. Yet, direct evidence for this assumption is scarce, as experimental studies often quantify the subjective value of mental effort by observing people's choices but not their experiences. It is unclear whether people's choices are a reliable proxy for the experienced unpleasantness of mental effort. Thus, a core question in this area remains unanswered: Does mental effort really feel aversive?

On the one hand, there are good reasons to think that mental effort should feel aversive. For example, negative affect is involved in initiating cognitive control (Botvinick, 2007; Dignath et al., 2020; Dreisbach & Fischer, 2012; Inzlicht et al., 2015), and cognitive control is assumed to be effortful (Morsella et al., 2009; Silvestrini et al., 2023). On the other hand, somewhat paradoxically, several lines of research suggest that mental effort can feel pleasant rather than aversive (Cacioppo et al., 1996; Eisenberger, 1992; for an overview, see Inzlicht et al., 2018). In this research, we systematically review and meta-analyze prior studies on the association between effort (as experienced during mental tasks) and negative affect. We estimate the strength of this mental effort–negative affect association, and we examine how it varies across tasks and populations. By doing so, we aim to draw a broad conclusion as to whether—and if so, when and for whom—mental effort feels aversive.

Why Mental Effort May Be Inherently Aversive

In an early essay titled "The Feeling of Effort," James (1880) proposed that mental effort arises when people are confronted with internal conflicts (e.g., between different representations or action plans) that they are attempting to resolve. According to this perspective, the experience of mental effort arises when people make decisions between alternatives that involve "mixed good and evil" (p. 22), such as when people decide to get out of bed on a cold morning. Relatedly, building on a set of pioneering experiments, Ach (1910/2006) proposed that acts of will (e.g., choosing to ignore a previously learned response rule) are accompanied by feelings of effort and tension (e.g., manifested as clenching the teeth, pressing together the lips). Thus, the phenomenology of effort has been a topic of interest in psychology for well over a century.

The idea that animals (including people) tend to minimize effort became mainstream in the 1930s and 1940s. For example, based on a large set of animal studies, Tsai (1932) wrote:

> The *law of minimum effort* [emphasis added] states that among several alternatives of behavior leading to equivalent satisfaction of some potent organic needs, the animal, within the limits of its discriminative ability, tends to finally select that which involves the least expenditure of energy. (p. 2)

This idea was later incorporated in Hull's (1943) attempt to formulate a general, mechanistic theory of behavior. In the book "Principles of Behavior," Hull (1943) formulated what he called the *law of less work*:

> If two or more behavior sequences, each involving a different amount of energy consumption or work (W), have been equally well reinforced an equal number of times, the organism will gradually learn to choose the less laborious behavior sequence leading to the attainment of the reinforcing state of affairs. (p. 294)

This line of research established the conservation of resources as one of the basic principles of psychology: All else being equal, when given the choice, animals will minimize the expenditure of effort.

In the decades that followed, several lines of research used the resource conservation principle to explain effort, both physical and mental, in humans. We highlight three influential research traditions. First, Kahneman (1973) conceptualized mental effort as a limited resource that must be allocated strategically during cognitive processes. In line with this conceptualization, experiments showed that mental effort—operationalized as pupil dilation—scales with task difficulty (suggesting that people expend the amount of effort that is necessary to perform well, but not more; Kahneman & Beatty, 1966) and responds to reward (suggesting that people invest effort, especially when their investment is likely to pay off; Bijleveld et al., 2009; Kahneman et al., 1968; Kahneman & Peavler, 1969). The idea that mental effort is a limited resource, in turn, is a close cousin of the well-established proposal that people often apply *heuristics*—simple processes that replace complex algorithms—when they make judgments and decisions (Bless & Fiedler, 2004; Newell & Simon, 1972; Shah & Oppenheimer, 2008; Tversky & Kahneman, 1974). By using heuristics, people make often-reasonable decisions while minimizing the expenditure of mental effort (for a synthesis, see Shah & Oppenheimer, 2008).

Second, Brehm and Self (1989) extended the discourse around mental effort by introducing *motivational intensity theory*, which was designed to predict, with high precision, when people should exert effort versus when they should refrain from doing so. In essence, motivational intensity theory predicts that effort should scale with task difficulty, but only (a) as long as success is possible and only (b) as long as the expenditure of effort is justified by the value of the outcome. This model is supported by dozens of studies (e.g., Bouzidi et al., 2022; Falk et al., 2022; Richter et al., 2008; Richter & Gendolla, 2009; for narrative reviews, see Gendolla et al., 2012; Richter et al., 2016). For example, in one study (Richter et al., 2008), participants were assigned to either of four levels of difficulty (low, moderate, high, impossible) of a cognitive task. Results indicated that effort—operationalized as cardiovascular reactivity—increased with task difficulty across the first three difficulty levels; yet, people refrained from exerting effort when the task was impossible. Studies like these have led to a nuanced account of effort allocation (Richter et al., 2016), which is in line with the resource conservation principle: People only exert effort when the rewards at stake are attainable and sufficiently valuable. When they do exert effort, they expend no more effort than is demanded by the task.

Third, over the past 15 years, there has been an upsurge in research on *effort-based decision making* (for a review, see Kool & Botvinick, 2018). In this research tradition, resonating with classic research (Hull, 1943; Tsai, 1932), researchers study how people decide between two or more choice options that are associated with different amounts of required effort. Studies in this tradition yielded several insights into the nature of effort-based decision making. For example, effort-based decisions are underpinned by the dopamine pathways (Treadway, Buckholtz, et al., 2012) in combination with medial prefrontal cortex (Chong et al., 2017; Silvetti et al., 2018); effort-based decisions are biased in patients with depression and schizophrenia (Barch et al., 2014; Treadway, Bossaller, et al., 2012); effort-based decisions respond to reward (Kool et al., 2010); effort-based decisions are modulated by fatigue and sleep (Dora et al., 2022; Massar et al., 2019; Müller et al., 2021); effort-based decisions depend on environmental factors (Bijleveld & Knufinke, 2018); effort based-decisions are influenced by the order in which information about reward and effort requirements is presented (Vassena et al., 2019); and, effort-based decisions about physical versus mental effort are computationally similar (Matthews et al., 2023). Though there are some important challenges in this domain (e.g., different effort-based decision-making tasks show low intercorrelations; Mækelæ et al., 2023), all these studies consistently support the resource conservation principle. That is, these studies show that, all else being equal, people prefer choice options associated with less effort.

In sum, the assumption that people tend to minimize effort, including mental effort, is deeply ingrained in psychology. Based on this rich history of ideas, one may be tempted to conclude that mental effort should also *feel* unpleasant. This conclusion would be consistent with mainstream models of cognitive control (which assume that negative affect plays a role in triggering cognitive control, e.g., Dignath et al., 2020; Dreisbach & Fischer, 2012; cognitive control, in turn, is assumed to be effortful; Morsella et al., 2009; Silvestrini et al., 2023). Moreover, recent studies showed that mental effort is associated with tension in the *corrugator supercilii*, a facial muscle that is known to be linked to negative affect (Devine et al., 2023), and that, under some conditions, people even choose to endure physical pain rather than to expend mental effort (Vogel et al., 2020). Despite this body of research, there is also an argument to be made that effort can, in fact, be pleasant rather than aversive. We now turn to a discussion of this competing perspective.

Why Mental Effort May Not Be Inherently Aversive

There are three arguments to suggest that effort may—at least for some people in some situations—*not* feel aversive. First, a well-established line of research shows that people vary in their *need for cognition*, that is, their "tendency to engage in and enjoy effortful cognitive endeavors" (Petty et al., 2009, p. 318). For example, in one classic study, participants read a narrative text that involved several arguments. Findings indicated that, in a surprise memory test, participants high in need for cognition remembered more of these arguments, suggesting that they expended more mental effort while reading (Cacioppo et al., 1983; for further support, see Lassiter et al., 1991; Priester & Petty, 1995; Srull et al., 1985). Overall, this body of literature suggests that at least some people should enjoy mental effort. This suggests that mental effort is *not* inherently aversive.

Second, theory on *learned industriousness* assumes that, when people have repeatedly been rewarded for expending effort, effort becomes a *secondary reinforcer*. According to this assumption, it should be possible to instill a generalized willingness to exert effort in people. Several studies on mental effort, both classic and modern, support this principle (Clay et al., 2022; Eisenberger et al., 1985; Lin et al., 2024). Based on these findings, one could hypothesize that people for whom effort expenditure has become a secondary reinforcer should experience effort as less aversive—or, perhaps, even as pleasant (Eisenberger, 1992). This line of reasoning, too, suggests that mental effort may *not* be inherently aversive.

Third, in many cultures, the expenditure of mental and physical effort has a positive rather than a negative connotation. For example, a recent line of cross-cultural studies showed that the expenditure of effort tends to be perceived (by others) as a signal of moral character (Celniker et al., 2023). Similarly, several religions emphasize that "working hard" is a moral virtue (e.g., Islam: Ali & Al-Owaihan, 2008; Protestantism: van Hoorn & Maseland, 2013), potentially causing billions of people around the globe to have positive associations with the exertion of effort. Thus, also from this perspective, one could argue that effort—including mental effort—is *not* inherently aversive.

To summarize, three strands of literature suggest that, at least for some people in some situations, the exertion of mental effort may be rewarding in and by itself. So, despite the long history of research on effort, it is still controversial whether mental effort is inherently aversive. This controversy is also fueled by casual observations—for example, if mental effort is aversive, why do millions of people play chess?

Prior Reviews and Meta-Analyses on the Aversiveness of Mental Effort

Before we lay out our empirical approach, we discuss some previous reviews and meta-analyses that have influenced the debate on the aversiveness on effort.

In a narrative review, Eisenberger (1992) synthesized ∼100 studies on learned industriousness. Most of these experiments used hungry animals as subjects. To give a typical example: In one experiment (Eisenberger et al., 1979), one group of rats was repeatedly rewarded for completing a high-effort sequence of behaviors (running back and forth in an alley five times; $n = 5$). Another group of rats was rewarded for completing a low-effort sequence (running back and forth in an alley once; $n = 5$). Findings indicated that the rats that were rewarded for high effort exerted more effort on a new, unrelated task (pressing a lever). This experiment, along with many others, suggests that effort can become a *secondary reinforcer*, which implies that the aversiveness of effort can be diminished through reward learning. It is important to note that Eisenberger (1992) focused exclusively on behavior and not on subjective experiences. Thus, although extensive and thorough, this review provides no direct evidence that rewards may change the experienced unpleasantness of effort.

In a systematic review of over 100 studies on the *need for cognition*, Cacioppo et al. (1996) made an argument for the validity of a self-report instrument designed to capture this construct, the *need for cognition scale*. This self-report instrument requires people to indicate their agreement with items such as "I prefer my life to be filled with puzzles I must solve." Much more directly than Eisenberger (1992) did, Cacioppo et al. (1996) claimed that, for some people, effort should feel *pleasant* and not just *less aversive*. Specifically, people who are high in need for cognition should enjoy the expenditure of mental effort. To support their claim, Cacioppo et al. (1996) reviewed several studies that examined the association between need for cognition and ratings of task enjoyment (as measured directly after a cognitive task). Some of these studies showed the expected positive correlation, whereas others showed null results. On balance, Cacioppo et al. (1996) provided sufficient ground to formulate a more nuanced hypothesis: Mental effort may be aversive, just not for everyone (e.g., not for people higher in need for cognition, such as people with a higher education).

By contrast to these two landmark reviews that suggest that effort is *not* inherently aversive, a large set of reviews have reinvigorated the classic proposal that people tend to minimize the expenditure of effort, implying that effort is costly and aversive after all. We mention several representative reviews here: In a narrative review, Richter et al. (2016) synthesized 30 years of research on motivational intensity theory; findings generally supported the idea that people avoid investing more effort than necessary. In further narrative reviews, Shenhav et al. (2017) and Silvestrini et al. (2023) evaluated a range of putative neural and computational mechanisms that may underpin the costs of mental effort. In a systematic review, Shah and Oppenheimer (2008) catalogued various ways in which people minimize the expenditure of mental effort through heuristics. In a systematic review and meta-analysis, Torka et al. (2021) synthesized how people choose to exert effort—or choose to refrain from exerting effort—when working in teams. Finally, in several systematic reviews and meta-analyses, occupational health researchers have examined the association between effort expenditure at work and health outcomes; findings indicated that *effort–reward imbalance* (i.e., the sustained combination of high effort and low reward) predicts mental and physical illnesses in the long run (e.g., Dragano et al., 2017; Rugulies et al., 2017). Together, though this set of prior reviews generally does not directly address people's subjective experiences while exerting effort, they are consistent with the assumption that effort is inherently costly and aversive.

Thus, the controversy in the literature around the aversiveness of mental effort also emerges from our survey of prior reviews. That is, some reviews suggest that the aversiveness of effort varies between people and situations, whereas others suggest effort is inherently aversive. This tension was previously described in a narrative review by Inzlicht et al. (2018), who coined the term *effort paradox*: On the one hand, people have a clear tendency to avoid effort; on the other hand, at least some people may enjoy effort, at least sometimes. In the present research, we go beyond this previous work by offering a quantitative synthesis. We meta-analyze a substantial body of studies to better understand if—and if so, under what conditions—mental effort is experienced as unpleasant.

The Present Meta-Analysis

Our research addresses the controversy in the literature by tackling two questions: First, is mental effort generally experienced as aversive? Second, what sample and task characteristics moderate the experienced aversiveness of mental effort?

We examine these questions by meta-analyzing a substantial set of recent studies in which a sample of healthy adults carried out some cognitive task and then reported how much mental effort and how much negative affect they experienced during that task. A challenge for any meta-analysis on the link between mental effort and negative affect is that (self-reported) mental effort and negative affect are often considered to be secondary or exploratory measures. So, although both constructs are routinely included in behavioral research, they are often reported only as an afterthought (e.g., only briefly in the main text, not in the abstract or keywords). This makes it hard to systematically search for and then identify studies that include measures of both mental effort and negative affect. In the present study, we solve this challenge by focusing our search-and-inclusion strategy on a self-report instrument called the NASA Task Load Index (NASA-TLX; Hart, 2006; Hart & Staveland, 1988), which captures both mental effort and negative affect.

We capitalize on the fact that the NASA-TLX has gained traction in different scientific disciplines (e.g., ergonomics, psychology, and computer science). As such, we will examine mental effort and negative affect in a wide range of populations (e.g., American physicians, Indian fighter pilots, Japanese students) and a wide range of tasks. As the body of studies that uses the NASA-TLX is enormous, we conduct a rapid review in which we include only recent studies. Our main hypothesis is that, across tasks and populations, the feeling of effort should be associated with negative affect. Nonetheless, we also expect this association to vary across populations and tasks.

First, as reviewed above, studies on learned industriousness show that people who are repeatedly rewarded for exerting effort develop a tendency to exert greater effort in the future (Clay et al., 2022; Eisenberger et al., 1985; Lin et al., 2024; for experiments on animals, see Eisenberger et al., 1979). Speculatively, this learning process may train people to *enjoy* effort due to its prior association with reward. Based on this work, we reasoned that the link between mental effort and negative affect should depend on people's *learning history*. To test this idea, we examined four moderators: *education level* (higher educated people may have been rewarded more frequently for mental effort), *work experience* (people who worked longer in a certain job had more opportunities to get rewarded for mental effort), *skill-task fit* (people who were trained to do a specific task may have been rewarded to expend effort, especially in that task), and *continent/country* (educational systems are different across the globe; some reward effort more explicitly than others). To illustrate the latter point: Country-level differences may emerge because there are cultural differences in the value placed on hard work (Ali & Al-Owaihan, 2008; van Hoorn & Maseland, 2013), but also because some governments have been inspired by research on *growth mindset* and have therefore decided to encourage teachers to recognize and value pupils' effort (for meta-analytic reviews, see Burnette et al., 2023; Macnamara & Burgoyne, 2023; for an example of an application, see Western Cape Education Department, 2023).

Second, some tasks are, by design, more pleasant than others. Most notably, occupational psychology has a long history of studying what task parameters promote motivation and job satisfaction. Specifically, job characteristics theory starts out from the assumption that tasks (or jobs) that provide people with meaning, responsibility, and knowledge of one's own performance should be most conducive to motivation and job satisfaction (Fried & Ferris, 1987; Hackman & Oldham, 1976; Oldham & Fried, 2016). Based on this theory, we examined six design features of tasks: whether a task requires a variety of different activities and skills (*skill variety*), whether it has a clear start and finish (*task identity*), whether performance affects other people or otherwise has meaningful consequences (*task significance*), whether a task involves some degree of autonomous decision making (*control*), whether a task provides feedback on the consequences of actions (*monitoring feedback*), and whether a task provides feedback on performance (*performance feedback*). We are aware that research has examined more job characteristics beyond the ones proposed by job characteristics theory (e.g., related to social aspects of work, Morgeson & Humphrey, 2006). We chose to use the original characteristics from job characteristics theory, as these characteristics stick closely to objective task parameters, making it possible for us to code them based on the method sections of research articles. Though we based our choice of moderators on job characteristics theory, it is interesting to note that these design features resonate with the recent trend of *gamification*—that is, the addition of gamelike elements to learning platforms, aiming to enhance student engagement (Dalmina et al., 2019; Zainuddin et al., 2020). That is, designers often add gamelike elements such as "quests" and "levels" (which increase *skill variety* and *task identity*), performance feedback as compared to peers (which increases *task significance*), the possibility for the user to customize task elements (which increase *control*), progress tracking (which is a form of *monitoring feedback*), and point scoring systems (which is a form of *performance feedback*). We test the prediction that in tasks that have these design features, effort is less likely to translate into aversive feelings.

Method

Transparency and Openness

We preregistered our hypotheses, procedure, coding scheme, and analysis plan at https://osf.io/mktbr/ (David et al., 2024). Data and analysis scripts are stored there as well. All deviations from our preregistration are reported in the following section (under the Deviations From Preregistration section).

Inclusion Criteria

1. Article Type: We only included articles that were peer-reviewed, that were written in English, and that reported original data. Conference proceedings were included; dissertations were excluded.
2. Sample Size: We only included samples with a sample size greater than 10 to make sure we would spend our finite coding time on relatively robust studies. If an article reported several independent samples (e.g., two separate studies or two conditions of a between-subjects design), we coded these separately.
3. Participants: We only included samples of healthy participants who were not under the influence of a pharmacological or severe psychological treatment (e.g., induction of stress, mental fatigue, or sleep deprivation).
4. Procedure: We only included studies in which the NASA-TLX was administered directly after a single, discrete task that was described in the article. Per this criterion, we excluded studies that used the NASA-TLX to probe how people experienced a full working day, their job in general, or any activity that took longer than 1 day. If the same group of participants carried out multiple tasks consecutively, we coded all these tasks separately (if each was followed by a NASA-TLX measure).
5. Task: We only included tasks that required at least some cognitive effort (i.e., not tasks that consisted only of physical exercise).
6. Measures: We only included tasks that reported means and standard deviations (or standard errors or 95% confidence intervals) of both the *effort* and *frustration* items of the NASA-TLX.

Systematic Search Strategy

We searched for articles using the NASA-TLX via the online database Scopus. We chose to use Scopus as this database allowed us to search the main text of articles (i.e., not just the abstract, title, and keywords, which often do not mention measurement instruments by name). We considered documents published between 2015 and 2020 found using the search term: ALL ("NASA-TLX" OR "NASA Task Load Index"). This search yielded 5,061 documents.

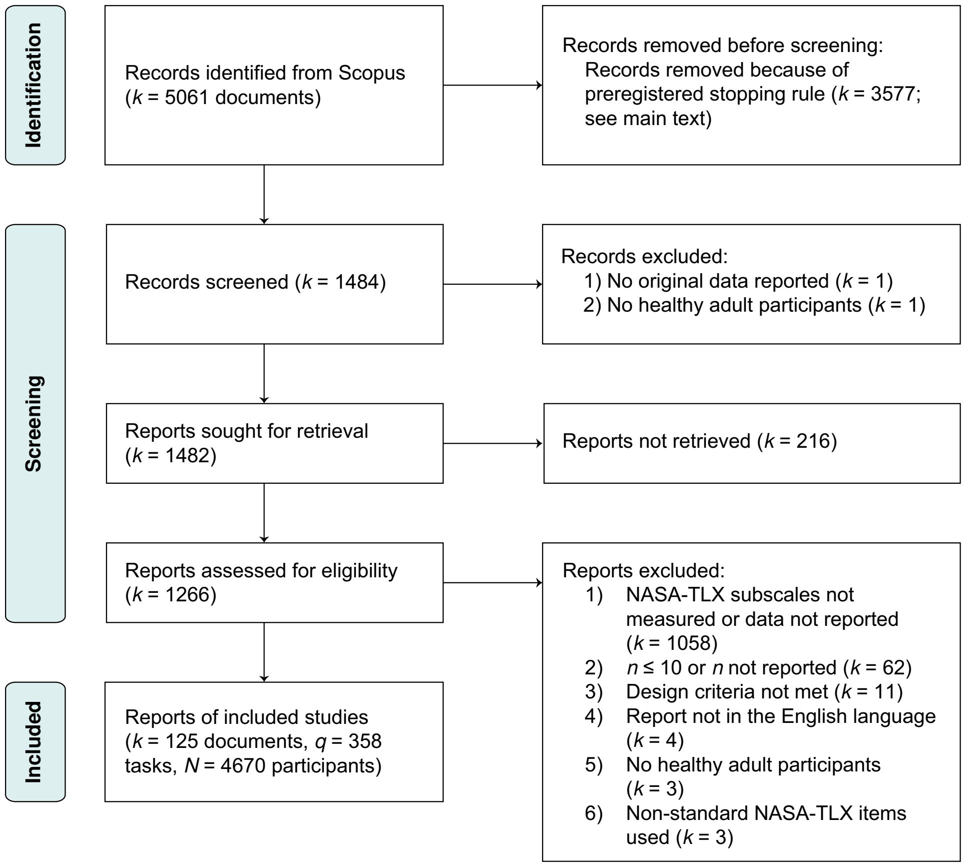
We applied our inclusion criteria in two steps (see Figure 1). First, two raters carried out an initial screening, checking (a) whether we had online access to the article through our university library and (b) whether Criterion 6 was met. Second, if the article passed this initial screening, one rater checked the remaining inclusion criteria. As our literature search yielded more articles than we could process given our resources, we preregistered an a priori stopping rule. We decided to start with the most recent article and then to code articles in reverse chronological order until either we processed all articles or it had become April 1, 2021, whichever came first. In line with this stopping rule, we processed the most recent 1,484 articles from our search. From these articles, 358 tasks (from 170 independent samples, from 125 articles; for a full list, see Supplemental Material; for a list of documents that we did not have digital access to at the time of searching, see https://osf.io/mktbr/) met our inclusion criteria. All these tasks were included in our analysis. In total, our meta-analysis was based on 9,144 NASA-TLX administrations from 4,670 unique individuals.

Calculating Effect Sizes

Our key outcome of interest was whether people experienced negative affect. We operationalized this outcome with the *frustration* item from the NASA-TLX, which is typically phrased as "How insecure, discouraged, irritated, stressed, and annoyed were you?" The main independent variable of this research is the feeling of effort, which we assessed with the *effort* item from the NASA-TLX, typically phrased as "How hard did you have to work to accomplish your level of performance?" Commonly, people respond to both items on a visual analog scale (with the anchors *very low* to *very high*) divided into 20 equal-width intervals. Based on their response, people typically receive a score between zero and 100 in increments of five.

We coded effort and frustration on the task level (i.e., each data point reflected a group of people that carried out the same task). To do so, we extracted raw means and standard deviations of the effort and frustration scales for all 358 tasks. If standard errors or confidence intervals were reported (instead of standard

Figure 1
Flow of Study Reports Into the Meta-Analytic Review



Note. NASA-TLX = NASA Task Load Index. See the online article for the color version of this figure.

deviations), we converted these into standard deviations. If means and standard deviations were not on a 1–100 scale, we converted them to that scale through linear transformation (Cohen et al., 1999). In most articles, these statistics were reported either in the main text or in tables. Yet, in some articles, these statistics were reported in plots. When we encountered a plot, we used *webplotdigitizer* (Rohatgi, 2021) to extract the relevant statistics. If it was not clear which dispersion measure was reported or which scale endpoints were used, we contacted the authors to provide clarification. If the authors did not respond, we excluded the article from our sample (k = 3). Based on the means and standard deviations for frustration, we calculated sampling variances with the *escalc* function of the *metafor* package (Viechtbauer, 2010) in R (R Core Team, 2021; Wickham et al., 2019).

To enhance the reuse potential of our dataset, we also coded the four remaining items of the NASA-TLX (*mental demand*, *physical demand*, *temporal demand*, and *performance*). As these were not our primary focus, we coded these only when they were presented in the main text or in tables (i.e., we did not digitize plots). Data from these items (q = 187 tasks; henceforth, we use the symbol q to denote sets of tasks) are available at https://osf.io/mktbr/.

Coding

Procedure

Table 1 presents an overview of all moderators. The first author coded all moderators for all tasks. To estimate the reproducibility of her coding, we created a detailed coding protocol. Using this protocol, an independent coder coded all moderators and control variables for 10 articles. To assess interrater reliability, we calculated Cohen's k for categorical variables and Pearson correlations for continuous variables. Initially, Cohen's k ranged from .36 (fair) to 1 (perfect; Landis & Koch, 1977); Pearson correlations were $\geq$.99. After discussing these results among the team, we increased the level of detail of the protocol (e.g., we added some more explanation and examples). The second coder then coded 10 new articles using the improved protocol. Cohen's k now varied between .69 (substantial) and 1 (perfect), with an average of .90 (almost perfect); Pearson correlations were $\geq$.99. Our final protocol is available at https://osf.io/mktbr/.

Learning History Moderators

Level of Education. We coded the highest level of education that participants in the sample typically received. Most samples

Table 1
Overview of Moderator Variables

Moderator	Level of coding	Description
Learning history moderators		
1. Education	Sample	Nonuniversity secondary education (32); university education[a] (199)
2. Work experience	Sample	*Continuous*, in years [M = 10.3; SD = 9.3] (58)
3. Skill–task fit	Task	Low fit (248); high fit[a] (109)
4a. Continent	Sample	Europe (158); North America (116); Asia (72)
4b. Country	Sample	United States (83); Germany (40); Canada (33)
Task design moderators		
5. Skill variety	Task	Repetitive (218); varied[a] (140)
6. Monitoring feedback	Task	Yes[a] (227); no or unknown (131)
7. Performance feedback	Task	Throughout[a] (47); No (309)
8. Control	Task	High control[a] (117); Low control (241)
9. Task significance	Task	High/medium significance[a] (170); Low significance (188)
10. Task identity	Task	High identity[a] (139); Low identity (219)
Exploratory moderators		
11. Age	Sample	*Continuous*, in years [M = 28.7; SD = 9.9] (277)
12. Gender	Sample	*Continuous*, proportion females [M = .36; SD = .22] (267)
13. Duration of task	Task	*Continuous,* in minutes [M = 35.2; SD = 54.7] (166)
14. Physical activity	Task	Light activity (112); no activity (228)
15. Group setting	Task	Individual (184); observers present (131); together with others (43)

Note. For categorical moderators, the Description column reports all moderator categories that were included in our analysis. Categories with <30 tasks were excluded from analysis; these categories do not appear in this table, but they are described in the main text. The number between round brackets is the number of tasks (denoted q in the main text) in the category. For continuous moderators (labeled *Continuous*), the Description column reports the unit of analysis, descriptive statistics (between square brackets), and the number of tasks for which we coded this moderator (between round brackets).
[a] Indicates moderator categories in which we expect a weaker (or negative) association between mental effort and negative affect.

consisted of university-educated participants (q = 199 tasks), followed by participants with some nonuniversity secondary education (q = 32), and participants who completed high school (q = 17). In some cases, studies provided no explicit information on participants' educational background. In these cases, we could sometimes infer the typical educational level from participants' occupation. If this was not feasible, we excluded the sample from analysis (q = 110). College samples were coded as university-educated. We excluded the high school category from analysis due to the small number of samples.

Work Experience. For most samples, we found no information on the mean work experience that participants had (q = 300). For the remaining samples (q = 58), we coded work experience in years.

Skill–Task Fit. We coded tasks as low fit when participants' skills—acquired either through formal education or experience—were unrelated to the task (q = 248). For example, in one study, participants (who hardly ever traveled by train) had to find their way in a virtual-reality version of the Saint-Michel Notre Dame train station in Paris (Armougum et al., 2020). This task was unrelated to participants' acquired skills, so we coded this task as low fit. Conversely, tasks were coded as high fit when participants could rely on previously acquired skills during the task (q = 109). For example, in one study that we coded as high fit, well-trained fighter pilots completed a flight simulation session (Mohanavelu et al., 2020). We could not code skill–task fit for one task (q = 1).

Continent and Country. We coded the country in which the data were collected. If no information was given, we assumed that data collection took place in the country in which most of the article's authors were based. Data came from the United States (q = 83), Germany (q = 40), Canada (q = 33), China (q = 22), United Kingdom (q = 20), Italy (q = 16), Japan (q = 13), Norway (q = 12), Netherlands (q = 11), Australia (q = 10), Denmark (q = 10), Poland (q = 9), Finland (q = 8), France (q = 8), Iran (q = 7), India (q = 7), Spain (q = 7), South Korea (q = 7), Sweden (q = 5), Indonesia (q = 5), Saudi Arabia (q = 4), Austria (q = 4), Belgium (q = 4), Malaysia (q = 3), Brazil (q = 2), Hong Kong (q = 2), Taiwan (q = 2), and Portugal (q = 1). One research team collected data in both Switzerland and Germany (q = 3).

We analyzed this moderator in two ways. First, we analyzed countries on the continent level, leading us to include Europe (q = 158), North America (q = 116), and Asia (q = 72). Second, to provide a more fine-grained analysis, we also analyzed this moderator on the country level, including the three countries with the most data points (i.e., the United States, Germany, and Canada; q ≥ 33 each).

Task Design Moderators

Skill Variety. We coded tasks as low skill variety if one component of a task was repeated several times or if a task involved short, standardized routines that were repeated continuously (q = 218). For example, in one task, participants performed ±2,900 trials of a computerized stimulus categorization task (Szychowska & Wiens, 2020). This task was coded as low variety, as all trials were very similar. We coded a task as high skill variety if a task consisted of several qualitatively different components or routines (q = 140). For example, in one study, novice surgical residents had to conduct a live robot-assisted laparoscopic surgery (Gerull et al., 2020). As this procedure required various steps (e.g., making incisions in the abdomen, inserting a medical instrument with a camera attached, controlling the camera, suturing, and communicating with staff), we coded this task as high variety.

Monitoring Feedback. We coded tasks as monitoring feedback present when participants received some sort of nonvalenced feedback from an external source on how they were doing on a task ($q = 227$). This feedback had to be related to the task goal. For example, in a driving simulation task (Zeller et al., 2020), participants drove a virtual car for 2 hr while receiving direct visual feedback on their actions (e.g., they could see their current speed on their dashboard). Similarly, in the study on surgical residents described above (Gerull et al., 2020), participants could see what they were doing on a monitor while they were handling the camera. We coded tasks as monitoring feedback absent when participants received no such external feedback ($q = 131$).

Performance Feedback. We coded tasks as performance feedback present when any sort of explicit performance feedback was given during the task ($q = 47$). Performance feedback could take different forms. In some tasks, some display element turned green or red, indicating good or poor performance, respectively (Blundell et al., 2020a). In other tasks, a virtual agent gave verbal performance feedback (e.g., "Wow, you are really good at this"; Wang, Buchweitz, et al., 2020). In yet other tasks, performance feedback was represented as the amount of money participants had accumulated (Dickinson et al., 2020). In most cases, performance feedback was absent ($q = 309$). In rare cases, participants received performance feedback, but only at the end of the task ($q = 2$). We excluded the latter level from analysis.

Control. We coded tasks as high control when participants could independently decide how to plan or carry out the task and whether there was room to take independent decisions. In other words, tasks were coded as high control if the task required (or at least allowed) participants to use their own judgment ($q = 117$). For example, in one task, participants had to drive back and forth to a certain location in a driving simulation (Milleville-Pennel & Marquez, 2020). Participants could choose between various routes (e.g., a longer route in a rural area vs. a shorter route in the city during rush hour). We coded tasks as *low control* when participants were not able to take independent decisions during the task, for example, if the task was fully scripted or if participants could only take minor decisions during the task ($q = 241$). For example, in one task, beginner golfers practiced their golf swings at a driving range 10 times (Woźniak et al., 2020). As they merely followed scripted instructions, we coded this task as low control.

Task Significance. We coded tasks as high significance when the participants' task behavior either affected other people or affected some real-world outcome ($q = 22$). All real-life performance situations fell into this category. For example, in one study, surgeons operated on real patients (Mendes, Costa, et al., 2020). We coded all simulations or training situations as medium significance ($q = 148$). For example, in one study, fighter pilots completed a simulated mission (Mohanavelu et al., 2020). We coded all other tasks as low significance ($q = 188$).

Task Identity. We coded tasks as high task identity if the task had a clear start and endpoint, that is, when participants were required to complete an entire piece of work from beginning to end ($q = 139$). For example, in one task (Ciumedean et al., 2020), participants played a game in virtual reality in which the goal was to escape from a prison. The task ended when they succeeded. We coded tasks as low task identity when tasks did not have clear-cut start and endpoints or when participants only performed part of a larger task or product ($q = 219$). For example, in one study, participants were asked to listen to and then categorize numerous short audio samples (Ishibashi et al., 2020). This task had no clear-cut start and endpoints and was therefore coded as low task identity.

Exploratory Moderators

Age. We coded the mean age of participants in the sample when this was reported ($q = 277$).

Gender. We coded the number of females and males in the sample when this was reported ($q = 267$). In our analysis, we used the proportion of females as a moderator.

Duration. We coded the duration of the task that participants had to conduct before they filled in the NASA-TLX. We only coded this moderator if the duration was explicitly stated in the article ($q = 166$).

Physical Activity. We coded the amount of physical activity that was necessary during the task in order to account for possible spill-over effects between physical and mental effort (Preston & Wegner, 2009). We categorized physical activity using a taxonomy that is commonly used in research on physical exercise (Piercy et al., 2018). We coded tasks as sedentary when people carried out the task in a sitting posture, for example, behind a desk ($q = 228$). We coded tasks as light physical activity when people carried out the task while standing and/or walking ($q = 112$). We coded tasks as moderate-to-vigorous physical activity when tasks required more intense physical exertion ($q = 18$).

Group Setting. We coded the group setting in which the task was conducted. We did this to account for effects of other people being present and for effects of working in teams (vs. individually). We coded tasks as individual–alone when participants were alone in a room (e.g., a cubicle), working on the task by themselves ($q = 184$). We coded tasks as individual–observers present when other people were physically present, but only in an observing role ($q = 131$). We coded tasks as *with others* when tasks were done together with other people ($q = 43$).

Reporting Transparency

As an index of the reporting transparency of the original studies, we coded whether the articles included data availability statements and, if they did, if the participant-level data were publicly accessible. Most of the articles ($k = 109$ articles, $q = 308$ tasks) did not include a data availability statement. Sixteen articles did include a data availability statement. Of these 16 articles ($q = 50$), eight articles ($q = 28$) mentioned that the data were available upon request. The remaining eight articles ($q = 22$) included a link to a public repository, from which the original data could be downloaded.

Analytic Strategy

We conducted the meta-analysis with the *metafor* package (Viechtbauer, 2010) in R (R Core Team, 2021; Wickham et al., 2019). We computed the best linear unbiased predictor for effort. Then, to examine Research Question 1, we adopted the best linear unbiased predictor for *effort* as the main predictor in a multilevel mixed-effects metaregression model using the raw mean (and the corresponding sampling variance) of *frustration* as our outcome measure. To examine Research Question 2, we added the moderators to this model. We only included moderator categories that consisted of ≥30 tasks. Each moderator was tested and interpreted individually. As such, each model included an intercept, the main effect of effort, the main effect of the moderator, and the Moderator × Effort interaction. To facilitate interpretation, we then estimated (and plotted) the effect of effort on frustration separately for each category of each categorical moderator. For continuous moderators, we estimated (and plotted) the effect of effort separately for several representative values of that moderator. We selected these values based on visual inspection of that moderator's distribution.

Articles often reported multiple samples; samples often carried out multiple tasks. Thus, our data had a nested structure, which we took into account by using a multilevel mixed-effects meta-analysis. First, to account for dependency among negative affect scores within each article, we included the article number in our random effects structure. This level assumes that scores within one article can be more similar than scores from other articles. Second, to account for a dependency among scores within studies, we included the study ID. This level assumes that scores within one study are more similar than scores across studies. Third, we added a unique identifier of each task within each independent sample to our random-effects structure. Thus, our random-effects structure was specified as "∼ 1 | ArticleID/SampleID/MeasureID." As an additional measure to account for the unknown structure of dependency within our data, we report cluster robust tests and confidence intervals (Pustejovsky & Tipton, 2018) applying the *clubSandwich* package (Pustejovsky, 2022).

A potential challenge for our meta-analysis stems from the fact that associations between questionnaire items may be inflated by response biases (e.g., people who have a stronger tendency to agree with items may score higher on both effort *and* negative affect, inflating the correlation between the two items; Baumgartner & Steenkamp, 2001; Bless & Fiedler, 2004). Such response biases may inflate correlations *within* individual samples. It is not a priori clear, however, if and how such response biases could have affected results from our meta-analysis. After all, we analyzed our data on an aggregate level (i.e., we analyzed sample means, not individual responses), and it is not a given that individual-level associations are mirrored by group-level associations (Kievit et al., 2013). Thus, we conducted a set of simulations to examine whether correlations within samples (which may be affected by response biases) could have plausibly biased our meta-analytic results. We described these simulations in the Supplemental Material.

Though analyses of publication bias are common in articles that report meta-analyses, we decided against presenting publication bias analyses for two reasons. First, the effect size of interest in our meta-analysis (i.e., the magnitude of the effect of effort on negative affect) was computed across studies. This approach is somewhat uncommon in that most meta-analyses focus on effect sizes that are computed within studies (e.g., standardized mean differences). As a result, common techniques for studying publication bias (e.g., funnel plots; Vevea and Hedges' weight function model) do not work in our case, as our effect size of interest necessarily includes effort as a between-studies predictor. Second, the raw NASA-TLX mean scores that we meta-analyzed were typically a byproduct of the original studies. That is, it seems unlikely that a study would be selected (or rejected) for publication based on the raw mean of any of the NASA-TLX dimensions. Thus, whereas publication bias may well exist in the body of literature we analyzed, it is unlikely that such bias affected our results (for a similar line of reasoning, see Buecker et al., 2021).

Deviations From Preregistration

In addition to examining all moderators separately, we planned to test all learning history moderators together in one model and all task design moderators in another to take into account correlations between moderators. However, during coding, it turned out that we could not code all moderators for all studies in a meaningful way, leading to missing data spread out over moderators. This especially affected the learning history moderators. If we had followed our plan (while excluding cases with missing data list-wise), we would have needed to exclude 93% of tasks in the learning history model. We felt this analysis would not be worthwhile, so we refrained from carrying it out. We did follow our plan for the task design moderators (we excluded <1% of tasks).

We planned and attempted to code two additional moderators. First, in cases where participants received performance feedback, we attempted to code the valence of this feedback (positive feedback only, negative feedback only, or both). It turned out that, in most tasks, participants received no performance feedback ($q = 309$). Only in two cases, participants received only positive feedback; in 11 cases, only negative feedback; and in 36 cases, both kinds of feedback. Therefore, we could not analyze positive versus negative feedback in a meaningful way. Second, we attempted to code whether tasks included performance-contingent incentives. It turned out that performance-contingent incentives were used only rarely ($q = 11$). Thus, we decided to drop these two moderators.

Moreover, we did not preregister the moderators that we labeled "exploratory moderators" nor the analysis in the section labeled "exploratory."

Results

Description of Included Studies

We included studies on a wide range of topics that used a variety of approaches, samples, and tasks. As the NASA-TLX is a popular tool in ergonomics, many of the included studies aimed to test how people experienced some kind of equipment or software (e.g., tools used in surgical procedures, various types of consumer electronics, flight simulator software). Several other studies were done within the standard experimental psychology paradigm. In these studies, participants carried out a computer task under controlled laboratory conditions. Studies were conducted in 27 different countries, mainly in Europe, North America, and Asia.

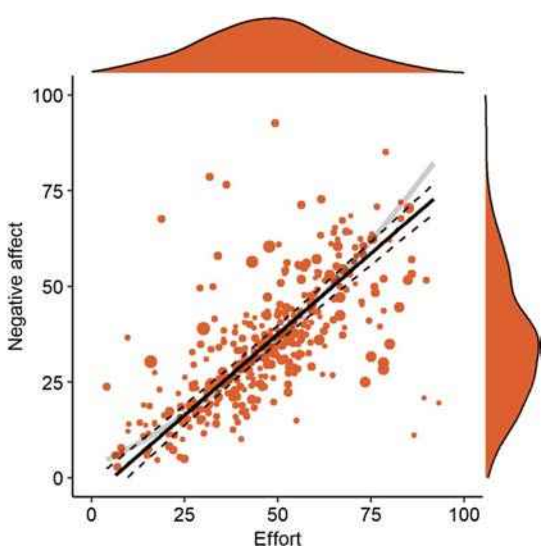
Table 1 presents descriptive statistics for all moderator variables that we coded. We also explored the associations between all moderators (reported in detail in the Supplemental Tables S1–S3). The most notable finding from this exploration was that four of the task design moderators—skill variety, control, task significance, and task identity—were correlated. That is, when a task was coded as having high skill variety, that task was more likely to be coded as high control (Cramer's $V = .66$), high task significance (Cramer's $V = .66$), and high task identity (Cramer's $V = .68$). In our moderator analysis, we dealt with these associations by testing the task design moderators individually but also all together in one model, which allowed us to explore whether the associations between these four moderators affected our main conclusions.

The Aversiveness of Effort

We first meta-analyzed all raw negative affect means (without including any predictors). On average, participants rated negative affect below the midpoint of the scale ($M = 34.6$, $SE = 1.3$, 95% CI [32.0, 37.1]). The prediction interval was [3.5, 65.6], suggesting that we can expect the true mean of negative affect in future, similar studies to lie anywhere in the bottom two thirds of the NASA-TLX scale. The Q-test was significant, $Q(357) = 53268.8$, $p < .001$, suggesting that the variability in observed negative affect was larger than would be expected based on sampling variability alone. I^2 was 98.3%, suggesting that almost all observed variance could be attributed to variance in true means rather than to sampling variance. Specifically, 52.2% of the variance was between-articles variance ($\sigma^2 = 132.3$); 8.4% was within-articles but between-samples variance ($\sigma^2 = 21.3$); 37.7% was within-samples but between-tasks variance ($\sigma^2 = 95.7$).

To test our main hypothesis, we proceeded by adding effort as a predictor to the model. As predicted, the effect of effort was significant, $\beta = 0.85$, $SE = 0.06$, 95% CI [0.73, 0.96], $t(33.2) = 14.7$, $p < .001$. The effect was large: with each point increase in effort, negative affect increased by 0.85 point on average (Figure 2). To check the robustness of this association, we explored the impact of influential cases. To that end, we excluded 16 tasks that had a Cook's distance larger than 0.4 or a $df\beta$ value outside the (−0.2, 0.2) range and reran our model, which did not substantially change our results, $\beta = 0.82$, $SE = 0.05$, 95% CI [0.72, 0.92], $t(20.2) = 17.0$, $p < .001$.

Figure 2
Plot of the Relationship Between Mean Effort and Mean Negative Affect



Note. Dots represent tasks. Dots are scaled to sample size; the smallest dot represents a sample size of 11; the largest dot represents a sample size of 114. The solid black line reflects the estimate from the multilevel metaregression model described in the main text. Black, dashed lines reflect the 95% confidence interval around that estimate. The light gray line in the background reflects the estimate from the quadratic model described in the main text. See the online article for the color version of this figure.

We finally tested whether the association between mental effort and negative affect may be better described as a curve (rather than a line). We did this to test the possibility that the link between effort and negative affect is U-shaped, such that (very) low and (very) high levels of effort both feel aversive. To that end, we ran a meta-analytic model that included both a linear and a quadratic term for effort. The quadratic model fit the data somewhat better than the linear model (Akaike information criterion$_{\text{linear}}$ = 2639.3, Akaike information criterion$_{\text{quadratic}}$ = 2633.0, Bayesian information criterion$_{\text{linear}}$ = 2658.7, Bayesian information criterion$_{\text{quadratic}}$ = 2656.3, likelihood ratio test = 8.3, $p = .004$). Nevertheless, the quadratic term was not significant, $\beta = 0.005$, $SE = .003$, $t(23.2) = 1.7$, $p = .104$. For descriptive purposes, we plotted this model's estimates in Figure 2 as a light gray line.

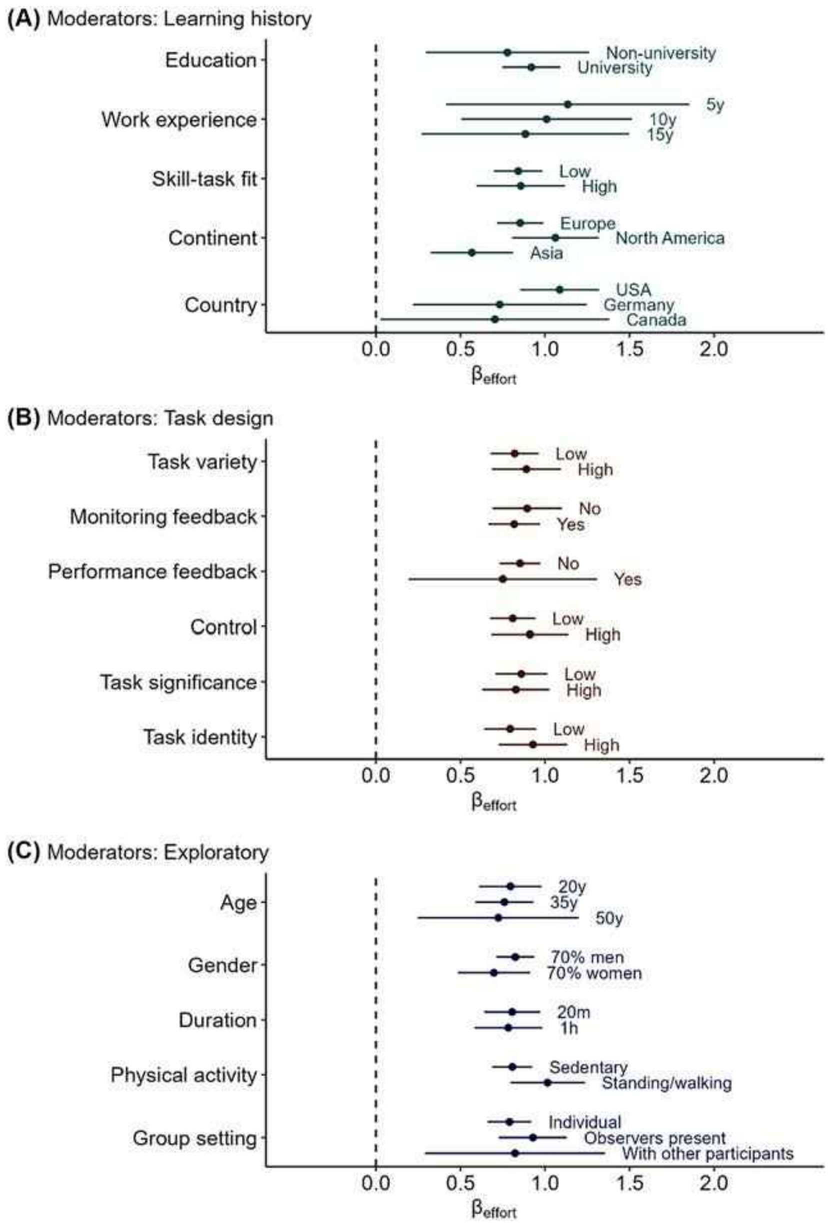
Moderator Analysis

Results from our moderator analyses are summarized in Figure 3 (and in more detail in the Supplemental Tables S4–S7). We first examined the moderators related to learning history. These moderators did not significantly interact with effort (Supplemental Table S4), except for continent. That is, we found no evidence that the association between effort and negative affect depended on education ($\beta_{\text{Effort} \times \text{Education}} = 0.14$, 95% CI [−0.33, 0.61], $p = .515$), work experience ($\beta_{\text{Effort} \times \text{Work-Experience}} = 0.03$, 95% CI [−0.11, 0.06], $p = .362$), or skill–task fit ($\beta_{\text{Effort} \times \text{Skill–Task-Fit}} = .01$, 95% CI [0.28, 0.31], $p = .918$). However, we found that effort was less strongly associated with negative affect in studies conducted in Asia, compared to studies from Europe ($\beta_{\text{Effort} \times \text{Continent}} = -0.29$, 95% CI [−0.55, −0.02], $p = .046$) and North America ($\beta_{\text{Effort} \times \text{Continent}} = -0.49$, 95% CI [−0.83, −0.16], $p = .006$; see Figure 4). Importantly, within each level of each moderator, there was a clear relationship between effort and negative affect ($\beta > .56$), including in studies conducted in Asia. So, effort felt aversive regardless of education, work experience, skill–task fit, or geographical location.

We next examined task design moderators. None of the moderators related to task characteristics significantly interacted with effort (Supplemental Table S5). That is, we found no evidence that the association between effort and negative affect depended on task variety ($\beta_{\text{Effort} \times \text{Task-Variety}} = 0.07$, 95% CI [−0.16, 0.30], $p = .539$), monitoring feedback ($\beta_{\text{Effort} \times \text{Monitoring-Feedback}} = -0.08$, 95% CI [−0.32, 0.17], $p = .530$), performance feedback ($\beta_{\text{Effort} \times \text{Performance-Feedback}} = -0.10$, 95% CI [−0.65, 0.44], $p = .658$), control ($\beta_{\text{Effort} \times \text{Control}} = 0.10$, 95% CI [−0.14, 0.34], $p = .388$), task significance ($\beta_{\text{Effort} \times \text{Task-Significance}} = -0.03$, 95% CI [−0.27, 0.21], $p = .784$), or task identity ($\beta_{\text{Effort} \times \text{Task-Identity}} = 0.13$, 95% CI [−0.11, 0.38], $p = .261$). Also here, within each level of each moderator, effort was associated with negative affect ($\beta > .75$; Figure 3). So, effort felt aversive on varied and repetitive tasks, on tasks with feedback and with no feedback, on tasks with high and low control, on tasks with high and low significance, and on tasks with high and low task identity. These results did not meaningfully change when we tested all six task design moderators together in one model (Supplemental Table S6).

Finally, we examined our exploratory moderators: age, gender, task duration, physical activity, and group setting. None of these

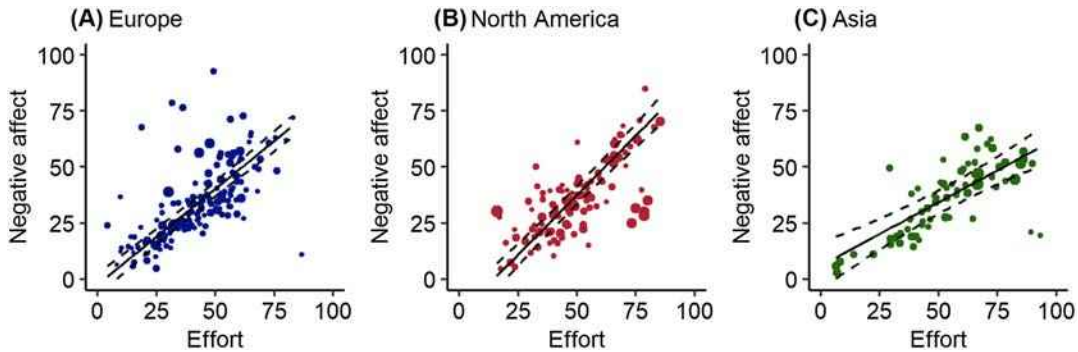
Figure 3
Results From Moderator Analysis



Note. The horizontal axis represents the metaregression parameter for the effect of effort on negative affect, separately for different moderator categories (for continuous moderators: for different representative values of the moderator). Error bars reflect 95% confidence intervals around the estimate. y = year; h = hour; m = minute. See the online article for the color version of this figure.

moderators significantly interacted with effort (Supplemental Table S7). That is, we found no evidence that the association between effort and negative affect depended on age ($\beta_{\text{Effort} \times \text{Age}}$ = 0.00, 95% CI [−0.02, 0.01], p = .730), gender ($\beta_{\text{Effort} \times \text{Gender}}$ = −0.32, 95% CI [−0.74, 0.11], p = .124), duration ($\beta_{\text{Effort} \times \text{Duration}}$ = 0.00, 95% CI [~0.00, ~0.00], p = .594), physical activity ($\beta_{\text{Effort} \times \text{Physical-Activity}}$ = 0.21, 95% CI [−0.02, 0.44], p = .069), or group setting (individual vs. observers present: $\beta_{\text{Effort} \times \text{Group-Setting}}$ = 0.14, 95% CI [−0.09, 0.37], p = .227; individual vs. together with others, $\beta_{\text{Effort} \times \text{Group-Setting}}$ = 0.03, 95% CI [−0.48, 0.54], p = .891). Here too, for each level of each moderator, effort was associated with negative affect (β > .69; Figure 3). So, effort felt aversive regardless of people's age and gender; regardless of whether tasks were short or long; regardless of whether people sat, stood, or walked; and regardless of whether people were alone or were watched or joined by other people.

Figure 4
The Association Between Effort and Negative Affect on Three Continents



Note. Solid lines represent estimates from the model described in the main text. Dashed lines indicate 95% confidence intervals. Dots are scaled to sample size. See the online article for the color version of this figure.

Robustness Analysis (Exploratory)

So far, we found that mental effort is associated with negative affect across tasks and populations. In principle, this finding is consistent with the idea that mental effort is inherently aversive. Still, it is important to note that meta-analysis is an observational technique. So, we cannot exclude the possibility that the association between effort and negative affect is spurious—that is, due to a third variable (Lipsey, 2003). Specifically, if subjective mental effort and negative affect are both triggered by some common cause (e.g., some property of the task or the population), this could explain the association, at least in part. To test this possibility, we ran a model in which we predicted negative affect from effort, as before. However, we now also included the 10 moderators for which we had >90% valid data points: skill–task fit, continent, all six task design moderators, physical activity, and group setting. We found that the association between effort and negative affect was in the same range as it was in our main analysis, $\beta = 0.88$, $SE = 0.06$, 95% CI [0.76, 1.00], $t(26.0) = 14.9$, $p < .001$ (for details, see Supplemental Table S8). Thus, the association between mental effort and negative affect cannot be explained by the possibility that both were caused by any of these 10 variables.

A further potential threat to our conclusions is that the association between effort and negative affect may be inflated by response biases (e.g., acquiescence bias). To assess whether response biases may have affected our conclusions, we conducted computer simulations. Findings suggest that, even if these response biases would have been extremely strong in the original samples, our main conclusion would not change. Details are reported in the Supplemental Material.

Discussion

This meta-analysis shows that mental effort is strongly associated with negative affect across populations and tasks. As for populations, mental effort felt aversive among university and nonuniversity educated people for experienced and inexperienced workers, regardless of whether people received specific training for the task at hand. Moreover, mental effort felt aversive in Europe, in North America, and, to a lesser extent, in Asia. As for tasks, mental effort felt aversive in varied and repetitive tasks, in tasks with and without feedback, regardless of whether people had control over how to plan or carry out the task, regardless of whether the task affected some real-life outcome, and regardless of whether the task had a clear beginning and end. Together, the link between mental effort and negative affect was ubiquitous, suggesting that mental effort is inherently aversive.

Theoretical Implications

Our study provides a new, crucial piece of support for models—for example, from psychology, economics, and cognitive neuroscience—that assume that mental effort is costly (Douglas & Shepherd, 2002; Holmstrom & Milgrom, 1994; Shenhav et al., 2017; Silvetti et al., 2018; Vassena, Deraeve, & Alexander, 2017; Vassena, Holroyd, & Alexander, 2017; Verguts et al., 2015; Vogel et al., 2020). That is, across populations and tasks, our study showed that people's subjective experiences corroborate the assumption that mental effort is perceived as a cost. At the same time, our conclusion that mental effort is inherently aversive also raises a problem (Inzlicht et al., 2018): What should we do with the idea that mental effort sometimes can feel positive? Or, to use a real-world example, if mental effort is inherently unpleasant, why do millions of people play chess?

Our preferred post hoc explanation is that high-mental-effort activities may be pleasant despite the effort, not because of it. Specifically, the utility of daily-life activities, like playing chess, can be conceptualized as a compound of costs (e.g., effort costs; opportunity costs) and benefits (e.g., monetary rewards; social rewards; mastery- and challenge-related rewards). It may often happen that this compound (sometimes called the *integrative value signal* or the *net value*; Apps & Ramnani, 2014; Vassena et al., 2015) turns out to be positive on balance. Similarly, people may *learn* that exerting mental effort—at least in the context of some specific activities—is likely to lead to reward. To return to our chess example, if the benefits of chess outweigh the costs, people may choose to play chess and even self-report that they enjoy chess. Crucially, we suggest that this does *not* imply that people enjoy the mental effort that is involved.

This explanation—which we call the *integrative value account*—can be reconciled with research on the need for cognition, which suggests that there are individual differences in the tendency to seek out and enjoy mentally effortful activities (Cacioppo et al., 1996). Still, this reconciliation requires the assumption that people higher in the need for cognition either (a) place greater value on the rewards typically associated with mentally effortful activities (e.g., feelings of mastery, self-efficacy, and competence; see Gheza et al., 2023) or (b) experience a higher probability of these rewards (e.g., because they perform relatively well at mentally effortful activities).[1] Under this assumption, people higher in need for cognition assign higher expected value to mentally effortful activities and, thus, are more likely to seek out mentally effortful activities and are more likely to receive and enjoy the rewards associated with these activities. Yet, even for people high in need for cognition, the expenditure of mental effort may feel unpleasant. This interpretation is viable given that several items of the need for cognition scale emphasize reward (e.g., "I really enjoy a task that involves coming up with new solutions to problems"; Cacioppo & Petty, 1982). Also, previous experiments suggest that people high in need for cognition are more motivated to avoid negative consequences (such as failure) during mental tasks that they expect to be difficult (Steinhart & Wyer, 2009).

To illustrate the latter point in terms of our chess example, people higher in the need for cognition may place higher value on the rewarding aspects of chess (e.g., feeling competent; winning games; accumulating rating points). Also, people higher in the need for cognition may experience such chess-related rewards more frequently because they tend to be better at chess. For these reasons, people high in need for cognition may be more likely to seek out and enjoy chess. Yet, even for them, we predict that mental effort will still feel unpleasant.

The integrative value account is consistent with a growing body of computational and neuroimaging research (Silvestrini et al., 2023). Specifically, most computational models of effort allocation include a cost function that assumes that higher effort equals higher cost. In these models, this cost is typically traded off against prospective rewards. As a result, tasks that require more effort have a lower net value. Thus, consistent with our interpretation and with classic work in psychology (Brehm & Self, 1989; Hull, 1943; Kahneman & Peavler, 1969; Richter et al., 2016), modern computational models suggest that people exert effort despite the inherent cost and only when these costs are compensated by a sufficiently valuable reward.

Though computational models of effort converge on the idea that effort is a cost, they differ in what other decision-relevant variables they include. For example, some emphasize the uncertainty of the outcome; others incorporate the volatility of the environment, the presence of punishments, or the value of obtaining information (Gottlieb et al., 2020; Shenhav et al., 2013; Silvetti et al., 2011, 2023). In this sense, *value* can be conceptualized as a multifaceted construct that is influenced by a range of extrinsic and intrinsic factors, which together can balance out the cost of exerting effort. Importantly, the precise nature of the cost of effort remains debated (Shenhav et al., 2017). One could speculate that exerting effort on one task makes it impossible to invest effort in some other task at the same time (i.e., there are *opportunity costs*; Dora et al., 2022; Kurzban et al., 2013). Relatedly, one recent model suggests that deep focus on one task reduces people's ability to multitask (Musslick & Cohen, 2021). Alternatively, one could argue that the exertion of effort carries some biological cost within the neural systems involved in cognitive control (Holroyd, 2016; Silvetti et al., 2023; Wiehler et al., 2022). Thus, although our integrative value account is broadly consistent with modern computational models, we should note that these models have not yet reached consensus on *why* mental effort is costly.

From a neural perspective, the computation of integrative value has mostly been associated with the dorsal anterior cingulate cortex (Silvetti et al., 2023; Vassena et al., 2014). Interestingly, though, cognitively demanding tasks also elicit activity in the anterior insula (Engström et al., 2015; Otto et al., 2014), a brain region that is implicated in interoception (Simmons et al., 2013). Speculatively, this system—that includes the dorsal anterior cingulate cortex and anterior insula—may integrate objective performance outcomes, environmental states, and bodily states, which ultimately results in the experienced aversiveness of effort.

Regardless of the specific neural mechanisms, the line of reasoning laid out above implies that there is a dissociation between choices (to pursue some high-effort activity) and feelings (during that high-effort activity). This putative dissociation is important, especially from a clinical perspective. Specifically, prior research suggests that people with depression and schizophrenia (vs. healthy controls) tend to avoid high-effort activities (Culbreth et al., 2018). In this area, the dominant paradigm focuses exclusively on people's choices: Researchers tend to use laboratory tasks in which participants choose between two behavioral options that vary in effort requirements and reward value (Culbreth et al., 2018; for an exception, see Brinkmann & Gendolla, 2007). However, as choices (to pursue some high-effort activity) and feelings (during that high-effort activity) may be dissociable, this approach may not tell us a lot about patients' subjective experience when they exert effort. It is an interesting avenue for future research to examine the aversiveness of mental effort directly, using subjective measures, in clinical samples.

Finally, it is important to note that people may justify their effort expenditure after the fact. That is, when people do something unpleasant to attain some goal (e.g., exert effort, endure pain, undergo humiliation), they may later infer that that goal must have been very valuable to them (Aronson & Mills, 1959). After all, why else would they have carried out the aversive action? This notion of *effort justification* has its roots in dissonance theory, and—although the exact mechanisms are still under debate (e.g., Zentall, 2010)—modern approaches (e.g., related to the *effort heuristic*, Kruger et al., 2004; Ziano et al., 2023, and the so-called *Ikea effect*, Norton et al., 2012) resonate with this idea. In any case, effort justification models reconcile two empirical facts, that is, the finding that effort is experienced as aversive (e.g., this meta-analysis; see also Devine et al., 2023; Vogel et al., 2020), and the finding that experiencing effort causes people to value the outcome of their effort more. From this perspective, too, it makes sense that, even though mental effort may be inherently aversive, people sometimes seek out mentally effortful activities.

Alternative Models

We will now consider four ways in which one can account for our findings without assuming that mental effort is inherently aversive: First, all studies that we included were instances of human-subject research. In such research, participants typically follow a series of instructions for which they get some external reward, such as a monetary payment. So, most tasks that we included in our study could still be said to be *extrinsically motivated*. Conversely, it is possible that effort can only feel pleasant during *intrinsically motivated* activities, that is, during activities that people carry out

[1] We thank Sean Devine for suggesting this interpretation.

naturally without some external goal or reward, during which people typically feel more task enjoyment. Though intuitively plausible, we do not think this account is viable. Specifically, it relies on the assumption that external rewards (e.g., money) decrease intrinsic motivation, and this assumption has often been challenged (Cerasoli et al., 2014; Gerhart & Fang, 2015). Therefore, even if all participants were paid in the included studies, there should still be meaningful variation in intrinsic motivation. Specifically, *self-determination theory* (Ryan & Deci, 2000) predicts more intrinsic motivation in tasks that allow people to make their own decisions (which should satisfy the *need for autonomy*), in which people receive feedback (which should satisfy the *need for competence*), and in which people work together with others (which should satisfy the *need for relatedness*). We found no evidence that these moderators mattered. Similarly, we found no evidence for a role of task variety and task significance, which may also be linked to intrinsic motivation (e.g., Bailey & Madden, 2016). So, our findings provide no hints that intrinsic motivation dampens the aversiveness of effort.

Second, although participants were externally rewarded for taking part in the included studies, they were usually not paid as a function of their task performance (see the Deviations From Preregistration section). Instead, in most studies, participants received a fixed monetary payment for their time (e.g., as is common in experimental psychology), or they participated as a part of their work tasks (e.g., as is common in applied research). Thus, regardless of how much mental effort was demanded by the task, participants' compensation was similar. This feature of the included studies leaves open the possibility that participants who took part in more demanding studies (and thus experienced more mental effort) were more likely to feel underrewarded. One could thus argue that the negative affect that we observed in our meta-analysis stemmed from a mental effort–reward discrepancy, not from mental effort per se. Under this account, mental effort is not inherently aversive; instead, mental effort only becomes aversive when coupled with low reward (Dragano et al., 2017; Kurzban et al., 2013; Rugulies et al., 2017; Siegrist, 1996). We cannot rule out this possibility. Future research is needed to test directly whether performance-contingent rewards can dampen the aversiveness of effort (but see Garrison et al., 2024). A general challenge for this future research is that it may be hard to empirically distinguish the (decreased) aversiveness of mental effort from the (increased) experience of reward associated with the task.

Third, prior research suggests that people often hold folk-psychological beliefs, or *lay theories*, about mental work. For example, research has documented that people often hold beliefs about intelligence ("You cannot change your intelligence"; Dweck & Yeager, 2019), concentration ("You can change how much you mind wander"; Zedelius et al., 2021), and self-regulation ("After mental activity, your energy is depleted and you must rest to get it refueled again"; Job et al., 2015). Although such lay theories may not always be accurate, they affect people's judgments in various ways (Zedelius et al., 2017). Speculatively, it could be the case that many people hold the lay theory that difficult tasks—or tasks that require mental effort—tend to be unpleasant. If true, mental effort may not be inherently aversive. Instead, its aversiveness would result from people's folk-psychological expectations. A challenge for this account, however, is that lay theories should be expected to vary between populations (Haslam, 2017), and we found little evidence for this idea (but see the following section, under the Diminished aversiveness of effort in studies from Asia section).

Fourth, in all studies that we included, mental effort was passively observed rather than manipulated. As a result, our findings cannot be used to make claims about the causal chain of processes that mediate the mental effort–negative affect relationship. As mentioned in the introduction, a well-established model is that negative affect plays a role in initiating cognitive control (Botvinick, 2007; Dignath et al., 2020; Dreisbach & Fischer, 2012; Inzlicht et al., 2015), which is widely assumed to be effortful (Silvestrini et al., 2023). Under this account, mental effort is inherently aversive. Nevertheless, we cannot exclude other mechanisms. For example, one could speculate that when people experienced negative affect during task performance, they may *infer* from this feeling that the task must have been effortful. Under this account, mental effort is not inherently aversive—rather, the feeling of mental effort results from negative affect. Experimental work that measures the physiological correlates of mental effort (e.g., using designs akin to Bogdanov et al., 2022; Devine et al., 2023) is necessary to further unravel the causal chain of processes that link effort and negative affect.

Diminished Aversiveness of Effort in Studies From Asia

The finding that mental effort felt less aversive in studies conducted in Asia is intriguing. This finding fits the general idea that the aversiveness of effort depends on people's learning history. In Asian countries, especially in China, it is relatively common for high school students to spend $\geq$60 hr per week on school work (based on a large-scale international study of 15 year olds; Organisation for Economic Co-operation and Development, 2017). Moreover, in Japan, it is common for children to spend time in *juku* (private tutoring schools) after regular school hours to cram for exams (Lowe, 2015). So, speculatively, continent-level differences in exposure to mental effort in educational settings may explain our finding. Yet, we cannot exclude two further explanations. First, it may be the case that words like "effort" and "annoyed" have different connotations in different languages (Steele, 2020), leading people to respond differently to translated items. Second, it may be the case that researchers from different continents favor tasks with different characteristics. This explanation seems somewhat unlikely, though, as we coded six core task characteristics, which did not significantly affect the aversiveness of mental effort.

Strengths and Limitations

We highlight that we analyzed sample means rather than individual responses. Thus, our findings showed that when *a sample* of participants felt more effort on average, *that sample* also tended to feel more negative affect on average. A clear advantage of this approach is that our findings cannot be explained by individual-level response biases (see simulations in Supplemental Material). However, a drawback of this approach is that we need to be cautious to generalize our conclusions to the individual level. That is, our meta-analysis does not show that when one person experiences more effort, that person will also experience more negative affect (some previous studies do show this, e.g., Hart & Staveland, 1988).

A potential criticism of our study may stem from the fact that, in ergonomics, the NASA-TLX is often used to measure a single construct called "workload." "Workload" is typically computed as the sum (or mean) of effort, negative affect, perceived performance, and three items that measure perceived task demands. If one accepts "workload" as a meaningful construct, one could argue that effort and negative affect are simply part of the same construct—and thus, that the association between effort and negative affect is trivial. To argue against this line of reasoning, we note that the compound concept of "workload" is controversial, even in ergonomics (e.g., the concept of "workload" lacks precision and explains behavior only superficially; Dekker & Hollnagel, 2004). Moreover, we note that mental effort and negative are clearly dissociable, in that people can readily experience negative affect without experiencing mental effort (e.g., when they feel bored, sad, or lethargic; see Russell & Carroll, 1999). Indeed, from a psychological perspective, it makes little sense to lump effort and negative affect together in one measure. In our view, these constructs are distinct, and it is worthwhile to study their association (Inzlicht et al., 2018).

As the number of studies that used the NASA-TLX is large, we decided to start coding the most recent studies and then work backward in time until our resources ran out, including all articles to which we had digital access. Although this strategy was successful—that is, it led us to include a substantial number of studies with substantial diversity—we should note that (a) we ended up including only studies that were published in 2019 and 2020, and (b) there were several articles that we discarded because we could not access them (see the Method section). Also, we note that we only included articles written in English. Altogether, we cannot exclude the possibility that our search-and-inclusion strategy led to biases (e.g., because more recent studies systematically differ from older studies; because the studies we could access systematically differ from the studies we could not access; because studies reported in English systematically differ from studies reported in other languages). Thus, future rapid reviews on the NASA-TLX could consider different sampling strategies (e.g., sampling randomly from the literature to also include older studies). Similarly, we cannot be certain that the tasks we included are fully representative of tasks that are used in the real world, nor can we be certain that we captured all relevant moderators.

The availability of the primary data from the studies we included was suboptimal. Specifically, only eight out of 125 articles included a link to a public repository from which the data could be downloaded. Thus, there is ample room for improvement regarding transparency and openness in this research area. Although we were able to extract our measures of interest from the articles that did not publicly share their data, it would greatly benefit future meta-analyses if researchers who use the NASA-TLX would make their participant-level data more accessible. This would allow the research community to study the link between mental effort and negative affect in a more fine-grained manner. For example, this could be done by examining whether the aversiveness of effort is subject to circadian or seasonal variations or whether it varies with personality traits (such as conscientiousness; Bates, 2024).

A final limitation of our study is that effort was measured solely using self-report. Although physiological effort (during mental activity) and the feeling of effort are correlated (Bijleveld, 2018), further research is necessary to examine the relationship between physiological and behavioral measures of effort on the one hand and negative affect on the other. Also, the NASA-TLX has limitations. That is, although negative emotions are heterogeneous and separable (Walters & Simons, 2022), the NASA-TLX lumps together several types of task-related negative affect (feeling insecure, discouraged, irritated, stressed, and annoyed) in one dimension. Thus, one could argue that the NASA-TLX is too coarse. Conversely, one could also argue the opposite, that is, that the NASA-TLX is too narrow. After all, the NASA-TLX does not capture all forms of task-related negative affect (e.g., it does not capture boredom and fatigue). We were aware of these limitations from the start of this study. Nevertheless, despite its shortcomings, the NASA-TLX is so widely used that it has allowed us to assess the relationship between effort and negative affect on a large, near-global scale. This would not have been feasible with any other meta-analytic approach. We thus feel that the advantages of the NASA-TLX outweigh the disadvantages, but we note that future research needs to look at the aversiveness of mental effort in complementary ways as well—in particular, by using different strategies to measure effort and negative affect.

Conclusion

The main finding from this meta-analysis is that mental effort is strongly associated with negative affect. We found this association in all types of tasks that we studied, including tasks that have motivating features (e.g., tasks in which people have autonomy, tasks in which people receive feedback, and tasks in which performance has real-world consequences). Furthermore, we found this association in all types of populations that we studied, including populations in which mental effort likely had been rewarded in the past (e.g., experienced professionals, university-educated people). In sum, the association between mental effort and negative affect proved to be robust and generalizable across a wide range of tasks and populations.

This meta-analysis provides a new, central piece of evidence for models that assume that mental effort is costly. This assumption is made in various fields (e.g., psychology, neuroscience, and economics), but it is not often tested. Our meta-analysis supports the assumption that mental effort is costly by showing that mental effort is consistently accompanied by negative affect.

This meta-analysis speaks to the open challenge that is commonly referred to as the effort paradox (Inzlicht et al., 2018): If mental effort is consistently unpleasant, why do people still voluntarily pursue mentally effortful activities? For example, why do millions of people play chess? Our results suggest that there may be a dissociation between choices and feelings: When people choose to pursue mentally effortful activities, this should not be taken as an indication that they enjoy mental effort per se. Perhaps people choose mentally effortful activities despite the effort, not because of it.

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Received September 23, 2022
Revision received May 23, 2024
Accepted May 24, 2024 ■

Supplementary information with *The unpleasantness of thinking: A meta-analytic review of the association between mental effort and negative affect*

by Louise David, Eliana Vassena, & Erik Bijleveld (2024)

Table of contents

Associations between moderators

Table S1. Association strength (Cramer's V) between all categorical moderators.

		1	3	4a	4b	5	6	7	8	9	10	14	15
1	Education (2)	1											
3	Skill–task fit (2)	.29	1										
4a	Continent (3)	.10	.16	1									
4b	Country (3)	.05	.15	1	1								
5	Skill variety (2)	.26	.56	.16	.25	1							
6	Monitoring feedback (2)	.09	.14	.09	.17	.13	1						
7	Performance feedback (2)	.01	.04	.21	.50	.05	.21	1					
8	Control (2)	.25	.48	.13	.14	.66	.10	.06	1				
9	Task significance (2)	.19	.66	.16	.21	.66	.22	.00	.48	1			
10	Task identity (2)	.09	.58	.13	.28	.68	.07	.09	.61	.57	1		
14	Physical activity (2)	.01	.09	.08	.21	.10	.11	.09	.04	.09	.03	1	
15	Group setting (3)	.27	.33	.14	.25	.28	.05	.13	.24	.29	.21	.40	1

Note: The numbering in the leftmost column corresponds to the numbering in Table 1 in the main text. Numbers between brackets refer to the number of categories of that moderator. Derived from $\chi2$, Cramer's V is an effect size measure for pairs of nominal variables. V can range between 0 (no association) and 1 (perfect association). V is sometimes referred to as $\varphi_{C.}$

Following common rules of thumb (Cohen, 1988, *Statistical power for the behavioral sciences*, 2nd Ed.), the interpretation of V depends on the lowest number of categories in the variable pair.

- If the variable with the lowest number of categories has 2 categories, V is considered large when V ≥ .50.
- If the variable with the lowest number of categories has 3 categories, V is considered large when V ≥ .35.

Shaded cells in Table S1 contain effect sizes that can be considered large, following these rules of thumb.

To interpret the 10 large associations in Table S1, we computed odds ratios (ORs). We report these by going through Table S1 column by column, from left to right:

- Tasks that had high skill–task fit, were more likely to also have high task variety (OR = 15.6), high task significance (OR = 95.5), and high task identity (17.3)
- Naturally, there was a perfect association between continent and country.
- Tasks from Canada were more likely to have continuous performance feedback, when compared to tasks form the USA (OR = 11.9) and Germany (OR = 15.4).
- Tasks that had high skill variety, were more likely to also have high control (OR = 29.9), high task significance (OR = 28.4), and high task identity (OR = 27.8).
- Tasks that had high control, were more likely to also have high task significance (OR = 10.2).
- Tasks that had high task significance, were more likely to also have high task identity (OR = 15.2).

Table S2. Association strength (η^2) between the four continuous moderators (columns) with the categorical moderators (rows).

		Work experience	Age	Gender	Duration of task
1	Education	.00	.32	.02	.02
3	Skill–task fit	.02	.07	.00	.06
4a	Continent	.16	.02	.00	.02
4b	Country	.01	.12	.02	.06
5	Skill variety	.00	.05	.01	.03
6	Monitoring feedback	.06	.02	.04	.01
7	Performance feedback	.00	.00	.01	.00
8	Control	.02	.01	.02	.01
9	Task significance	-	.09	.02	.04
10	Task identity	.01	.02	.00	.01
14	Physical activity	.10	.02	.00	.01
15	Group setting	.14	.00	.03	.17

Note: η^2 is an effect size measure that can be used to quantify the strength of association between continuous and categorical variables. Shaded cells contain effect sizes that can be considered large, $\eta^2 \geq .14$, following common rules of thumb (Cohen, 1988). We were unable to compute η^2 for the link between task significance and work experience, because where we could code work experience (k = 58), task significance was always high.

To interpret the 4 large associations in Table S1, we interpreted the means of the continuous moderators separately for all levels of the relevant categorical moderators. We report these by going through Table S2 column by column, from left to right:

On average, participants in studies conducted in Europe (M = 13.3y, SD = 8.8y) and Asia (M = 10.5y, SD = 11.8y) had more work experience than participants in studies conducted in North-America (M = 4.1y, SD = 3.9y).

On average, participants who took part in studies individually (M = 11.3y, SD = 9.9y) or with observers present (M = 15.0y, SD = 10.1y), had more work experience than participants who took part in studies that required them to engage with others (M = 5.9y, SD = 6.5y).

On average, participants without university education (M = 35.8y, SD = 12.2y) were older than participants with university education (M = 24.4y, SD = 4.0y).

On average, tasks that were administered with observers present were relatively short (M = 14.7m, SD = 16.6m). Tasks that required participants to engage with others were relatively long (M = 84.3m), though there was a lot of variation (SD = 123.0). Tasks that were conducted individually fell in between (M = 38.7m, SD = 16.6m).

Table S3. Association strength (Pearson r) between the four continuous moderators.

	Work experience	**Age**	**Gender**	**Duration of task**
Work experience	1			
Age	.89	1		
Gender	-.29	-.20	1	
Duration of task	-.16	.03	.24	1

Note: Work experience and age were coded in years; gender was coded as the proportion of females in the sample; duration of task was coded in minutes.

Following common rules of thumb (Cohen, 1988), we note the strong correlation between age and work experience. Naturally, participants with more work experience also tended to be older.

Detailed results of moderator analyses

Table S4. Moderator analyses for learning history moderators.

	Moderator	Term	β	95%CI	Q_M	Q_E	I^2
1	Education	Intercept	32.6 ***	[23.0, 42.3]	341.7 ***	35486.8 ***	97.8
		Effort	0.8 **	[0.3, 1.3]			
		Education = University	3.0	[-6.6, 12.7]			
		Effort × Education	0.1	[-0.3, 0.6]			
2	Work experience	Intercept	34.4 ***	[24.4, 44.4]	296.1 ***	8436.5 ***	98.8
		Effort	1.0 **	[0.5, 1.5]			
		Work experience	-0.3	[-1.5, 0.9]			
		Effort × Work experience	0.0	[-0.1, 0.1]			
3	Skill-task fit	Intercept	35.5 ***	[32.8, 38.2]	554.9 ***	33284.2 ***	97.2
		Effort	0.8 ***	[0.7, 1.0]			
		Skill–task fit = High	-0.7	[-5.8, 4.3]			
		Effort × Skill–task fit	0.0	[-0.3, 0.3]			
4a	Continent	Intercept	34.3 ***	[30.2, 38.4]	657.7 ***	25830.8 ***	97.2
		Effort	1.1 ***	[0.8, 1.3]			
		Continent = Asia	-1.7	[-7.2, 3.9]			
		Continent = Europe	3.4	[-2.0, 8.9]			
		Effort × Continent = Asia	-0.5 **	[-0.8, -0.2]			
		Effort × Continent = Europe	-0.2	[-0.5, 0.1]			
4b	Country	Intercept	36.2 ***	[32.3, 40.1]	285.0 ***	1324.1 ***	91.1
		Effort	1.1 ***	[0.9, 1.3]			
		Country = Canada	-7.9	[-17.4, 1.6]			
		Country = Germany	7.9 *	[0.4, 15.3]			
		Effort × Country = Canada	-0.4	[-1.1, 0.3]			
		Effort × Country = Germany	-0.4	[-0.9, 0.2]			

Note: Each moderator is tested in a separate model. In all models, the dependent variable was negative affect. * $p < .05$; ** $p < .01$; *** $p < .001$.

Table S5. Moderator analyses for task design moderators.

	Moderator	Term	β	95%CI	Q_M	Q_E	I^2
5	Skill variety	Intercept	35.2 ***	[32.9, 37.5]	567.9 ***	22433.9 ***	97.2
		Effort	0.8 ***	[0.7, 1.0]			
		Skill variety = High	0.0	[-4.5, 4.4]			
		Effort × Skill variety	0.1	[-0.2, 0.3]			
6	Monitoring feedback	Intercept	34.8 ***	[30.3, 39.3]	553.0 ***	24414.0 ***	97.2
		Effort	0.9 ***	[0.7, 1.1]			
		Monitoring feedback = Yes	0.6	[-4.2, 5.4]			
		Effort × Monitoring feedback	-0.1	[-0.3, 0.2]			
7	Performance feedback	Intercept	35.2 ***	[32.9, 37.6]	552.5 ***	38745.8 ***	97.3
		Effort	0.9 ***	[0.7, 1.0]			
		Performance feedback = Throughout	0.6	[-3.1, 4.4]			
		Effort × Performance feedback	-0.1	[-0.6, 0.4]			
8	Control	Intercept	34 ***	[32.0, 36.0]	578.7 ***	18501.6 ***	97.1
		Effort	0.8 ***	[0.7, 0.9]			
		Control = High	3.4	[-1.3, 8.0]			
		Effort × Control	0.1	[-0.1, 0.3]			
9	Task significance	Intercept	34.8 ***	[32.2, 37.4]	548.3 ***	23351.9 ***	97.1
		Effort	0.9 ***	[0.7, 1.0]			
		Task significance = High/medium	0.8	[-3.6, 5.3]			
		Effort × Task significance	0.0	[-0.3, 0.2]			
10	Task identity	Intercept	34.4 ***	[32.0, 36.9]	581.7 ***	21226.0 ***	97.2
		Effort	0.8 ***	[0.6, 0.9]			
		Task identity = High	1.8	[-3.1, 6.7]			
		Effort × Task identity	0.1	[-0.1, 0.4]			

Note: Each moderator was tested in a separate model. In all models, the dependent variable was negative affect. * $p < .05$; ** $p < .01$; *** $p < .001$.

Table S6. Moderator analyses for task design moderators.

Term	β	95%CI	Q_M	Q_E	I^2
Intercept	33.5 ***	[29.4, 37.7]	601.9 ***	13342.3 ***	97.1
Effort	0.9 ***	[0.6, 1.1]			
Skill variety	-2.4	[-7.0, 2.1]			
Monitoring feedback	1.4	[-4.3, 7.1]			
Performance feedback	1.1	[-3.1, 5.2]			
Control	4.0	[-0.8, 8.8]			
Task significance	-1.0	[-6.2, 4.2]			
Task identity	2.1	[-5.4, 9.6]			
Effort × Skill variety	0.0	[-0.4, 0.4]			
Effort × Monitoring feedback	-0.1	[-0.3, 0.2]			
Effort × Performance feedback	-0.1	[-0.5, 0.4]			
Effort × Control	0.1	[-0.3, 0.5]			
Effort × Task significance	-0.3	[-0.6, 0.1]			
Effort × Task identity	0.3	[-0.3, 0.8]			

Note: All moderators were tested together in the same model. The dependent variable was negative affect. * $p < .05$; ** $p < .01$; *** $p < .001$.

Table S7. Moderator analyses for exploratory moderators.

	Moderator	Term	β	95%CI	Q_M	Q_E	I^2
11	Age	Intercept	35.6 ***	[32.8, 38.4]	356.2 ***	38291.5 ***	98.0
		Effort	0.8 ***	[0.6, 0.9]			
		Age	-0.2	[-0.5, 0.1]			
		Effort × Age	0.0	[0.0, 0.0]			
12	Gender	Intercept	34.2 ***	[32.2, 36.2]	513.2 ***	2384.1 ***	95.2
		Effort	0.8 ***	[0.7, 0.9]			
		Gender	-5.0	[-15.2, 5.3]			
		Effort × Gender	-0.3	[-0.7, 0.1]			
13	Duration	Intercept	35.8 ***	[32.6, 39.0]	286.4 ***	1258.0 ***	92.5
		Effort	0.8 ***	[0.6, 1.0]			
		Duration	0.0	[-0.1, 0.1]			
		Effort × Duration	0.0	[0.0, 0.0]			
14	Physical activity	Intercept	36.3 ***	[33.7, 38.9]	635.0 ***	36818.5 ***	97.5
		Effort	0.8 ***	[0.7, 0.9]			
		Physical activity = Light activity	-2.3	[-6.8, 2.2]			
		Effort × Physical activity	0.2	[0.0, 0.4]			
15	Group setting	Intercept	36.9 ***	[33.4, 40.4]	551.4 ***	37516 ***	97.2
		Effort	0.8 ***	[0.7, 0.9]			
		Group setting = Together with others	-2.8	[-8.7, 3.1]			
		Group setting = Observers present	-2.8	[-6.8, 1.2]			
		Effort × Group Setting: Together w. others	0.0	[-0.5, 0.5]			
		Effort × Group Setting: Observers present	0.1	[-0.1, 0.4]			

Note: Each moderator was tested in a separate model. In all models, the dependent variable was negative affect. * p < .05; ** p < .01; *** p < .001.

Detailed results of robustness analysis

Table S8. Robustness analysis.

	Term	β	95%CI	Q_M	Q_E	I^2
	Intercept	35.8 ***	[28.5, 43.2]	597.2 ***	10030.0 ***	97.3
	Effort	0.9 ***	[0.8, 1.0]			
3	Skill–task fit = High	-1.4	[-11.3, 8.4]			
4a	Continent = Asia	-2.7	[-9.9, 4.6]			
4a	Continent = Europe	3.8	[-1.9, 9.6]			
5	Skill variety = High	-2.8	[-10.3, 4.7]			
6	Monitoring feedback = Yes	0.1	[-5.6, 5.8]			
7	Performance feedback = Throughout	0.6	[-3.8, 4.9]			
8	Control = High	5.2 *	[0.2, 10.3]			
9	Task significance = High/medium	-0.1	[-7.5, 7.2]			
10	Task identity = High	1.9	[-5.2, 9.0]			
14	Physical activity = Light activity	-3.1	[-9.7, 3.5]			
15	Group setting = Together with others	-1.7	[-8.7, 5.4]			
15	Group setting = Observers present	-2.8	[-7.1, 1.4]			

Note: All moderators are tested together in the same model. The dependent variable was negative affect. * $p < .05$; ** $p < .01$; *** $p < .001$.

Simulations to examine if individual-level response biases can account for the results

Background

One may suspect that our main finding—i.e., the association between effort and negative affect—does not reflect a true association between effort and negative affect, but that it can be explained from individual-level response biases. For example, people who are more likely to agree with questionnaire statements (i.e., people higher in acquiescence bias) will likely score higher both on effort *and* on negative affect. As a result of such variation in response tendencies, within individual samples, the correlation between effort and negative affect could be inflated or even fully spurious. However, our meta-analysis was done *across samples*, i.e., on summary statistics that were computed on the sample level. It is an open question whether within-samples correlations (which may be due to response biases) can explain our main finding. We conducted simulations to explore this question.

Method

We present four sets of simulations. In each set, we assumed a different true correlation between effort and negative affect (within individual samples). In the first set, we assumed $\rho = .30$; in the second, $\rho = .50$; in the third, $\rho = .70$; in the fourth, $\rho = .90$.

The core part of our simulation script (https://osf.io/mktbr/) worked as follows:

1) We first simulated individual datasets. In several ways, these simulated datasets mirrored the datasets that we included in our meta-analysis. That is, they had similar N ($N_{mean} = 26$, $N_{sd} = 16$, $N_{min} = 10$). Also, the sample means and standard deviations for effort and negative affect were similar to those from the real datasets. However, as described above, we assumed different correlations between effort and negative affect in the populations from which the simulated samples were drawn (i.e., different ρs). We constructed these datasets using the rnorm_multi() function from the *faux* package in R (DeBruine, 2021, https:/doi.org/10.5281/zenodo.2669586).
2) From these datasets, we constructed a meta-analysis dataset. This meta-analysis dataset included means and standard deviations for effort and negative affect, computed for each of the simulated samples. As in our meta-analysis, this meta-analysis dataset contained summary data from 357 individual samples.
3) We ran a meta-analysis on the dataset constructed at step #2, using the same procedures as described in the main text.
4) We stored the β-value for the effect of effort on negative affect.

We ran the core part of our script 4 x 1000 times, i.e., 1000 times for each correlation. We report the distributions of β-values and compare these distributions to the β of 0.85 that we found in our meta-analysis.

If within-samples correlations did not affect the outcome of the meta-analysis at all, βs should be distributed around 0 (as we did not manufacture any across-samples association in our simulations). By contrast, if our main finding can be fully explained by within-samples correlations, βs should be in same range as the β we found in our meta-analysis.

Results

Figure S1. Simulation results.

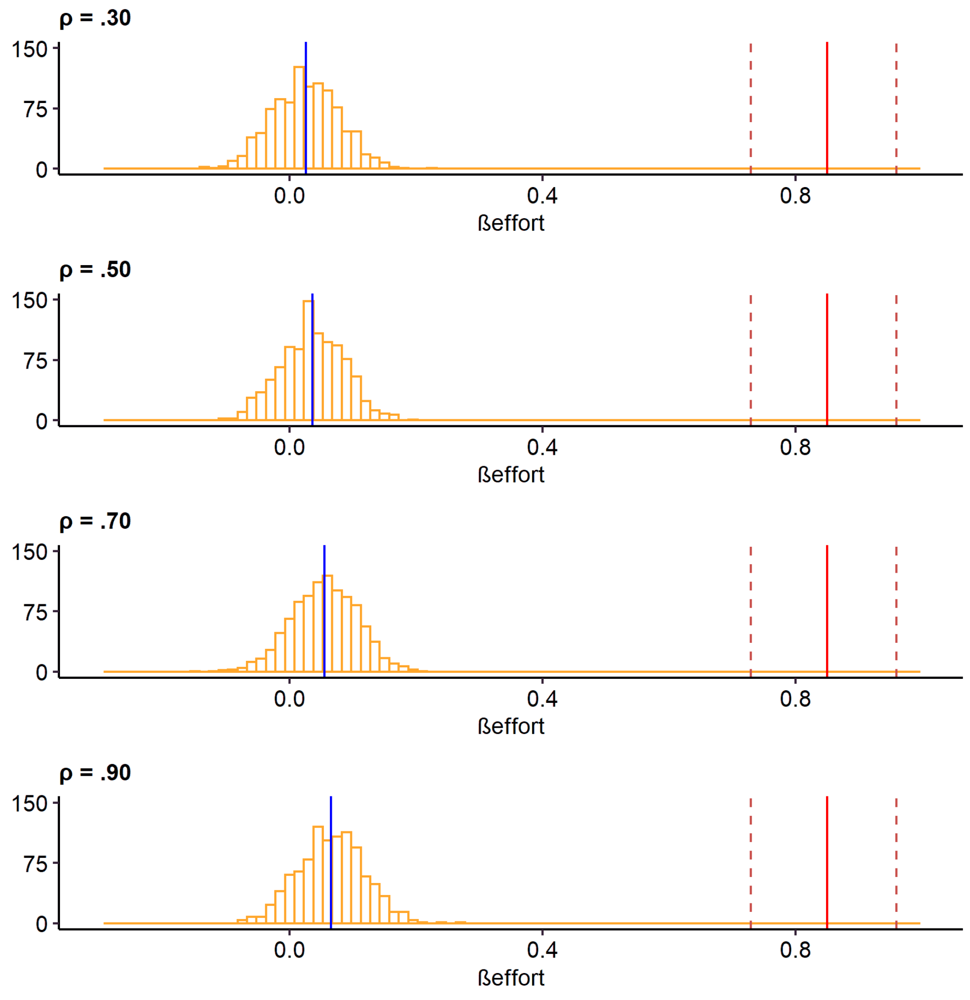

Figure S1 shows results from four sets of simulations, each with a different correlation (within individual samples). Histograms reflect the distribution of simulated β-values for the meta-analytic effect of effort on negative affect. Red vertical lines mark the β-value of 0.85 that we found in our main meta-analysis (with the 95% confidence intervals in dashed lines).

Discussion

Inspection of Figure S1 reveals that the simulated meta-analyses yielded β-values that were, on average, slightly above 0. So, if within-samples correlations were present in the original datasets (which, in turn, could be due to response biases), these may have slightly inflated the overall effect size that we found. We say 'slightly' because even when within-samples correlations were unrealistically strong (ρ = .90), the distribution of β-values centered only at around 0.07. Importantly, inspection of Figure S1 further reveals that the β-value we found in our meta-analysis was well outside the range of our simulations, suggesting that within-samples correlations (and, thus, potential response biases) cannot account for our findings.

List of papers included in the meta-analysis

Abd Rahman, N. I., Md Dawal, S. Z., & Yusoff, N. (2020). Driving mental workload and performance of ageing drivers. *Transportation Research Part F: Traffic Psychology and Behaviour*, *69*, 265–285. https://doi.org/10.1016/j.trf.2020.01.019

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