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Appealing to Motivation to Change Attitudes, Intentions, and Behavior: A Systematic Review and Meta-Analysis of 702 Experimental Tests of the Effects of Motivational Message Matching on Persuasion

Keven Joyal-Desmarais[1,2]*, Alexandra K. Scharmer[3], Molly K. Madzelan[3], Jolene V. See[4], Alexander J. Rothman[3], Mark Snyder[3]

[1]Department of Health, Kinesiology, and Applied Physiology, Concordia University, Montreal, QC, Canada
[2]Montreal Behavioral Medicine Centre, Centre intégré universitaire de santé et de services sociaux du Nord-de-l'Île-de-Montréal (CIUSSS-NIM), Montreal, QC, Canada
[3]Department of Psychology, University of Minnesota, Minneapolis, MN, USA
[4]Department of Psychiatry, University of Minnesota, Minneapolis, MN, USA

*Correspondence concerning this article should be addressed to Keven Joyal-Desmarais (keven.joyaldesmarais@gmail.com), Department of Health, Kinesiology & Applied Physiology, Concordia University, 7141 Sherbrooke Street West, Montreal, Quebec, Canada, H4B 1R6.

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Open Science Documentation. To complement the current report, a project page has been constructed (at: https://osf.io/6f24t) that links to our preregistration (https://osf.io/s63p2/) and contains data, analytic code in *R*, and research materials (including our coding scheme) used in our review.
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Abstract

Message matching refers to the design and distribution of persuasive messages such that message features (e.g., the themes emphasized) align with characteristics of the target audience (e.g., their personalities). *Motivational message matching* is a form of this technique that seeks to enhance persuasion by matching specifically to differences in motivational characteristics (e.g., salient goals, needs, values). Despite widespread use of motivational matching, there is little understanding of how and when to use it. We conducted a preregistered (PROSPERO CRD42019116688; osf.io/rpjdg) systematic review and three-level meta-analysis of 702 experimental studies on motivational matching (synthesizing 5,251 effect sizes from N = 206,482). Studies were inclusive of publications until December 2018, and primarily identified using APA *PsycInfo*, *MEDLINE*, and *Scopus*. We evaluate moderation using meta-regressions, and provide bias assessments (sensitivity analyses, funnel plots). Motivational matching increases persuasion by an average of r = .20 (95% CI: .18, .22) as assessed by differences in attitudes, intentions, self-reported behavior, and observed behavior, relative to comparison conditions. This effect is larger than previously observed for other message matching approaches (e.g., message tailoring, message framing) which usually average r < .10. Although motivational matching can effectively improve persuasion, its effects are also marked by meaningful heterogeneity. Notably, motivational matching effects are largest when matching to contextual factors (than to individual differences), when compared to messages that conflict with people's motivations, and when target characteristics are manipulated rather than assessed. Through this review, we develop and evaluate theoretical propositions that inform the optimization of motivational matching.

Keywords: message matching, message tailoring, message framing, functional matching, attitudes and persuasion

Public Significance Statement

This meta-analysis of over 700 studies shows that persuasive messages are more effective when they are designed to match the motivational underpinnings of people's beliefs and actions (e.g., their values or personality). Yet, we also show that the performance of matched messages can vary substantially. Based on our findings, we provide a set of propositions to help researchers and practitioners maximize the effective use of message matching to influence attitudes and change behavior.

Appealing to Motivation to Change Attitudes, Intentions, and Behavior: A Systematic Review and Meta-Analysis of 702 Experimental Tests of the Effects of Motivational Message Matching on Persuasion

One of the main goals of the behavioral sciences is to understand why people think and behave the way that they do and uncover the factors that lead people to change their thoughts and actions. Such knowledge is frequently used to encourage people to adopt new attitudes and behaviors, often through message-based efforts. The goals of such efforts are many and varied; they include promoting the collective welfare of society (e.g., encouraging healthier lifestyles and higher rates of altruistic behaviors), advancing commercial objectives (e.g., shaping consumers' purchases), and influencing political processes (e.g., mobilizing people to vote). At times, persuasion efforts adopt standardized "one-size-fits-all" strategies in which the same approach is used for all message recipients. However, at other times, particularly with recent technological advances, those who seek to influence people's attitudes and behaviors employ adaptive strategies that change depending on characteristics of their target audiences.

The logic of such an adaptive approach is perhaps best captured by the concept of message matching. *Message matching* is a set of techniques whereby a message's features (e.g., types of arguments used, spokespersons employed) are systematically altered to match the characteristics of the people to whom they are delivered (e.g., their needs, concerns, values, sociodemographics, or contexts: Joyal-Desmarais et al., 2020; Kreuter et al., 1999b; Kreuter & Skinner, 2000; Lavine & Snyder, 2000; Noar et al., 2007; Rakowski, 1999; Rothman et al., 2020). An example of matching involves ensuring that the source of a message (e.g., a spokesperson) is demographically similar to the target audience; for instance, a message promoting smoking cessation may be more effective if it depicts people similar to the target audience in gender, race, age, or other easily identifiable characteristics (Strecher et al., 2008).

Although there are many ways to implement message matching, one approach stands out for its emphasis on engaging the motivational processes that underlie beliefs and behavior. *Motivational message matching* (or "*motivational matching*") is an approach that seeks to persuade people by matching messages to qualitative differences in the motivational underpinnings of their thoughts and actions; these underpinnings include differences in people's goals, needs, values, motives, or concerns–which are frequently described as the psychological "functions" served by people's thoughts and behaviors (Carpenter et al., 2013; Lavine & Snyder,

2000; Maio & Olson, 2000; Shavitt, 1990; Snyder, 1993; Snyder & Cantor, 1998; Snyder & DeBono, 1985). For example, to encourage volunteerism, messages that emphasize networking opportunities are more effective for people motivated by relational goals, but messages emphasizing societal benefits are more effective for people with altruistic goals (Clary et al., 1994; 1998; Clary & Snyder, 1999). Similarly, messages that contain themes of independence are more effective when administered to members of individualistic cultures, whereas messages containing themes of interdependence with others are more effective for members of collectivistic cultures (Gardner et al., 1999; Han & Shavitt, 1994; Xue, 2015).

Motivational matching is a vast field of research, yet there has not been a systematic synthesis of this literature or a comprehensive specification of the principles that underlie its effectiveness. This gap is notable, as motivational matching may be an especially potent form of message matching. We address this gap through a large-scale review and meta-analysis of research on motivational matching and delineate three propositions that can help understand variations in its effectiveness.

First, we provide a framework to organize and bring together research traditions that have examined variations of motivational matching, but that have operated largely in isolation from one another (e.g., message framing, message tailoring). We then examine the degree to which these traditions are associated with different effect sizes. *Second*, we propose that persuasive messages can be conceptualized along a continuum defined by the degree to which they match people's underlying motivations, and we examine how the use of different comparison conditions in matching studies can impact the apparent effectiveness of motivational matching. *Third,* we examine how the effectiveness of motivational matching depends on the method and specificity with which researchers determine the characteristics (i.e., motivations) to which they match messages (i.e., whether these are directly measured, inferred, or manipulated).

Motivational Matching: A Rich Tradition in Need of Integration

Research on motivational matching originates from the functional perspective in psychology (e.g., Angell, 1907; 1909; Dewey, 1895; James, 1890; Mead, 1910), which maintains that human thoughts and behaviors can best be understood by uncovering the functions (i.e., purpose, or motivations) these thoughts and behaviors serve. In the 1950s, functional theorists extended this idea to attitude change processes, introducing the core proposition that underlies most motivational matching research to this day: that the key to changing people's attitudes and

behavior is to address the key motivations (or "functions") that underlie and support those attitudes and behavior (e.g., Katz, 1960; Katz et al., 1957; Kelman, 1958; Smith et al., 1956). Early functional theories of attitudes and persuasion emphasized an initial set of motivations that could be targeted for message development (i.e., utilitarian, social-adjustive, value-expressive, knowledge, and ego-defensive motives; Katz, 1960; Smith et al., 1956). In the decades since the introduction of this approach, message-based interventions have sought to engage a broader array of motivations that people's thoughts and behavior serve, express, and fulfill (e.g., cultural and moral values, approach/avoidance motives, political ideologies, temporal goals, need for cognition/affect, sensation seeking; Batavia et al., 2018; Campbell & Kay, 2014; Gardner et al., 1999; Haddock et al., 2008; Han & Shavitt, 1994; Martin et al., 2005; Orbell et al., 2004; Sherman et al., 2006; Voelkel & Feinberg, 2017).

Today, hundreds of articles on motivational matching have been published, examining ways that persuasion can be optimized by matching messages to relevant motivations across a range of behavioral domains, such as health (e.g., Mann et al., 2004; Orbell et al., 2004), environmental (e.g., Muralidharan & Sheehan, 2017; Scharmer & Snyder, 2021), prosocial (e.g., Clary et al., 1994; Zemack-Rugar & Klucarova, 2018), consumer (e.g., Kramer et al., 2007; Roy & Phau, 2014), and political behaviors (e.g., Binning et al., 2015; Lavine & Snyder, 1996). Yet, there has not been a formal attempt to integrate the literature within a single synthesis. Instead, the literature on motivational matching has been fragmented and the few published reviews and meta-analyses that cover motivational matching (Carpenter, 2012; Covey, 2014; Gould et al., 2013; Heo & Braun, 2014; Hornikx & O'Keefe, 2009b; Huang & Garcia, 2018; Huang & Shen, 2016; Lagisetty et al., 2017) have each focused on limited domains at a time, such as by examining specific behaviors (e.g., cancer-prevention behaviors), types of message manipulations (e.g., gain vs. loss frames), characteristics on which to match messages (e.g., cultural values), or population groups (e.g., Korean Americans). Because each review has limited coverage, there has been very little overlap in the studies they cover, even when reviews focus on conceptually similar forms of matching. For instance, Gould et al. (2013), Heo and Braun (2014), Hornikx and O'Keefe (2009b), Huang and Garcia (2018), and Huang and Shen (2016) each review cultural tailoring effects, an area largely concerned with matching messages to value-based cultural differences, but only two of these reviews (Heo & Braun, 2014; Huang & Shen, 2016) show any overlap in the articles they synthesize.

Conducting a broader synthesis of the literature can provide more precise and reliable effect size estimates. Such a review can also allow researchers and practitioners to better inform their expectations about motivational matching by documenting variation in its effectiveness. Currently, there are many claims that motivational matching is a very effective way to enhance persuasion (e.g., Carpenter et al., 2013; Lavine & Snyder, 2000; Shavitt & Nelson, 2002) and successful applications of this method are easy to locate. Yet, the literature also contains examples of mixed, null, and even adverse effects of motivational matching on persuasion (e.g., Cheong & Kim, 2011; Kareklas et al., 2012; Kim et al., 2009a). If there is a large degree of heterogeneity in the effectiveness of motivational matching, then researchers and interventionists should approach the use of this technique with caution even if the average effect is substantial.

Given these considerations, a primary goal of this systematic review and meta-analysis is to provide a precise estimate of the average effect size of motivational matching. To do so, we review evidence generated from *experimental* studies to maximize our ability to make causal inferences (Rubin, 2008; Shadish et al., 2002). We also focus on estimating effects on individuals' attitudes and intentions, as well as on self-reported and observed behavioral outcomes; doing so allows us to make more specific inferences and distinguish between proximal versus distal outcomes in behavior change (Ajzen & Fishbein, 1977; Bagozzi, 1981). To be comprehensive, we do not limit our review to a particular behavioral domain.

Before conducting the review, we can outline some expectations about the average effect size of motivational matching for enhancing persuasion. We start with the observation that meta-analyses of other variants of message matching (i.e., those matching predominantly to non-motivational characteristics such as age, or belief-based variables) have mostly found small average effects ($r < .10$; e.g., Gallagher & Updegraff, 2012; Huang & Shen, 2016; Krebs et al., 2010; Lustria et al., 2013; Noar et al., 2007; O'Keefe & Jensen, 2006; 2007; 2009; Wanyonyi et al., 2011). This observation provides a baseline from which to make predictions, as several authors have proposed that matching messages to psychologically central characteristics (e.g., core cultural values)–the type of characteristics typically targeted by motivational matching interventions–should lead to higher increases in persuasion than matching messages to demographic variables (e.g., age) or non-value-based beliefs (e.g., risk perceptions; Abrams et al., 1999; Huang & Shen, 2016; Joyal-Desmarais, 2020)–which reflect the most common types of characteristics targeted in interventions examined in previous meta-analyses. Consequently, if

motivational matching is a particularly potent form of message matching, we can expect an average effect size larger than that observed in these other literatures ($r > .10$). However, since techniques designed to improve the *relative* persuasiveness of one message over another are generally small in magnitude (O'Keefe, 2013), it is reasonable to expect motivational matching to have (at most) a moderate average effect. Following this rationale, we hypothesize that:

> **H1**: the average effect of motivational message matching should be small-to-moderate in magnitude; that is, between $r = .10$ and .30 (Cohen, 1988).

In addition to documenting the average effect size associated with motivational matching, we also seek to document how effect sizes vary across different implementations of motivational matching, as such heterogeneity has implications for the development of interventions based on motivational matching. Although many have called for a greater specification of the conditions that lead to larger versus smaller matching effects (e.g., Abrams et al., 1999; Hawkins et al., 2008; Noar & Harrington, 2016; Rothman et al., 2020; Updegraff & Rothman, 2013), progress in the search for moderators has been limited by a lack of principles to guide such work. In the sections that follow, we delineate three propositions to guide explorations of when motivational matching leads to larger versus smaller effect sizes.

Motivational Matching and its Relation to Other Approaches within Message Matching: An Organizational Framework

To adequately review research on motivational matching, it is essential to understand the breadth of works that use the technique, while also differentiating motivational matching from other forms of message matching. For this purpose, we can organize research on message matching around *four* key traditions, each of which is centered on a variation of the message matching technique, and each of which has produced research to test whether messages are more effective when they engage with people's motivational concerns. Bringing these traditions together not only affords a more comprehensive review of research on motivational matching, but also offers insights into the relative performance of different approaches to message matching.

The first of the four traditions is of particular importance to this review, as it consists of areas of research that have *exclusively* focused on understanding *motivational matching*. For example, motivational message matching is the core principle underlying classic research on functional matching (e.g., Lavine & Snyder, 2000; Shavitt & Nelson, 2002; Snyder & DeBono,

1985), moral framing research (i.e., matching to differences in political/moral values; Feinberg & Willer, 2019; Scharmer & Snyder, 2021), cultural matching to differences in collectivistic and individualistic values (e.g., Han & Shavitt, 1994), and other forms of matching centered on individual differences in motivational concerns (e.g., matching to regulatory focus, volunteer functions, or self-monitoring; Cesario et al., 2013; Clary et al., 1998; Shavitt et al., 1992). For example, high self-monitors are largely motivated by social concerns (e.g., maintaining relationships) and are more responsive to messages leveraging these concerns (e.g., appeals to social norms; DeBono, 1987; Snyder & DeBono, 1985), whereas low self-monitors are more attuned to utilitarian concerns (e.g., how good they feel a consumer product is) and are more responsive to appeals congruent with this concern (e.g., emphasizing the quality of a product; e.g., DeBono & Packer, 1991; Paek et al., 2010; Snyder & DeBono, 1985).

In contrast to works centered on motivational matching itself, other research traditions have been focused on three technical variations of message matching: (a) *message framing* techniques, (b) *message tailoring* techniques, and (c) *context matching* techniques. We examine each tradition next and highlight how each has given rise to sub-literatures that intersect with motivational matching. Table 1, Part A, summarizes the key features of each variation.

Message Framing

Message framing is an area of research that uses matching principles to predict the differential effects of messages that vary in their emphasis on the costs versus benefits obtained (or avoided), as a result of engaging in (or withholding from) a particular behavior (Cesario et al., 2013; Rothman et al., 2020). Such message variations are known as “message frames" and primarily[1] take the form of: (a) *gain frames*, emphasizing the benefits obtained by (compliance with) a behavior (e.g., “exercise leads to good health”); (b) *loss frames*, emphasizing the costs obtained by (noncompliance with) a behavior (e.g., “not exercising leads to illness”); (c) *non-gain frames* emphasizing the benefits avoided by (non-compliance with) a behavior (e.g., “not exercising prevents good health”); and (d) *non-loss frames*, emphasizing the costs avoided by (compliance with) a behavior (e.g., “exercise prevents illness”).

[1]In addition to the four types of frames presented in text (gain, loss, non-gain, non-loss frames), authors occasionally use other message manipulations that primarily reflect mixtures of the four frames defined in our text. For example, researchers may use the terms *positive frame*, or *pleasure frame*, when messages emphasize desirable outcomes (i.e., gains and non-losses are both desirable), and the terms *negative frame*, or *pain frame*, when messages emphasize undesirable outcomes (i.e., loss and non-gains are both undesirable; Cesario et al., 2013; Dijkstra et al., 2011).

Overall, works on message framing can be divided into two dominant areas of research, only one of which involves motivational matching. The first of these areas focuses on matching message frames to people's *risk perceptions* towards a behavior (or in the health domain, uses a heuristic that illness detection behaviors [e.g., cancer screening] are perceived as riskier than illness prevention behaviors [e.g., flossing]; e.g., Gallagher et al., 2011; Gallagher & Updegraff, 2012; Meyerowitz & Chaiken, 1987; O'Keefe & Jensen, 2007; Rothman et al., 1993; 2006). This area is *not* concerned with motivational matching, as risk perceptions deal with a person's beliefs about possible outcomes of a behavior, not whether such outcomes are aligned with their motivations. Further, this area assumes that all individuals seek to obtain gains and avoid losses, without considering ways in which motivational systems (e.g., a person's regulatory focus; Cesario et al., 2013; Higgins, 1997) can strengthen or weaken these tendencies.

In contrast, the *second* major area of research in message framing focuses explicitly on how motivational differences influence people's responsiveness to gain- and loss-related information (Cesario et al., 2013; Mann et al., 2004; Rothman et al., 2020). For example, *Regulatory Focus Theory* (Higgins, 1997; 1998) argues that people with a promotion focused orientation attend more to the presence/absence of positive outcomes (gains, non-gains), whereas people with a prevention focus attend more to messages about the presence/absence of negative outcomes (losses, non-losses; Cesario et al., 2004; 2013). *Reinforcement Sensitivity Theory* (Corr, 2004; Gray, 1990) similarly distinguishes between a behavioral activation system, which sensitizes people towards cues for rewards and non-punishments (gains, non-losses), and a behavioral inhibition system, which alerts people towards cues of punishment and non-reward (i.e., losses, non-gains; Gerend & Shepherd, 2007; Mann et al., 2004; Updegraff et al., 2015).

Although researchers have explicitly emphasized the importance of these two areas (e.g., Covey, 2014; Rothman et al., 2020), meta-analyses of message framing (e.g., Gallagher & Updegraff, 2012; O'Keefe & Jensen, 2007) have mostly focused on the first (i.e., the risk-perceptions approach). Given the size of the literature that has used the second approach (i.e., matching to motivational orientations), a meta-analysis on motivational matching should explicitly incorporate this stream of research to be complete.

Message Tailoring

Message tailoring, also referred to as "personalized matching", focuses on matching messages to characteristics of the *individuals* receiving messages (i.e., who they are, how they

think and act), often relying on questionnaire-based assessments to gather individualized data prior to interventions (Kreuter & Skinner, 2000; Kreuter et al., 1999b; Noar et al., 2007; 2011; Teeny et al., 2020). Much as is the case with message framing research, several streams of tailoring research have emerged over time. Some studies of tailoring focus on matching messages to demographic information about a message recipient–e.g., assessing their age or gender ("demographic tailoring"; Christy et al., 2022; Noar et al., 2007). Other studies of tailoring match messages to constructs derived from models of health behavior (e.g., the Reasoned Action Approach, the Transtheoretical Model, the Health Belief Model; Noar et al., 2007; Prochaska et al., 2013; Sohl & Moyer, 2007), usually focusing on matching to belief-based constructs (e.g., risk perceptions, self-efficacy, perceived barriers) or a person's intentions and past behaviors (e.g., by giving behavioral feedback). In these studies, the prevailing assumption is that people share common motivations (e.g., to avoid risks, achieve health, connect with those similar to oneself) and, thus, the manner in which matching is implemented does not depend on differences in motivations, but instead on correcting deficits in beliefs and engagement. For example, when people hold weak beliefs that sunscreen use is important for their health, this approach suggests addressing these beliefs using health-benefit appeals (e.g., de Vries et al., 2012), overlooking the possibility that some individuals are less concerned about health and may be more responsive to social or appearance-based appeals (e.g., Hevey et al., 2010).

Nevertheless, there is a stream of research on tailoring that uses ideas of motivational matching to account for such differences. For instance, research on cultural tailoring frequently matches messages to value- and identity-based constructs (e.g., familial values, spirituality, acculturation, identity strength; Huang & Shen, 2016; Webb, 2008; Yzer et al., 2018), and tailoring interventions often seek to be responsive to individuals' stated reasons for wanting to change their behaviors (e.g., wanting to quit smoking for personal vs. social reasons; Curry et al., 1995; Strecher et al., 2008). As with message framing, reviews of the message tailoring literature (e.g., Anderson, 2011; Noar et al., 2007; Krebs et al., 2010; Sohl & Moyer, 2007) have tended to focus on interventions matched to demographic, or belief- and behavior-based variables, and to overlook research that uses motivational matching (with the exception of reviews focused more narrowly on cultural tailoring; e.g., Hornikx and O'Keefe, 2009b; Huang & Shen, 2016). Similarly, our own review on motivational matching would be incomplete if it did not consider the substantial body of motivational matching works from message tailoring research.

Context Matching

The final variation to be considered is *context matching*, also referred to as "context congruity", and involves matching messages to a person's context or situation (Joyal-Desmarais, 2020; Lee et al., 2015). Context matching reflects a conceptual opposite to message tailoring. Whereas tailoring matches messages to characteristics of persons themselves (e.g., ethnic background, personality), context matching focuses instead on matching messages to factors that exist externally to, or independently of, a person (e.g., their environment, properties of objects around them, or features of other people one is interacting with).

Context matching can also be divided between studies that do and do not overlap with motivational matching. For example, messages can be matched to geographic locations (e.g., referring to businesses near a person's location; Hühn et al., 2017; Lee et al., 2015) or the time of day (Müller et al., 2017), neither of which explicitly considers motivational factors. In contrast, substantial literature focuses on how objects are tied to specific motivations (e.g., cereals serve to satisfy hunger; t-shirts serve self-expression), emphasizing that messages can be matched to those motivations (Johar & Sirgy, 1991; Shavitt, 1990). For instance, chocolate products, which are typically consumed for hedonic goals (i.e., enjoyment), are best promoted by emphasizing their enjoyable taste, whereas foods that are associated with health goals (e.g., granola bars, yogurt, soy milk) are best promoted by emphasizing their nutritional content (Cheong & Kim, 2011; Choi & Springston, 2010; Choi et al., 2012a). Overall, research on matching to the motivational concerns elicited by a person's context has been very generative (e.g., Shavitt, 1990; 1992), but there has not been a systematic review of this perspective. Given the size of this research area and its conceptual ties to classic functional theories of attitudes (Johar & Sirgy, 1991; Shavitt, 1990), these studies should be included in a review of motivational matching.

Building a Synthesis of Traditions in Message Matching Research

Perhaps because research centered on motivational matching, message framing, message tailoring, and context matching have each used distinct terminology and different approaches to designing message-based interventions (Joyal-Desmarais, 2020), reviews of empirical research on message matching have tended to follow these demarcations closely. For example, reviews by Krebs et al. (2010), Noar et al. (2007), and Sohl and Moyer (2007) are explicitly concerned with message tailoring; reviews by Carpenter (2012), Lavine and Snyder (2000), and Shavitt (1990) focus on "functional" (i.e., motivational) matching; and reviews by Gallagher and Updegraff

(2012), O'Keefe and Jensen (2006), and Xu and Huang (2020) are focused on message framing. To facilitate the integration of research on motivational matching that spans these literatures, a framework is needed that delineates how studies of motivational matching relate to other forms of message matching. Figure 1 presents a mapping that organizes how the traditions that we have discussed can be construed as providing sub-variations within the technique of motivational matching. To help readers track how motivational matching studies can be categorized according to their use of these technical variations, Table 1, Part B, provides a series of examples.

A starting premise of Figure 1 is that, theoretically, every test of motivational matching can be categorized as relying either on message tailoring or context matching, depending on whether the characteristic being matched to is a property of a person (e.g., their personal values) or a property of their external context (e.g., the object that an advertisement is promoting). The most common motivational matching designs that rely on *message tailoring* include: (a) matching messages to measured individual differences in people's dominant motives–such as Snyder and DeBono's (1985) work on matching to individual dispositions toward self-expression vs. social-adjustment motives–and (b) matching messages to dominant motives inferred by a person's group membership–such as in cultural matching research where a person's nationality or ethnic group is used to infer value-based differences (e.g., Huang & Shen, 2016; Ko & Kim, 2010). In contrast, the most common motivational matching designs that rely on *context matching* include matching messages to: (a) the dominant motive associated with particular objects or behaviors–for instance, using utilitarian, value-expressive, or hedonic appeals for items that serve predominantly utilitarian (e.g., hammers), value-expressive (e.g., a poster), or hedonic (e.g., a cookie) purposes (e.g., Johar & Sirgy, 1991; Shavitt, 1989; 1990)–and (b) priming manipulations designed to increase the momentary salience of a motivational factor such as interdependent values (Gardner et al., 1999) or regulatory focus (Cesario et al., 2013). Priming studies are instances of context matching because messages are matched to experimental *conditions*, rather than to a person's actual motivations following the prime.

Motivational matching studies can also be differentiated based on whether or not message manipulations focus on how messages are framed (i.e., comparisons between gains, non-losses, losses, or non-gains). For example, studies that examine the effect of matching messages to individual differences in regulatory focus (e.g., Cornelis et al., 2012; Joyal-Desmarais et al., 2020) afford tests of message framing when such studies explicitly compare the differential

effects of message frames (e.g., comparing gain frames to loss frames). In contrast, studies that test matching messages to motivational concerns but manipulate a message feature distinct from the presence/absence of gains or losses would be an example of motivational matching but *not* message framing. For example, a researcher might test the effectiveness of matching hedonic versus utilitarian appeals to regulatory focus (Lin & Shen, 2012).

Organizing motivational matching according to Figure 1 also allows us to examine how motivational matching operates at unique intersections of the above variations of message matching. For example, a motivational matching study may use both message framing *and* context matching if it matches message frames (e.g., gains/losses) to experimental primes of regulatory foci (e.g., Avnet et al., 2013; Cesario et al., 2013). Overall, we can use Figure 1 as a guide to formulate the following exploratory research questions:

> **RQ:** Do motivational matching effects vary in magnitude according to: (a) whether they make use of message tailoring or context matching techniques, and (b) whether or not they make use of message framing techniques?

Reconceptualizing Motivational Message Matching as a Continuum

In addition to our exploration of the four techniques of motivational matching, we also propose and test theoretical propositions that have fundamental implications for understanding when motivational matching will produce stronger versus weaker enhancements to persuasion. The first of these calls for reconceptualizing matching effects as existing along a continuum and considering how different types of messages fall along that continuum.

Most motivational matching research adheres to a similar notion of what constitutes a *matched message*: messages that are aligned with the characteristic(s) to which they are matched. However, there is markedly less clarity on what constitutes a message that is *not matched* (Joyal-Desmarais, 2020; Rothman et al., 2020). For instance, some studies compare matched messages to messages that are systematically mismatched (i.e., incongruent with one's values; e.g., Lavine & Snyder, 1996), whereas other studies compare matched messages to generic messages (that use the same form regardless of who receives them; e.g., Alexander et al., 2010). This distinction is important, as one of the main mechanisms thought to underlie message matching is the degree to which messages appeal to people's underlying motivations (Snyder & DeBono, 1985).

If the benefits of matched messages depend on appealing to motivational forces, then their benefits relative to a comparison message should depend on the degree (i.e., strength) to

which the latter is less congruent, or in conflict with, the same motivational forces (Rothman et al., 2020). This pattern is shown in the upper panel of Figure 2, which represents matching as a continuum along which messages vary in degree and direction (congruence vs. conflict with people's motivations). Generally, the more that messages are congruent with motivations that are important to a person (i.e., are *positively* matched in direction), the more persuasive they should be. In contrast, the more that messages conflict with a person's motivations (i.e., are *negatively* matched in direction), the more they should elicit active resistance to persuasion, not unlike psychological reactance effects (Miron & Brehm, 2006).

Through the continuum in Figure 2, it becomes clear that evidence of persuasive benefits for a positively matched message (i.e., finding that it outperforms a comparison message) can be attributed to (a) the facilitating effect of the positively matched message on persuasion, (b) the inhibiting effect of a (negatively matched) comparison message on persuasion, or (c) some combination of the two forces. To date, most published work focuses on the first of these interpretations without considering the latter two, which is problematic for both theoretical and practical reasons. Theoretically, it hinders our understanding of the phenomenon and can lead message matching researchers to focus disproportionately on identifying factors that facilitate persuasion (e.g., feelings of fluency, positive affect), while overlooking inhibitory mechanisms (e.g., reactance, negative affect). Practically, if an effect is driven primarily by avoiding negative matches, then implementing matching may not always be desirable. This implication may be especially true when matching requires substantial resources to assess people's dispositions (e.g., using personality assessments, ambulatory assessments), identify who should receive what message based on those assessments, and develop message delivery tools to ensure that people receive the intended message (and only the intended message; Coppock et al., 2020; Joyal-Desmarais et al., 2020). If the key to intervention success is avoiding negative matching, it may be that using a single generic message could achieve similar persuasive benefits without many of the costs.

Classifying and Ordering Messages Along a Matching Continuum

Given the premise that a message's success depends on the degree to which it is congruent with (positively matched) or in conflict with (negatively matched) the motivations that drive people's choices, the lower panel of Figure 2 depicts how this dynamic maps on to the different types of messages commonly used in the literature: Positively matched messages and

mismatched messages. Mismatched messages are messages that are *not* congruent with people's motivational orientations. For example, if a person is predominantly motivated by altruistic values, messages that use altruistic appeals are positively matched, whereas any messages that do not are mismatched (regardless of whether they use egoistic appeals, appeals to other weakly held values, or fail to make any kind of appeal). Mismatched messages represent the main category of comparison messages used in motivational matching studies, with investigators seldomly making finer distinctions. However, finer distinctions can be made between types of mismatched messages that are expected to have different effects on persuasion (i.e., inhibiting vs. failing to facilitate persuasion). Notably, most mismatched messages can be classified into two categories: non-matched messages and negatively matched messages.

In what follows, we describe positively matched messages in greater detail and define the two types of mismatches. We then describe how additional types of messages–generic messages and weak positive matches–relate to the continuum from Figure 2. These categories will be the experimental and comparison conditions evaluated in our meta-analysis.

Positively Matched Message

Positively matched messages are those that align with people's motivational orientations and are typically referred to by investigators as "matched", "tailored", or "congruent". We describe such messages as *positively* matched instead of just matched, as the act of matching refers to the systematic delivery of a message to certain levels of a characteristic and does not necessitate congruence in direction (e.g., incongruent messages are often systematically matched). The term *positive* indicates the direction in *congruence* that the message is designed to achieve; it should not be taken to indicate that a message necessarily has beneficial effects on persuasion. Whereas motivational matching theory generally holds that positively matched messages are more effective, there are cases in which mismatched messages may outperform positively matched messages (e.g., Fridman et al., 2016; Teeny et al., 2020).

Non-matched Messages

Non-matched messages refer to those designed to have a relatively neutral or inert level of congruence with a motivational orientation. They are neither congruent nor in conflict with a person's values, needs, etc. Because of this neutrality, non-matched messages should, on average, have a relatively inert effect on persuasive success. For example, messages that contain themes orthogonal to the liberal-conservative continuum (i.e., politically neutral themes) may be

equally (non)persuasive for people across the political spectrum, and people without strong political feelings (i.e., moderates that are politically disengaged) may respond similarly to messages emphasizing liberal or conservative values (e.g., Joyal-Desmarais et al., 2022a; Voelkel & Feinberg, 2017).

Negatively Matched Messages

A message is negatively matched if it contains features that are in *direct conflict* with the motivational orientation being targeted. Generally, negative matching can be expected to decrease the effectiveness of persuasive communication. For example, when conservatives or liberals receive messages containing themes opposite to their political views (i.e., conservatives viewing a liberal message, liberals viewing a conservative message), they can become markedly less likely to comply (Joyal-Desmarais et al., 2022a). Overall, negative matches are expected to be the least persuasive messages and the only comparison type that we would expect to commonly "backfire" altogether (e.g., making people less likely to engage in a behavior than when they receive no persuasive message).

Generic Messages

Generic (or "standardized") messages are messages that take the same form regardless of who receives them. Investigators often treat generic messages as equivalent to mismatched messages, but their effects may be quite different. Specifically, generic messages are frequently constructed to appeal to the average person and are *not* designed arbitrarily. For instance, mass media adverts typically reflect the dominant cultural values of the countries in which they are deployed (e.g., ads in the United States commonly appeal to individualistic values, whereas ads in Korea commonly appeal to collectivistic values; Han & Shavitt, 1994). As a result, generic messages can act as positive matches for a substantial segment of the population, especially if a population is highly homogenous. In contrast, when there is high heterogeneity in a population, or when an interventionist is interested in targeting a subgroup (e.g., a cultural minority), the proportion of people for whom generic messages are positive matches can drop considerably. Dynamics such as these can dictate when message matching is more (vs. less) likely to provide noticeable benefits over generic messages.

Mixed appeals are a special case of generic messages that contain elements meant to appeal to different segments of a population (e.g., Gainforth et al., 2012; Lavine & Snyder, 1996). For example, a mixed appeal to cultural orientation might contain both individualistic and

collectivistic elements. Mixed appeals are intriguing because they theoretically contain positively matched content for most people; however, that content can be diluted with mismatched content, which may even include negatively matched elements that attenuate persuasion (Lavine & Snyder, 1996). For example, a collectivistic person may find the inclusion of other-focused appeals in a mixed message convincing, but the concurrent presence of self-focused appeals could make them reluctant to comply. This dynamic makes it difficult to pinpoint where, theoretically, mixed appeals should be placed within Figure 2.

Weak (Positive) Matches

Occasionally, studies attempt to create stronger instances of positive matching and compare them to weaker forms of positive matching. For instance, research has compared messages that are matched to a single characteristic (e.g., promotion focus) to messages matched to multiple characteristics (e.g., considering multiple traits simultaneously; Joyal-Desmarais et al., 2020; Strecher et al., 2008), with the hope that the latter would be more persuasive. We refer to this approach as employing a weak(er) positive match comparison (a message matched to few characteristics) to evaluate the benefits of a strong(er) positive match (messages matched to a larger number of characteristics).

Predictions Based on Comparison Conditions.

Using Figure 2, along with the arguments presented above, we can draw several predictions about the relative effect sizes one should expect to obtain when using different types of comparison messages to evaluate the effect of positively matched messages. First, we hypothesize that:

> **H2**: Motivational matching effects should have the largest effects when positively matched messages are compared to negatively matched messages (over *any* other comparison type).

This prediction reflects the idea that negative matches are the furthest away from positive matches on the continuum in Figure 2 and are expected to actively hinder the persuasion process (rather than fail to augment it). In addition, we hypothesize that:

> **H3**: Motivational matching effects should be larger when positively matched messages are compared to non-matched messages rather than to generic messages or weak positively matched messages.

These are expected as, on average, both generic messages and weak positive matches are thought

to achieve some limited amount of positive matching, whereas this is not the case for non-matches. Although mixed appeals may afford some degree of positive matching, their structure may also afford some degree of negative matching. Thus, they are not specified in H3 as we are unable to predict how they will perform compared to non-matches.

Overall, thinking about message matching along a continuum of effects allows us to make predictions about the relative effectiveness of any two given messages based on their distance along the underlying continuum. However, being able to locate where a message falls along the continuum depends not only on having accurate knowledge of features of messages (e.g., knowing that a message contains liberal themes), but also on having accurate knowledge of the characteristics of those to whom messages are delivered (e.g., being able to tell where along the political spectrum a target recipient of the message lies). We discuss this theme next.

Achieving Certainty in Matching: Does the Efficacy of Message Matching Depend on the Method Used to Determine Who Should Receive What Message?

Finally, we explore the degree of certainty with which the motivational characteristics that messages are matched to (e.g., a person's values) are determined. When we consider interventions designed to match to individual differences, we can distinguish between two common strategies for determining people's motivations. One strategy relies on the *direct measurement* of an individual's characteristics–for instance, prior to a persuasion attempt, an interventionist may ask individuals to complete questionnaires assessing the extent to which they endorse collectivistic versus individualistic values (e.g., Chang, 2009; Joyal-Desmarais et al., 2020). An alternate strategy is to *indirectly infer* characteristics based on a person's group membership, usually along sociodemographic lines–for example, an interventionist might use a person's nationality (e.g., American vs. Japanese) and/or ethnocultural background (e.g., European American vs. Asian Americans) to infer their likely cultural values (e.g., individualistic vs. collectivistic; Aaker, 2000; Uskul & Oyserman, 2010).

We propose that matches that are grounded on direct measurement should generally be more effective in enhancing persuasion than those grounded on indirect inference. Our rationale is that there is substantial variability on most psychological variables within demographic groups and that this variability can hinder accurate allocation of messages (see also Kreuter et al., 1999b, for a similar argument against matching to group-level variables). For instance, although the United States is often ranked as the most individualistic country in the world (Fernandez et

al., 1997; Hofstede, 1980), there is still substantial variation in American values such that many Americans hold more collectivistic than individualistic values (Markus & Connor, 2014; Vandello & Cohen, 1999). If messages are assigned according to group membership (e.g., assuming that all Americans are individualistic and should receive individualistic appeals), then any within-group variation will lead a segment of one's audience to be misclassified (e.g., collectivistic individuals will receive messages mismatched to their values) and it will weaken the effectiveness of message matching.

The logic outlined above is compelling, but at the same time researchers have successfully obtained message matching effects using both directly measured *and* indirectly inferred characteristics (e.g., Aaker & Williams, 1998; DeBono, 1987; Han & Shavitt, 1994; Joyal-Desmarais et al., 2020; Kramer et al., 2007; Orbell et al., 2004; Wang et al., 2000). Consequently, it is unlikely that only one approach is effective, and it instead becomes important to quantify the *difference* in effectiveness between them; how much more effective is direct measurement? If the benefits of direct measurement are small, then indirectly inferring motivations (based on group membership) could often be the preferable strategy as it is likely to be less costly and more efficient. Currently, no meta-analysis has compared the impacts of direct measurement and indirect inference on the efficacy of motivational matching. A few meta-analyses have examined the question in other literatures (i.e., message tailoring not focused on targeting motivational characteristics: Head et al., 2013; Kreuter & Skinner, 2000; Noar et al., 2009a; 2010; Portnoy et al., 2008), but findings have been mixed in direction. Consequently, the current meta-analysis will fill an important gap in the literature by examining this factor.

In addition to this distinction, we will also evaluate a third technique that researchers use to determine motivational characteristics; experimentally *manipulating* the salience or presence of motivational characteristics. Within motivational matching research, this approach often takes one of two forms. The first form involves using psychological primes–i.e., tasks or stimuli intended to elicit or make salient a particular motivational orientation (e.g., promotion focus, collectivistic values)–either prior to a messaging attempt or within messages themselves, whereas the second form involves manipulating the type of object/behavior individuals respond to (e.g., consumer products associated with utilitarian vs. social goals; Cesario et al., 2013; Gardner et al., 1999; Maio & Olson, 1995; Shavitt & Fazio, 1991; Shavitt et al., 1994).

Manipulations provide a degree of control over the characteristic to which messages are

matched. Psychologically, manipulations also attempt to alter state-level characteristics (i.e., a person's motivations in the moment), which may afford a more precise match at the moment a message is received than would matching to trait-level characteristics (i.e., chronic dispositions). Yet, manipulations can also differ in strength and quality, and people's psychological responses to them can vary (e.g., even a high-quality individualism prime may fail to change the dominant orientation of strong collectivists). These factors could reduce the effectiveness of this method.[2]

Given the arguments outlined in this section, we hypothesize that message matching using all three methods will prove to be effective. However, when comparing direct measurement to indirect inference, we hypothesize that:

> **H4**: Motivational matching effects will be larger when messages are matched to motivational characteristics that are directly measured rather than indirectly inferred.

Manipulations could also be expected to outperform indirect inference, as the latter makes strong, and often unrealistic, assumptions that members of a group (e.g., a Nation) are relatively homogenous in their motivations. Consequently:

> **H5**: Motivational matching effects will be larger when messages are matched to motivational characteristics that are experimentally manipulated rather than indirectly inferred.

As for the comparison between manipulations and direct measurement, it is more difficult to make *a priori* predictions. Although with manipulations, we know with certainty what experimental conditions people are in and can capitalize on motivational states at the moment of message reception, there can be low correspondence between experimental conditions and individuals' actual psyche (i.e., manipulations can fail to alter motivations). In contrast, direct measurement affords precise information about individuals' psychological orientations, but measurement can be imperfect, and assessments commonly focus on individuals' chronic tendencies rather than their motives in the moment. Given these factors, we examine the relative performance of these two methods from an exploratory viewpoint.

Additional Operational Factors Impacting the Effectiveness of Motivational Matching.

So far, we have given an overview of research on motivational message matching and

[2]An interventionist could make use of a manipulation check to verify the success of a prime. However, if messages are matched according to knowledge gained from manipulation check assessments, the method reverts to relying on direct measurement (and further becomes an instance of message tailoring rather than context matching).

introduced several propositions about the impact of this technique. In addition to exploring these propositions empirically, our review provides an opportunity to examine a series of operational factors that may influence the effect of motivational message matching. We focus on three categories of such factors. First, does the effectiveness of motivational matching depend on features of the *outcome* variable (e.g., attitude, behavior) being evaluated in studies? For instance, we may consider the attitudinal/behavioral domain (e.g., health, environment, consumer behavior) in which change is sought, whether messages are designed to promote or limit a behavior (e.g., increase exercise vs. reduce smoking), and when outcomes are assessed after a persuasion attempt (e.g., short-term vs. long-term change). Second, do effects vary based on characteristics of the *samples* being targeted by motivational matching? We may consider where samples were drawn from (e.g., what continent), the gender composition of the sample (e.g., percent of sample that is female), and the type of sample used (e.g., student vs. online samples). Third, does effectiveness depend on how the messages are constructed and delivered? We may consider factors such as the number of characteristics messages are matched to (e.g., 1 vs. 10), the length of messages (e.g., short vs. long), the modality through which matching was delivered (e.g., using text- vs. video-based messages), and the number of times participants were exposed to message interventions.

Examining the impact of these operational factors, along with the considerations that we have articulated in our hypotheses, will contribute to researchers' understanding of the when and where of motivational matching and provide interventionists with practical guidance on how to optimize the technique. Accordingly, we now turn to our systematic synthesis and meta-analysis of the empirical literature on motivational message matching.

Method

Research Question

The current systematic review and meta-analysis addresses a research question articulated in terms of a specific Population, Intervention, Comparisons, Outcomes of interest, and Study design (PICOS; Methley et al., 2014; Miller & Forrest, 2001; Richardson et al., 1995). First, the project concerns the human population, rather than a specific demographic. Second, the intervention of interest is defined as a positively matched message condition designed to explicitly align with a motivational characteristic. Third, comparisons of interest include

mismatched message conditions (including non-matched and negatively matched messages), generic message conditions (including mixed appeals), and weakly positively matched message conditions. Fourth, the outcomes of interest are attitudes, behavioral intentions, self-report behavior, and observed behavior. Fifth, only studies making use of experimental designs (i.e., using random allocation to intervention/comparison conditions) have been included.

Identifying Relevant Records, Reports, and Studies

Figure 3 provides an overview of the selection process used to identify records (i.e., titles/abstracts), reports (e.g., journal articles), and studies for the current synthesis.

Search Strategy

Records were identified via an electronic search strategy (using APA *PsycInfo* via Ovid, *MEDLINE* via Ovid, and *Scopus*), followed by backward and forward citation searches (using *Web of Science*), as well as less formal methods to identify additional literature. The *electronic search* was developed in consultation with an information specialist; it used a large set of terms that describe the message matching phenomenon across different traditions of research (e.g., including variants of "message matching", "functional matching", "attitude functions", "framing", "tailored communication", "targeting", "congruency", "personalization", "message fit", "individualization"), along with terms tied to specific forms of message matching (e.g., "gain-frame", "loss-frame", "cultural appeal", "value-expressive congruence"). Before conducting our review, we evaluated our search terms using a set of 60 empirical publications on message matching and found the search to identify 82% of these publications (see Joyal-Desmarais, 2020, for details); given the scope of this area of research, and the lack of standardized terminology across studies, this coverage rate was considered indicative of a good sensitivity-specificity tradeoff. The *backward citation* search made use of 81 key sources reviewing message matching effects (e.g., narrative reviews, systematic reviews, meta-analyses, chapters), and the *forward citation* search used the same 81 sources along with 33 influential and/or foundational reports of empirical studies on message matching. When these strategies were added to the electronic search, our coverage of the 60 empirical publications increased to >95%. Section 2 of the Supplemental Files provides the full electronic search queries for APA *PsycInfo*, *MEDLINE*, and *Scopus*, the coverage of our *Web of Science* search, and lists the sources we used for citation searches. The final search was conducted between December 15-19,

2018 and returned 38,594 records.

Screening

Records were compiled into a single database using *EndNote X7.8*. *EndNote*'s feature for finding and removing duplicate records was applied, leaving 25,414 records to screen. Titles were screened for relevance and to remove remaining duplicates. Titles were deemed relevant if they contained any theme related to message matching, but were excluded if they explicitly identified a report as a review or protocol paper. Abstracts were then screened. This step was more selective and required abstracts to refer to at least one intervention, experiment, or persuasive message, which could feasibly include a message matching paradigm. The screening of titles and abstract retained 2,735 records (covering 4,257 studies) for full-text screening.

Inclusion/Exclusion Criteria

Following the screening of abstracts, coders (KJD, AKS, MKM, JVS) downloaded full texts (reports), screened them, and coded studies. To be eligible, *reports* needed to: (a) describe at least 1 empirical study; (b) not have been retracted; (c) be written in English, and; (d) consist of either published peer-reviewed journal articles, or indexed dissertations/theses (which allowed us to have a formal sampling frame to compare published to unpublished [i.e., gray literature] studies). An implicit requirement was that reports be accessible to coders through the University of Minnesota library subscriptions. Lastly, coders could petition the team to have a report excluded for other reasons. The main reason for petitioned reports was that the quality of the writing made it impossible to conduct reliable coding. Two reports, however, were also excluded for methodological reasons.[3]

Once a report was deemed eligible, each *study* within it was evaluated using the following criteria. Studies needed to: (a) follow an experimental design, involving random allocation to at least two message-based conditions; (b) follow a message matching paradigm[4];

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[3]The two excluded reports were by Matz et al. (2017) and Graham et al. (2012). Both used social media platforms (e.g., *Facebook*, *MySpace*) to obtain over 1 million observations, setting them as numerical outliers within our review (e.g., the average *N* per study was 293 when excluding these reports, but would increase to 40,655 by including them). These exclusions therefore avoided these reports from having a disproportionate impact on our findings. The studies were further excluded for using website views as the unit of analysis (allowing repeat participation; Graham et al., 2012), and having strongly imbalanced allocations to message conditions (i.e., indicating a lack of randomization; Matz et al., 2017).

[4]Message variations had to be such that what was considered a positive match could theoretically vary. Message-based studies were excluded if they evaluated a technique that was thought to increase message persuasiveness under any circumstance (e.g., regardless of individual differences).

(c) contain at least one motivational matching effect (e.g., see Table 1, Part B); (d) contain at least one valid comparison between a positive match condition and an eligible comparison–a mismatch, non-match, negative match, generic message, weak positive match, or mixed appeal message; (e) evaluate at least one of the four outcomes of interest: attitudes, intentions, self-report-behavior, or observed behavior; (f) include experimental message conditions that were manipulated between-person, not within-person; (g) define matching categorically such that there was a clear distinction between positively matched message conditions and the comparison conditions used to evaluate them,[5] and; (h) describe novel findings (i.e., not overlap in data with another reviewed study). When excluding reports and studies, coders were required to identify at least one reason to exclude them. Following these assessments, 553 reports (845 studies) were coded in full.

Final Selection into Meta-Analytic Synthesis

For a study to be included in the meta-analysis, at least one effect size estimate had to be successfully extracted from it. After excluding studies from which no effects could be extracted, the final dataset for the meta-analytic synthesis comprised 5,251 effect size estimates from 702 studies (drawn from 463 reports). A summary table of all the studies included in our analyses is available through our project page (https://osf.io/tfvgq). This table provides descriptive information on extracted effect sizes (number of effects extracted, average magnitude, range), lists what motivational characteristics messages were matched to (e.g., regulatory focus, self-construal), and denotes the behavioral domain of interest.

Coding and Data Extraction

Between January and July of 2019, seven coders participated in training sessions, during which several reports were coded in groups of two to all seven coders. Coders completed coding individually, met to compare codes and resolve discrepancies, and adjusted the codebook as required. Once coders showed high interrater reliability (i.e., > 90% agreement), they transitioned to coding individually. The final team retained after the training phase consisted of four coders.

Coders reviewed reports individually and held weekly meetings to review coding. These

[5]Studies were excluded if they operationalized matching in a continuous fashion, such as by examining the interaction between message conditions and a continuous moderator (e.g., promotion focus). For further details on this criterion, see the distinction between Type I, Type II, and Type III designs by Joyal-Desmarais (2020).

meetings involved discussing decisions for inclusions/exclusions, resolving ambiguities in coding decisions, and scanning coding files for mistakes/omissions. Weekly meetings also served to monitor and reduce potential coder drift/fatigue (i.e., idiosyncratic biases and changes in the ways a coder applies coding over time: e.g., Raffle, 2006; Ratajczyk et al., 2016).

Raters used an online spreadsheet to perform coding, which was equipped with drop-down menus for every close-ended item. Coders were provided with a detailed codebook that described each variable to be coded, listed all response options, and provided tips. Coders were also provided with a coding dictionary that supplied definitions for each element contained in the codebook. The codebook and dictionary are provided in Joyal-Desmarais (2020) and on our project page.

Outcome Variables

Coders extracted effect size estimates for four outcome types. *Attitudes* were defined as the degree to which a person evaluates an object/behavior promoted by a message with favor/disfavor (e.g., as good vs. bad, valuable or not: Eagly & Chaiken, 1993; 2007). This definition explicitly excluded people's evaluations of messages themselves (or of the message source). *Intentions* were defined as a mental state of wanting or planning to act in a given way, and coding was applied to include related concepts such as willingness to engage in a behavior or expected likelihood to engage in a behavior. *Self-report behavior* was defined as participants' own disclosure of having engaged (or not) in a behavior, and *observed behavior* included non-self-report methods such as the use of sensors/actigraphs, observations by study staff, or the use of external records (e.g., hospital records; purchase data). All outcomes were coded such that a positive effect size would reflect a more successful intervention in the positively-matched message condition (e.g., improved attitudes towards a promoted product, or decreased smoking after a smoking-cessation message) relative to the corresponding comparison condition (e.g., mismatched message).

Primary Moderators

Coders classified each message matching effect according to three moderator variables, corresponding to the propositions outlined in the introduction. *First*, they coded whether effects made use of each of the four techniques from Table 1: motivational matching (required for inclusion into the review), message tailoring, message framing, and context matching. Separate assessments were made for each technique, as any given effect could fall into multiple

categories, as exemplified in Table 1 (Part B). *Second*, they coded whether comparison messages were negative matches, non-matches, mismatches (that could not be classified more specifically into negative/non-matches), generic messages, mixed appeal messages (selected over generic messages when appropriate), or weak positive matches. *Third*, they coded how studies determined characteristics for the purpose of matching; that is, whether characteristics were directly measured, indirectly inferred, or manipulated.

Additional Operational Factors

In addition to the moderators noted above, coders extracted several other variables (see codebooks in Joyal-Desmarais, 2020). The current review reports findings for ten such factors, including: (a) the *domain* in which influence was sought (e.g., whether messages promoted health vs. prosocial behaviors); (b) the type of change encouraged, distinguishing attempts to promote (e.g., increase physical activity, improve support for a policy) vs. limit an outcome (e.g., reduce smoking, reduce support for a policy); (c) when outcomes were assessed (e.g., the day of the study, at a 6 months follow-up); (d) the continent from which samples were drawn; (e) the gender composition of the sample; (f) the type of sample recruited (e.g., college/university students vs. online community samples); (g) the number of characteristics messages were matched to (counting both motivational and non-motivational characteristics); (h) the length of messages (e.g., two or fewer sentences was considered short; multiple pages of text was considered long); (i) the modality through which matched messages were delivered (e.g., text only vs. audiovisual), and; (j) the number of times participants could be exposed to messages (i.e., single exposures vs. multiple exposures).

Effects Extraction and Metric Choice

We chose to extract the correlation coefficient *r* as a common metric for effect sizes, as *r* has an intuitive interpretation across study designs (e.g., r^2 as the proportion of variance accounted for by an effect) and has been the normative metric for most message matching meta-analyses (e.g., Carpenter, 2012; Gallagher & Updegraff, 2012; Noar et al., 2007; O'Keefe, & Jensen, 2006). A positive correlation was coded to indicate an advantage of a positively matched message over a comparison condition. To convert effects to *r*, coders used a spreadsheet-based calculator that combined tools developed by others (e.g., DeCoster, 2012; Lakens, 2013) supplemented with established formulae from the literature to convert effect size metrics to *r*

(e.g., Borenstein et al., 2009; Polanin & Snilstveit, 2016).[6] For each study, coders extracted every eligible effect reported in sufficient detail to express in *r*.

Distinguishing Main Effects From Interactions

Given that many studies use factorial designs (e.g., examining the impact of two types of messages given two subgroups), our synthesis distinguished *main effects*–for which a positive *r* implicates a relative advantage of receiving a positively matched message compared to receiving a comparison message–from *interaction effects*–for which a positive *r* implicates a cross-over interaction such that, on average, members of two subgroups (e.g., approach-oriented and avoidance-oriented individuals) respond more favorably when they receive a positively matched message (e.g., gain frame for approach-oriented people, and loss-frame for avoidance-oriented individuals), than when they receive the alternative message (e.g., a loss frame when approach-oriented and a gain frame when avoidance-oriented). Interactions were only extracted for 2×2 comparisons.

Interrater Reliability

To evaluate interrater reliability, a random subset of 30 reports (covering 52 studies and 395 effect sizes) was coded by pairs of coders. Using these reports, we adopted an approach similar to the master coder approach described by Syed and Nelson (2015), whereby each article was coded by KJD (the "master coder", who coded the majority of reports in the final dataset), and by one of the three other coders (AKS, MKM, JVS). Interrater reliability was evaluated using percentage agreement between coders for categorical variables (e.g., type of comparison, behavioral domain). For continuous variables (e.g., effect size estimates, sample sizes), interrater reliability was evaluated using percentage agreement, Pearson correlation coefficients (*r*), and the intraclass correlation (*ICC*, form 3,1; Shrout & Fleiss, 1979). Good reliability was established as: a percent agreement of at least 80%; *r* of at least .80, and an *ICC* of at least .80 (Belur et al., 2018; Neuendorf, 2002; Syed & Nelson, 2015). An in-depth report of our interrater reliability analyses is provided in Joyal-Desmarais (2020), which includes analyses by coder and for each variable. Average percent agreement was 95.3% across variables, and the *r*s and *ICC*s for continuous variables were always above .80 (average: $r = .98$, $ICC = .97$).

[6] A small number of studies reported dichotomous outcomes with one or more zero-count cells. In such cases, we applied the modified Haldane-Anscombe zero-cell correction before calculating any effect size (Weber et al., 2020).

Meta-Analytic Statistical Procedure

Study effect sizes were aggregated using *three-level meta-analytic* models (multilevel meta-analyses; Konstantopoulos, 2011; Van den Noortgate et al., 2013; 2015; Van den Noortgate & Onghena, 2003). This approach allows analyses to explicitly model dependencies between extracted effect size estimates, which are very common in message matching studies. For example, studies commonly use multiple outcome measures (e.g., multiple indices of intentions; Detweiler et al., 1999; Kwon et al., 2016), multiple intervention or control groups (e.g., two positively matched groups each compared to a single generic message group; Alexander et al., 2010), the presence of multiple time points (e.g., looking at immediate and long-term outcomes; Lavine & Snyder, 1996), and the presence of multiple subgroups (e.g., breaking results down by subpopulations; Detweiler et al., 1999). For our analyses, effect sizes were nested within studies.

Our approach contrasts with previously reported meta-analytic reviews of message matching (e.g., Gallagher & Updegraff, 2012; Huang & Shen, 2016; O'Keefe & Jensen, 2006), which have relied on traditional univariate models (e.g., univariate random or fixed effects meta-analyses). Univariate approaches assume independence between observations; consequently, meta-analysts have engaged in strategies such as aggregating effect sizes (e.g., calculating mean effects per study), excluding effect sizes (e.g., picking one effect per study), or subgrouping effect sizes (e.g., classifying effects by measure type and extracting only one effect per measure type; Tipton et al., 2019b). These strategies allow observations to be independent, but incur substantial loss of information and underperform relative to newer meta-analytic techniques that model dependent effects (e.g., Moeyaert et al., 2017; Tipton et al., 2019a; 2019b).

Analyses were conducted using the *metafor* (version 3.0.2; Viechtbauer, 2010) and *dmetar* (version 0.0.9; Harrer et al., 2019) packages in *R* (version 4.1.2; R Core Team, 2020). Consistent with meta-analytic guidelines, models used restricted maximum likelihood estimation with the Knapp-Hartung adjustment (Harrer et al., 2021; Knapp & Hartung, 2003; Tipton et al., 2019a; 2019b). Prior to modeling, r was transformed to Fisher's z, and findings were converted back to r for presentation (Borenstein et al., 2009; Harrer et al., 2021). For each estimate, a 95% confidence interval was computed. Importantly, to ensure reliable estimation, we only computed models when *at least four studies* were available to aggregate meta-analytically (following recommendations by Fu et al., 2011).

Moderation

We used meta-regressions to formally evaluate our moderation hypotheses, supplemented by subgroup analyses. As with our main analyses, levels of a moderator were only evaluated if at least four studies could be aggregated within that level. When examining additional operational factors (e.g., sample type, message length), we only present subgroup analyses (as several factors had non-mutually exclusive levels).

Heterogeneity

To assess heterogeneity in effect sizes, we computed three indices. *First*, we computed 95% prediction intervals (Borenstein et al., 2017; IntHout et al., 2016). Prediction intervals represent the range of effects one would expect to find in a population of effects and inform us about the likely range in which any given future effect size (e.g., from a new upcoming experiment or intervention) may be expected to fall–this index considers both within-study and between-study heterogeneity. Prediction intervals have a very practical interpretation and are expressed in the same unit as our primary findings (the correlation coefficient *r*); consequently, most of our discussion will center on this index.

Second, we report the I^2 index (Borenstein et al., 2017; Higgins & Thompson 2002). This index estimates the percentage of variability in the observed effect sizes, which is not caused by sampling error. A value of 0 provides evidence that most of the observed variance in effect sizes may be due to sampling error, whereas a value of 1 provides evidence that most of the variance would remain even if we controlled for sampling error (Borenstein et al., 2017). In our three-level meta-analytic model, I^2 is provided at two levels: Level 2 I^2 reflects *within*-cluster (i.e., within-study) heterogeneity, and level 3 I^2 reflects *between*-cluster (i.e., between-study) heterogeneity (level 1 I^2 is the sum of levels 2 and 3). Although there exists normative suggestions for interpreting heterogeneity (25% = low; 50% = moderate; 75% = substantial: Higgins et al. 2003), it is important to keep in mind that I^2 is a *relative* index and does not indicate how much effects vary in an absolute sense (Borenstein et al., 2017).

Third, we provide σ as an estimate of τ, the standard deviation of true effect sizes (Borenstein et al., 2017; Harrer et al., 2021). As with I^2, in a three-level meta-analytic model, the total variance in effect sizes (i.e., level 1 σ^2) can be broken down to provide two components. Level 2 σ reflects the *within*-cluster (i.e., within-study) standard deviation of effect sizes, whereas level 3 σ reflects the *between*-cluster (i.e., within-study) standard deviation of effect sizes. Level 3 σ can be interpreted similarly to estimates of τ produced in random-effects models

(see Linden & Hönekopp, 2021, for normative examples in psychology). The index σ is expressed in the unit used during meta-analytic pooling–in our work, this is Fisher's z.

Examination of Bias

We use several strategies to examine how bias may influence our results. First, we use sensitivity analyses to evaluate the moderating influence of variables suspected to bias results. For this purpose, our codebook included a version of the Cochrane Collaboration's *Tool for Assessing Risk of Bias* (Higgins & Green, 2011), coding studies as having low, high, or unclear levels of risk for five types of biases: *selection bias* (bias in participant allocation to study conditions); *performance bias* (bias in intervention delivery; e.g., through failure to blind/mask participants); *detection bias* (bias in outcome assessment; e.g., failure to blind study staff assessing outcomes); *attrition bias* (differences in who withdraws from a study), and; *reporting bias* (e.g. selective reporting of outcomes by researchers). Of these, detection bias showed little variance in coding[7] and was excluded from sensitivity analyses.

In addition to Cochrane Risk of Bias variables, we evaluate factors such as whether authors made their *messages fully available* (i.e., their intervention materials), whether the effects extracted relied on analyses that *used covariates*,[8] whether experimental *manipulations were confounded* (e.g., message conditions differed importantly in length, not just content), the *percent of effects that were extracted* within a study (relative to the number of theoretically extractable effects if coders had complete access to data), *sample size* (both the sample size used to extract each effect and the overall sample size of studies), and the *publication status* of reports (comparing peer-reviewed journal articles to studies reported only as dissertation/theses).

Our inferences on the influence of bias rely predominantly on sensitivity analyses. That said, we also make use of funnel plots–with a focus on presenting contour-enhanced funnel plots (Palmer et al., 2008; Peters et al., 2008). Funnel plots present effect sizes according to their magnitude and standard errors (inversely related to sample size), and asymmetry in the distribution of effect sizes is taken as evidence of publication bias (Begg & Mazumbar, 1994; Egger et al., 1997). For example, if a disproportionate number of small studies have larger than typical effect sizes, this pattern can arise from publication/reporting biases favoring the

[7]Coders picked the same response option 99.2% of the time.
[8]Given that every study was experimental in nature, the use of covariates was not deemed necessary to obtain unbiased results and was treated as a degree of freedom researchers could use to alter the significance of their findings (Simmons et al., 2011).

publication of significant positive effects (though it can also arise from other non-bias sources; Terrin et al., 2005). *Contour-enhanced funnel plots* are centered around zero and use colored regions to indicate the degree to which effect sizes differ statistically from zero: that is, whether they are non-significant, or have significance values of $p = .10$ to .05, of $p = .05$ to .01, or of $p < .01$. The rationale behind contour-enhanced funnel plots is that many biases favoring significant effects (e.g., questionable research practices; John et al., 2012; Simmons et al., 2011) should produce a disproportionate number of results just below conventional levels of significance. For the current research, funnel plots are used descriptively as they have not been fully adapted for use with dependent effect size estimates (Fernández-Castilla et al., 2021; Rodgers & Pustejovsky, 2020); a more formal test of publication bias can instead be gleaned from the sensitivity analyses examining the publication status of reports.

Evaluating Certainty of Evidence for Our Primary Findings

To maximize the utility of our results, we provide ratings of certainty of evidence for our overall findings (broken down by our four outcomes), using the *Grading of Recommendations Assessment, Development and Evaluations* (GRADE; Guyatt et al., 2008; 2011a; 2011b) system. Generally, *certainty of evidence* refers to "the certainty that a true effect lies on one side of a specified threshold or within a chosen range" (Hultcrantz et al., 2017), and GRADE allows reviews to produce reliable, reproducible, and transparent ratings (Mustafa et al., 2013). It is accomplished by systematically accounting for factors such as the design of synthesized studies (e.g., experiments vs observational), study limitations (risk of bias), inconsistency, indirectness, imprecision, and publication bias (Balshem et al., 2011; Guyatt et al., 2008; 2011a; 2011b). Certainty ratings range across four levels: high, moderate, low, and very low.

We provide ratings of certainty for our primary hypothesis (H1) that the average effect of motivational matching is small-to-moderate in magnitude (between r of .10 and .30). We also rate the certainty of evidence for whether a future motivational matching study or intervention may be expected to have a positive effect (i.e., $r > .00$). Although the latter idea was not formally captured by our hypotheses, it is of crucial importance to help shape expectations for future users of motivational matching and accounts more heavily for effect size heterogeneity (e.g., weighing prediction intervals more than confidence intervals). The specific criteria and decision rules applied to obtain ratings of certainty are described in Section 7 of the Supplemental Files.

Transparency and Openness

A protocol for the synthesis was preregistered using the *Open-Science Framework* (OSF; Joyal-Desmarais et al., 2018) and the *International Prospective Register of Systematic Reviews* (PROSPERO; Joyal-Desmarais et al., 2019).[9] The current report adheres to the checklist of *Preferred Reporting Items for Systematic Reviews and Meta-Analyses* (PRISMA; see annotated checklist in the Supplemental Files; Page et al., 2021a; 2021b). To complement the current report, a project page has been constructed (at: https://osf.io/6f24t/) that links to our preregistration and contains: (a) a copy of our Supplemental Files, (b) lists of all records screened, (c) a list of all studies and possible matching effects excluded from our meta-analysis, with individual reasons for exclusions, (d) a dataset to reproduce our primary findings, (e) analytic code in *R* to reproduce our primary findings, and (f) copies of the codebook and dictionary files coders used to extract data. Additional details on this project can also be found in Joyal-Desmarais (2020).

Results

From this point forward, we use the term *effect size* to refer to individual effect sizes extracted from studies. In contrast, we use *estimate* (or meta-analytic estimate) to refer to estimates produced by three-level meta-analytic models (pooling effect sizes across studies).

Descriptive Summaries

Before conducting meta-analytic models, descriptive statistics were examined to offer an understanding of the demographic profile of the motivational matching literature.

The upper left quadrant of Figure 4 shows the number of studies using samples from different continents. Overall, samples were drawn from 37 countries, spanning five continents. The majority recruited samples from North America (430 studies), but there were also many samples drawn from Asia (135 studies) and Europe (97 studies). Fewer studies recruited from Oceania (18 studies), and only one from South America.

The upper right panel of Figure 4 shows the distribution of studies according to the percentage of their samples that was female. Overall, studies recruited a slightly larger

[9]The current report deviates slightly from the preregistered protocol to simplify our presentation of findings. Analyses that correspond more closely to the preregistered plan are documented in Joyal-Desmarais (2020). All inferences in the current text are similar to those in Joyal-Desmarais (2020).

proportion of female participants (on average, samples were 58% female).[10]

The lower left panel of Figure 4 provides information on the sampling frames used by studies. Overall, 447 studies (64%) used samples of college and/or university students. When studies relied on non-student samples, the most common strategy was to recruit adult community members using online means (e.g., online panels).

Lastly, the lower right panel of Figure 4 shows the distribution of studies according to their year of publication. Figure 4 documents an initial interest in experimental motivational matching research starting in the mid-1980s, followed by a slow increase in interest until the mid-2000s, after which there was a notable surge of experimental studies being published.

Distribution of Effect Sizes

Figure 5 presents a distribution of the 5,251 effect sizes included in the current synthesis. This analysis is provided for descriptive purposes, as it ignores dependencies between effect sizes and the size of the samples used to derive them. Overall, effect sizes are distributed following a fairly normal distribution (when expressed as Fisher's z) centered around a mean of $r = .18$. However, there is a large spread in effects, such that it is relatively common to see effects that are substantially stronger than average (e.g., $r > .40$), as well as negative effects (i.e., $r < .00$).

Primary Findings: Overall Effects of Motivational Message Matching

Meta-analytic estimates of the overall impact of motivational matching are presented in Table 2 and Figure 6. Each presents meta-analytic estimates (using r as a metric), broken down by *outcome type*–attitudes, intentions, self-report behavior, and observed behavior–and *effect type* (main effects vs. interaction effects). A total estimate is also provided that aggregates across outcome type and effect type. Table 2 and Figure 6 provide 95% confidence intervals around each estimate, the number of effect sizes being aggregated (ES), the number of separate studies effect sizes were extracted from (k), the cumulative sample size across the aggregated studies (N), and the significance level for each meta-analytic estimate. Finally, Table 2 also presents 95% prediction intervals, I^2 values, and σ (estimate of τ) values as indices of heterogeneity.

The results of these analyses are very clear; meta-analytic estimates are significant for

[10]Only 77% of studies reported gender distributions for participants. Very few studies reported/considered gender categories beyond male and female.

every combination of outcome type and effect type. Estimates range in size from $r = .08$ to $r = .24$, and center around $r = .20$. Nonetheless, a substantial amount of heterogeneity exists in the distribution of effects underlying the estimates such that most 95% prediction intervals extend well into the negative range (but also upwards to large positive effects). When examining main effects, most of the heterogeneity occurs within studies, whereas most of the heterogeneity for interaction effects occurs between studies. This distinction, however, may be an artifact of fewer interaction effects typically being extracted from any given study than main effects.

Evaluation of Bias

To examine the influence of bias in our results, a series of analyses were conducted on the primary findings reported in Table 2.

Sensitivity Analyses (Risk of Bias). The Supplemental Files contain extensive details on the sensitivity analyses we conducted (see Section 3). Here, we provide a high-level summary. Overall, sensitivity analyses were performed on 11 variables; Table 3 provides a short description of each variable and summarizes the results of our analyses. Overall, no strong evidence of bias emerged for any variable, and the direction of significant tests is mixed; three variables show some/limited evidence of upwards bias (e.g., smaller samples are associated with larger effect sizes, although the moderation is near null in magnitude), whereas two other variables show some/limited evidence of downwards bias (e.g., incomplete reporting of findings is associated with smaller effect sizes). Four variables have significant tests in mixed directions, and the remaining two variables show no significant tests. All-in-all, 32 of 152 tests are significant, 18 suggest upwards bias, and 14 suggest downwards bias. From these results, it is unclear whether effect sizes in the literature tend to be biased upwards or downwards, and patterns could have emerged largely due to chance. That said, there is strikingly high consistency in effects being positive in direction regardless of the impact of these variables. Across levels of risk of bias variables (low, unclear, or high risk), every total estimate (aggregating across outcomes/effect types) is significant between $r = .09$ and $r = .22$. When looking at specific outcomes/effect types, the pattern is similar: > 97% of estimates are positive, > 80% are above $r = .10$, and >85% are significant.

Funnel plot analyses. To complement sensitivity analyses, we generated funnel plots for each outcome/effect type estimate in Table 2. Figure 7 presents a representative sample of the contour-enhanced funnel plots we generated, and the full results are presented in the

Supplemental Files (also providing standard funnel plots; see Section 4). Each panel in Figure 7 presents effect sizes according to their magnitude on the X-axis, and their corresponding standard errors on the Y-axis. The top two panels are representative of the main effects we analyzed, whereas the interaction effects tend to follow a pattern more like the lower panel. Across funnel plots, there is no strong evidence for asymmetry in the distribution of effects. The shaded areas of the contour-enhanced plots also fail to show obvious evidence of bias for main effects; though, for interactions, a large portion of effects are located just below conventional significance levels (in the dark gray region of the plots), indicating possible upwards bias. It is important to note that for factorial studies (e.g., 2×2 designs), authors primarily operationalize support for hypotheses through interaction effects over pairwise comparisons (i.e., main effects). Doing so may lead to an unevenness in bias, such that there is greater pressure for research to produce significant interactions than main effects. Of note, though, meta-analytic estimates from both types of effects are in line with the range outlined by H1.

Taking the results of the funnel plot analyses together with our sensitivity analyses (comparing effects from published articles to effects from dissertations/theses, for which we failed to detect overall moderation; Table 3), we find little evidence that publication bias substantially impacted our inferences.

Rating the Certainty of Evidence for Our Primary Findings

Table 4 provides a summary of the evidence for our primary findings: evaluating the overall benefits of motivational matching on our four outcomes of interest. The table reiterates our PICOS and the effect sizes we observed–expressed as *r*, but also as Cohen's *d* and as odds ratios (ORs) to help readers interpret effect sizes. The table further provides ratings of certainty of evidence according to GRADE (Guyatt et al., 2008). Table 4 provides certainty of evidence ratings for evaluating H1 (that the average effect of motivational matching is between *r* of .10 and .30), and for evaluating whether a given future motivational matching study or intervention could be expected to have a positive effect (i.e., $r > .00$). The Supplemental Files (Section 7) provide how ratings were derived, along with evidence profile tables.

Generally, evaluations of H1 can be rated as having high certainty; we can have high confidence that the average effect size for motivational matching lies within the range of $r = .10$ to $r = .30$. There is less certainty for the self-report behavior outcome, for which it may be more appropriate to assign moderate certainty (the average effect is likely to be close or within the

hypothesized range, but there is potential for it to be substantially different).

For evaluations of whether a given future study or intervention is likely to obtain a positive effect size ($r > .00$), we can assign moderate certainty. In other words, we have moderate confidence that a future motivational matching study/intervention would produce an effect that is positive in direction; however, at this level of confidence, there is also potential for it to be negative in direction or approximately zero. This rating reflects the fact that although effects are on average positive, there is substantial heterogeneity (substantial σ, and prediction intervals extending below zero).

Evaluating the Moderating Impact of our Three Theoretical Propositions

To examine the three theoretical propositions that we presented in our introduction, we used subgroup analyses and meta-regression. We provide summaries of our findings in text, with a dominant focus on meta-regression models that compared estimates aggregating across outcome and effect types (i.e., akin to the total estimate in Figure 6 and Table 2). Figure 8 summarizes these analyses and displays (for each level of the moderator variables): the *number of effect sizes* (ES) used to derive estimates; the *number of studies* (*k*) from which effects sizes were drawn; the *cumulative sample size* (*N*) across those studies; the *meta-analytic estimates* expressed in *r* (calculated in our meta-regression models), and; their corresponding *95% confidence interval*. The final column in Figure 8 uses letters to denote which levels of the moderator variables significantly differ from each other; for example, the letter "a" denotes that an estimate significantly differs from the first level, the letter "b" denotes a significant difference from the second level, and so forth. Because moderation tests rely on synthesis-related evidence (i.e., on observational comparisons across studies rather than experimental manipulations within them; Cooper, 2009), we treat these findings as preliminary evidence for the propositions and do not provide in-depth ratings of certainty (certainty may be considered low pending sufficient experimental confirmation).

Section 5 of the Supplemental Files provides more detailed tables of our moderation analyses along with subgroup/meta-regression results for all estimates in Table 2 (i.e., allowing interested readers to examine moderation separately by outcome/effect type).

Proposition 1. Examining How Motivational Matching Effects Vary in Magnitude by Matching Technique/Tradition

The top rows of Figure 8 summarize how total meta-analytic estimates (aggregated across all outcome/effect types) vary according to whether motivational matching effects used each of the technical variations featured in Figure 1. Specifically, we computed estimates for (a) message tailoring effects that did *not* use message framing (r = .17), (b) message tailoring effects that used message framing (r = .15), (c) context matching effects that did *not* use message framing (r = .22), and (d) context matching effects that used message framing (r = .22). All estimates are significant and positive.

The distinction between the four motivational matching variations is a significant moderator: (F[3, 5,156] = 5.431; p <.001), with the two estimates using context matching being significantly larger in magnitude than each estimate using message tailoring. In contrast, estimates for studies that used message framing were not meaningfully different from those that did not use message framing.

We can also organize the above findings into two sets of planned contrasts to directly evaluate our research question (RQ). First, we can compare all studies that used message framing (r = .21) to all studies that did not use message framing (r = .20); these two estimates are not significantly different. Second, we can compare all studies that used message tailoring (r = .17) to all studies that used context matching (r = .22); this difference is significant (see full analysis in the Supplemental Files).

Proposition 2. Evaluating the Continuum of Matching Effects by Examining how Motivational Matching Estimates Vary According to Comparison Group Used

The middle rows of Figure 8 summarize how total meta-analytic estimates vary according to the type of comparison group that was used to evaluate the effectiveness of positively matched messages. Comparison messages include: (a) *negatively matched* messages (total r = .30), (b) *non-matched* messages (total r = .19), (c) *mismatched* messages–which represent messages that could not be further classified as non-matched or negatively matched (total r = .28); (d) *generic* messages (total r = .17); (e) *weak positively matched* messages (total r = .16), and (f) *mixed appeal* messages (total r = .17). All estimates are significant and positive.

The distinction between the six types of comparison conditions is a significant moderator: (F[5, 5,237] = 5.096. p <.001). In line with hypothesis H2, the largest estimate was obtained when negatively matched messages were used as comparison messages. The total estimate under this condition is significantly more positive than when comparisons are non-matched messages,

generic messages, weak positively matched messages, or mixed messages; it is also larger than when comparisons are mismatched messages (but this comparison is not significant). For Hypothesis 3, the estimate for when non-matched messages are the comparison is also larger than when generic, weak positively matched, or mixed appeal messages are used, but these comparisons are not significant. The estimate for using mismatched messages as a comparison is significantly larger than that of using non-matched messages, generic messages, weak positive matches, and mixed appeals. Other comparisons are not significant.

Proposition 3. Evaluating Whether Using More Specific Methods to Determine Characteristic(s) Used for Matching Leads to Stronger Effects

The bottom rows of Figure 8 summarize how total meta-analytic estimates vary according to the method studies used to determine the level of the characteristic being matched to. Estimates were computed for when studies: (a) *directly measured* the value of characteristics (total $r = .17$); (b) *indirectly inferred* the value of characteristics through a proxy variable (total $r = .14$), or; (c) *manipulated* the value of characteristics (total $r = .22$). All estimates are significant and positive.

The distinction between the three methods of determining characteristics is a significant moderator: ($F[2, 5{,}057] = 9.950$. $p < .001$). When evaluating hypothesis H4, the total estimate for studies that directly measured characteristics is larger than for studies that indirectly inferred characteristics (as predicted), but this difference is not significant. In line with hypothesis H5, the total estimate for studies that manipulated characteristics is significantly larger than the estimate for studies that indirectly inferred characteristics. The total estimate for studies that manipulated characteristics is also significantly larger than the estimate for studies that directly measured characteristics.

Additional Operational Factors: Examining the Breadth of Conditions Under Which Motivational Message Matching Effects can be Obtained

In addition to the moderators covered above, we explore how 10 operational factors influence the degree to which motivational matching is effective. These factors explore characteristics of: (a) the desired outcome targeted by messages; (b) the samples being recruited/influenced, and; (c) the message-based interventions. Figure 9 summarizes results from subgroup analyses, with a focus on presenting total estimates (aggregating across outcomes and

effect types). Findings delineated by outcome and effect types are presented in Section 6 of the Supplemental Files.

Characteristics of the Outcome

The first three factors in Figure 9 relate to the outcome that messages sought to change. *First*, we evaluate the domain in which influence was sought: whether messages sought to change health- (e.g., smoking, physical activity), environmental- (e.g., recycling, sustainable consumption), prosocial- (e.g., volunteerism, philanthropy), political- (e.g., supporting a candidate/policy), or consumer-related (e.g., tourism, brand purchases) outcomes. *Second*, we evaluate the type of change sought: whether messages sought to promote (e.g., increase fruit/vegetable consumption; create positive attitudes) versus limit (e.g., reduce smoking, create negative attitudes) an outcome. *Third*, we examine when study outcomes were assessed to understand the extent to which motivational matching can be used to produce short- versus long-term change. Meta-analytic estimates are significant and positive across all levels of the moderators, implying a wide range of effective applicability for the technique. That said, it appears message matching has shown greater success in some domains (e.g., consumer) over others (e.g., health), when promoting (vs. limiting) outcomes, and in the short term.

Characteristics of the Sample

The next three factors relate to the samples recruited in studies. *First*, we examine the continent from which samples were drawn. *Second*, the percentage of the sample that was reported as female. *Third*, samples were classified into six types: whether they consisted of college/university students, online community members (e.g., MTurk workers), offline community members (recruited in physical communities), individuals at risk for a health condition (but not yet diagnosed), patients diagnosed with a health condition, or children/adolescents. Although the effects are smaller for certain sample types–most notably for at-risk and patient populations (for whom estimates dip below $r = .10$)–all estimates are again significant and positive.

Characteristics of the Message-Based Interventions

The final four factors relate to how message matching interventions were designed and delivered. *First*, we examine the number of characteristics to which messages were matched, counting matching to both motivational (i.e., values) and non-motivational characteristics (e.g., health beliefs). *Second*, we examine the effectiveness of using short (e.g., two sentences or less),

medium (e.g., more than two sentences, <300 words), or long (e.g., 300+ words) messages. *Third*, we examine the modality through which matching was accomplished (e.g., did messages match persuasive texts to people's motivations, or did they also employ audiovisual elements?). *Fourth*, we examine the number of times participants were exposed to messages. Estimates are once again all positive and significant, with larger effects being observed for simpler interventions (e.g., short messages; messages presented only once).

Discussion

Having completed our systematic review and meta-analysis of the literature, we now turn to the implications of our findings. First, we revisit our primary goal of determining whether motivational matching improves persuasion and, if it does, by how much. Second, we turn to each of the key propositions that guided our review and examine them in the light of our findings. Third, we delineate implications for research and for practice, and address the strengths and limitations of our synthesis.

Primary Finding: On Average, Motivational Matching Substantially Improves Persuasion

The primary findings from our meta-analysis, which synthesized 5,251 effect sizes from 702 studies on motivational matching, provide clear and consistent evidence that motivational matching can increase the effectiveness of message-based interventions on diverse outcomes (i.e., attitudes, intentions, self-report-behaviors, and observed behavior) by an average of $r = .20$, which is roughly equivalent to a Cohen's d of 0.40, or an odds ratio of 2.08 (DeCoster, 2012). As summarized in Table 4, there is high certainty for H1–that the average effect of motivational matching is small-to-moderate in magnitude, positive in direction, and that this inference is unlikely to change by conducting further research. The effect size of $r = .20$ is larger than effects that have been routinely observed for other forms of message matching which do not use motivational matching (matching messages to health beliefs, risk perceptions, or demographic characteristics, which have tended to be $r < .10$), supporting the conclusion that motivational matching may be one of the more potent forms of message matching.

The average effect size documented in our meta-analysis is also larger than for most documented methods for improving the effectiveness of communication (e.g., mass-media) based interventions (Anker et al., 2016; Head et al., 2013; Keller & Lehmann, 2008; O'Keefe, 2013; Shen et al., 2015; Snyder et al., 2004). Like these strategies, message matching

interventions are often relatively simple and applicable to short messages that can be delivered automatically to large audiences at a time, which can allow messages to have a larger cumulative impact than is often possible with complex interventions delivered to single individuals (or small groups) at a time (Matthay et al., 2021). In evaluating effect sizes, however, we remind readers that the effects synthesized in this review are *relative* increases in persuasion achieved by positively matched messages over *active* comparison messages designed to be persuasive in their own right. If comparison messages are generally effective, the average absolute effect of receiving a positively matched message (i.e., over no message) could be even stronger than implied by the effect sizes we report.[11]

Finally, the average effect of motivational matching across outcomes and subgroup analyses is highly robust. Significant positive estimates can reliably be achieved for a wide breadth of outcomes (e.g., from health- to consumer-related domains, both for promoting and limiting behaviors), for a variety of populations (e.g., from students to patients, across continents), and under a wide range of intervention conditions (e.g., from short to long messages, through text, images, or video). However, although motivational matching is generally effective, the degree of heterogeneity in the effects we observed is an important caveat.

Considerable Heterogeneity in Effect Sizes Exists, Such That not Every Study or Intervention Will Observe Persuasive Benefits from Matching

Substantial heterogeneity was observed in the effect sizes underlying our meta-analytic estimates. From a practical perspective, most estimates in Table 2 have lower bounds for 95% prediction intervals that extend to $r = -.20$ or below. Thus, although motivational matching can be expected to have persuasive benefits *on average* (and we can expect most instances to have benefits), there is a nontrivial chance that *any given* study or intervention will find the technique to be ineffective (i.e., have no appreciable effect) or even counterproductive (i.e., reduce the persuasiveness of a message). Yet, the predicted variability is not one-sided. Most 95% prediction intervals in our synthesis also have higher bounds that extend to $r > .50$, suggesting that just as any given instance of motivational matching can be ineffective, it is similarly possible

[11]The absolute effect of a positively matched message should equal the absolute effect of the comparison modified by (added to) the relative benefit of the positively matched message as it compares to the comparison message. If the comparison message is already persuasive, the absolute effect of the positively matched message will be greater than implied by the relative effect. In contrast, if the comparison message has a detrimental impact on persuasion, the relative effect may instead overestimate the absolute impact of the positively matched message.

for an instance of the technique to be unusually effective.

From a research perspective, this heterogeneity also has implications for powering studies to detect message matching effects. The average effect, $r = .20$, is useful, but, based on our levels of heterogeneity, we may predict only around 60% of motivational matching effects to lie at $r \geq .20$ (~50% of effects) or $\leq -.20$ (~10% of effects). Thus, (two-sided) tests powered to detect effects of $r = .20$ may be underpowered in the roughly 40% of cases remaining. It would therefore be wise for researchers to plan for higher levels of power while assuming smaller effect sizes.

This heterogeneity underscores the importance of understanding moderators of motivational matching to better capitalize on (and improve) the effectiveness of the technique. To this end, our review began by introducing three propositions that should theoretically influence the effectiveness of motivational matching, and our meta-analytic findings sought to provide initial evidence to support them. We discuss each proposition in turn.

Proposition 1. Examining How Motivational Matching Effects Vary in Magnitude Across Message Matching Techniques

The literature on motivational message matching includes many variations on the technique (Figure 1). Research on *message tailoring* focuses on matching messages to person-based characteristics (e.g., someone's personality; cultural background; political beliefs), whereas research on *context matching* focuses on matching messages to the situational factors around people (e.g., what object/behavior they are asked to contemplate, the experimental conditions to which a person is assigned). Research on *message framing* focuses on understanding the differential impact of message frames (e.g., gain vs. loss frames), whereas other works focus on the differential impacts of other message features (e.g., self-focused vs. other-focused appeals). Because our review synthesized research across these areas, it provides an opportunity to examine how motivational matching effects have varied based on their use of these different variations of the technique.

Overall, we found that the benefits of motivational matching are significantly more pronounced when studies make use of context matching ($r = .22$) rather than message tailoring ($r = .17$). Although this difference is relatively small in magnitude, it offers promising implications. One of the major drawbacks of message tailoring has been the resources required to assess

individual differences, and allocate messages to those differences (e.g., Coppock et al., 2020; Joyal-Desmarais et al., 2020). In contrast, context matching studies often by-pass the need to assess individual differences by either priming motivational orientations prior to assigning messages (e.g., asking participants to complete a regulatory focus induction task before seeing a message; Cesario et al., 2013 [Study 4]), or by incorporating primes into messages themselves at the time of delivery (e.g., including themes of safety/growth in a message to alter receptivity to framed elements within the same message; e.g., Bertolotti & Catellani, 2015; Cesario et al., 2013 [Study 2]). Although priming prior to message delivery is commonly implemented as a lab-based procedure (and may not be feasible to implement on a larger scale), the option of incorporating primes into messages holds promise as a potentially cost-effective form of motivational matching. Future work should examine whether the larger effects sizes we observe for context matching hold when implementing this technique outside of lab-based environments.

We did not observe meaningful differences in efficacy between motivational matching interventions that used message framing (r = .21), compared to interventions that used other forms of message manipulations (r = .20). This result suggests that both forms of motivational matching are viable alternatives, which may be particularly good news for users of message framing, as this method is often easy to implement, and has a very wide range of applications. Specifically, whereas many message variations are highly dependent on the domain of application (e.g., emphasizing short- vs. long-term benefits of a behavior requires a behavior to have both types of benefits), message framing can generally be applied to any persuasive attempt that focuses on emphasizing the benefits (or costs) of a decision.

Mapping Out the Full Message Matching Literature

Although our review focused exclusively on motivational matching, many of the ideas we examine have implications for matching more generally. Given that the literature on message matching has largely been fragmented around the techniques we described (motivational matching, message tailoring, context matching, and message framing), we believe that these distinctions can be used as anchors to help researchers better and more systematically map out and understand research on message matching generally. Working from this perspective, we can generate a broader "map" of message matching research, as shown in Figure 10, and use it to understand how average effect sizes have varied in different sections of the figure.

Figure 10 is grounded on the premise that all message matching effects can be understood

as instances of either message tailoring (left side of Figure 10) or context matching (right side of Figure 10), by virtue of matching messages to characteristics that describe either a person or their context. Message framing interventions exist at the intersection of these techniques, depending on whether interventions seek to alter (and match) message frames, or another type of message feature (e.g., self-focused vs. other-focused appeals). These ideas are generalizations of the same principles we applied to motivational matching. From this viewpoint, motivational matching is itself another category at the intersection of the other techniques, representing cases when messages are matched to qualitative differences in motivations (as opposed to non-motivational differences such as health beliefs). Motivational matching covers sections a, b, c, and d of Figure 10, which duplicate Figure 1 (i.e., our mapping of motivational matching research). The sections outside the central area–i.e., sections e, f, g, and h of Figure 10–represent variations of message matching that do *not* make use of motivational matching (e.g., matching messages to risk perception beliefs).

This conceptual map of message matching research is comprehensive and can serve to locate any specific form of matching. For example, research on "moral reframing" (Feinberg & Willer, 2019) is a subset of motivational matching, which focuses on matching morally- and politically-based appeals to individual differences in political leanings (and is thus typically located in section a of Figure 10). "Demographic tailoring" (Christy et al., 2022; Noar et al., 2007) focuses on matching messages to people's demographic profiles (e.g., ensuring images used in a message match a person's race), and is an instance of tailoring that does not use motivational matching or framing (located in section e of Figure 10). Knowing about the techniques a given area of research uses can allow users of matching to not only locate instances of the technique more easily, but can also provide access to insights from areas studying closely-related techniques.

To illustrate the usefulness of this method of mapping the field, we can use Figure 10 to understand which areas of message matching have been subject to systematic meta-analytic investigations and summarize how the success of message matching interventions has varied depending on the techniques used. Prior to the current project, large-scale meta-analyses had explored only two main areas of Figure 10: *First*, large meta-analyses within the message tailoring tradition focused on understanding matching to sociodemographic variables and to factors commonly delineated by health behavior theories (e.g., health beliefs, perceived barriers,

intentions, past behavior; Krebs et al., 2010; Lustria et al., 2013; Noar et al., 2007; Sohl & Moyer, 2007), and generally report average effects of r = .06 to .10 . Although these works include some interventions that use motivational matching (e.g., matching to people's personal reasons to quit smoking; Curry et al., 1995; Strecher et al., 2008), they are generally focused on research that does not use motivational matching. Additionally, the interventions reviewed do not generally make use of message framing. Consequently, the estimates produced may reflect the typical range of effects within area e of Figure 10 (at least within the health domain). *Second*, large meta-analyses of message framing have almost exclusively focused on the idea that different frames should be used depending on whether a health behavior is enacted to detect versus prevent illness (e.g., Gallagher & Updegraff, 2012; O'Keefe & Jensen, 2006), and generally report average effects of r = .03 to .08. Given that frames are matched to the behaviors being advocated (i.e., the decisional context a person is faced with) rather than an attribute of a person, this research is generally an instance of context matching, not message tailoring. The typology of behaviors is a proxy for whether behaviors are generally seen as risky (detection behaviors like cancer screening) vs. safe (prevention behaviors like flossing). Because risk perceptions are a belief-based variable, which conveys little information on whether a person sees such risk as good vs. bad, this area of matching is not considered motivational matching. Consequently, the estimates produced may reflect the typical range of effects within area h of Figure 10 (again, at least within the health domain).

The current meta-analysis provides estimates for four new areas of Figure 10: Areas a (r = .17), b (r = .15), c (r = .22), and d (r = .22; see also Figure 8), leading to two main observations. *First*, it becomes clear that the effects of motivational matching (across areas a, b, c, and d) are consistently larger than effects for non-motivational matching studies that have been subject to meta-analyses to date. *Second*, this analysis reveals two major areas within message matching research that have yet to be adequately reviewed. These include area f, interventions that use message tailoring with message framing (but not motivational matching: e.g., matching message frames to personal risk perceptions; Apanovitch et al., 2003), and area g, interventions that use context matching without framing (but not motivational matching: e.g., matching the receipt of an advert to a person's proximity to the advertised business; Hühn et al., 2017). To help interventionists make maximally informed decisions, it will be essential for future reviews (and empirical studies) to examine these two areas more closely. It will also be

important for reviews of message tailoring and message framing to examine domains other than health.

Proposition 2. Viewing Matching Along a Continuum from Positive to Negative Matching to Understand How Comparison Groups Impact the Apparent Success of Matching

In our introduction, we argued that the relative success of positive matches against any type of comparison message (i.e., negative match, non-match, mismatch, generic message, weak positive match, or mixed appeal) would depend on where comparison messages are situated, on average, on the continuum ranging from positive matching to negative matching (see Figure 2). We hypothesized that matching interventions should produce the largest effects when positively matched messages (i.e., messages congruent with a person's motives) are compared to negative matches (i.e., messages that oppose a person's motives) than to any other type of comparison (H2). In addition, we argued that using non-matched messages as comparisons should lead to stronger effects than either generic messages or weakly matched messages, as these latter two types of comparisons typically lie on the positive side of the continuum in Figure 2 (H3)–weakly matched messages are inherently on the positive side, and generic messages are typically designed to appeal to average members of a larger population.

Our findings (i.e., Figure 8) are supportive of these hypotheses. First, positively matched messages are more effective when compared to negatively matched messages than to *any* other type of comparison messages, with four of five tests of this hypothesis being significant. Second, studies using non-matched comparison messages also have larger effects than those that use generic messages, weakly matched messages, or mixed appeals, though these tests are not statistically significant.

Overall, these findings support the notion that the continuum of matching effects depicted in Figure 2 is a useful tool for researchers to understand the operation of motivational matching (and message matching more broadly). In our introduction, we argued negative matches may exert an overall adverse influence on persuasive success, whereas non-matched messages are relatively inert. This dynamic will be crucial for future research to confirm and explore. To the extent that negative matches (but not non-matches) exert detrimental effects on persuasion, interventionists may need to be particularly attentive to situations when negatively matched messages arise (e.g., in highly politicized domains; for minority groups with values counter to the majority targeted by generic messaging). It will also be crucial to quantify the relative

benefits achieved by positive matching to the potential detrimental effects of negative matching.

Overall, we suggest that researchers should make use of the typology of comparison groups we have outlined and diversify their use of different comparison messages. The use of research designs that include multiple types of comparisons (e.g., the inclusion of both non-matched and negatively matched comparisons within a single experimental study; Joyal-Desmarais et al., 2022a) will allow us to build a strong base of experimental evidence regarding the relative impacts of these messages.

Proposition 3. Determining how the Effectiveness of Matching Depends on the Method Used to Assess Characteristics to Which Messages are Matched

We proposed that message matching interventions should be more effective when they *directly measure* people's motivations in order to guide matching efforts, than when messages are matched based on people's group memberships (i.e., *indirectly inferring* that members of a group share common motivations; hypothesis H4). We further proposed that *manipulating* characteristics (e.g., using primes) should confer an advantage over indirect inferences (hypothesis H5).

The direction of our findings was in line with H4, but the benefit of direct measurement over indirect inference was not significant. In contrast, manipulating characteristics led to significantly stronger estimates than either indirectly inferring (H5) or directly measuring characteristics. The lack of a significant difference between directly measuring and indirectly inferring characteristics is surprising as the proposition is rooted in the simple notion that indirect assessments should be more prone to error, leading to less certain matching (i.e., a higher chance that "matched" messages do not actually succeed in matching a person's motivations). Given that the few works that provide within-study data on this question tend to find benefits of direct over indirect assessment (e.g., Chang, 2006; Neale et al., 2016), it is possible that the lack of a conclusive difference is due to correlated features across studies that confound results (e.g., matching to different characteristics, using different designs). Consequently, further work will be needed to isolate the causal effect of this factor. Even if direct measurement can lead to larger effects over indirect inferences, it is of paramount importance to quantify the size of this advantage before making recommendations as direct assessments are considerably more costly to implement. Likewise, empirical studies should continue to examine the relative benefits of manipulating characteristics over measuring them (directly or through indirect assessments).

When evaluating the importance of different assessment methods, future work should consider the impact of matching to a person's psychological *state* (e.g., contextual or temporary dispositions) as opposed to matching to long-standing *chronic*, or "trait-level", dispositions. Manipulations typically rely on the former (i.e., manipulating which motives are temporarily salient), whereas both direct measurement and indirect inferences often rely on matching to chronic differences. This distinction has important implications for the potency of matching effects. Specifically, the further away a person deviates from their chronic disposition at the time they receive a message matched to their chronic disposition, the lower the actual degree of match achieved at that time. It is possible that the advantage we observe for manipulating characteristics emerges from the fact that matching is produced through an induced state, and that the effect of using direct measurement would become stronger (more like using manipulations,) if interventionists focus on matching to state-level variables (e.g., how motivated people feel to seek a given outcome in the moment preceding a persuasive message).

Priorities for Future Research to Build a Cumulative Research Base

Understanding the Operations of Motivational Matching

Research on motivational matching–and on message matching more broadly–has predominantly focused on evaluating whether the technique is effective or not. In line with this goal, the primary findings of our synthesis provide strong causal evidence of the effectiveness of motivational matching. A key priority for future research will now be to better understand when, where, for whom, how, and why motivational matching operates–goals which our review takes first steps toward elucidating.

We began our review by providing a detailed account of how three propositions can aid us in understanding motivational matching effects. Our analyses used these principles to evaluate *when* message matching would operate with larger (vs. weaker) effect sizes. For example, evidence suggests that motivationally matched messages are particularly impactful when they can capitalize on matching to contextual factors, and when the alternative to a positively matched message is the receipt of a negatively matched message. Our principles and our findings also provide insights to answering where, for whom, how, and why motivational matching works. For example, the key theoretical idea underlying our continuum of message matching effects is that messages are differentially effective to the degree to which they appeal to people's motivational systems. Delving deeper, matching effects should therefore operate through these motivational

systems, which may orient attentional processes (determining what cues people attend to), and lead people to differentially value and engage with certain pieces of information over others. Such processes (focused on the *how* and *why* matching works), among others, have been discussed before in the matching literature (e.g., Dijkstra, 2008; Falk & Scholz, 2018; Lavine & Snyder, 2000; Hawkins et al., 2008; Rimer & Kreuter, 2006; Rothman & Baldwin, 2012; Rothman et al., 2020; Updegraff & Rothman, 2013; Teeny et al., 2020); however, our continuum may help better predict when, for whom, and to what extent, these mechanisms operate. For example, these processes should operate most strongly at both ends of the continuum (e.g., when people with strong dispositions view positively *or* negatively matched messages), and least strongly at the center (i.e., for non-matched messages). Used in this way, our propositions can provide researchers with a generative source of hypotheses to guide future innovations in message matching. Empirically, our moderation findings are also promising, as moderation can be seen as evidence that the processes underlying a phenomenon (e.g., differential attention and valuing) are being changed (i.e., enhanced or disrupted) at different levels of the moderator variable (Rothman & Sheeran, 2021). It may therefore be a fruitful area for future research to elucidate *which* specific processes are being influenced (enhanced/disrupted) at different levels of the moderators described in our review.

The 10 operational factors we examined also provide insights into the conditions that enhance and inhibit the benefits of motivational matching, particularly as these touch on three aspects that characterize all matching interventions: (a) what outcome messages encourage, (b) who the recipients of messages are, and (c) how messages are communicated. Many of these factors have straightforward implications for understanding questions such as when, where, and for whom message matching improves persuasion, but each factor can also be used to generate hypotheses regarding mechanisms of matching. For example, two main findings emerged when examining features of the outcomes encouraged. The first finding is that the benefits of motivational matching declined substantially over time. Applying the logic that moderation often implicates a change in the operation of mechanisms, we may ask how time interacts with mechanisms of matching. For example, if matching improves people's encoding of messages into memory (e.g., Brug et al., 1999), forgetfulness over time may underlie this moderation effect (if so, could we then explore whether reminder messages are enough to counter this decline?).

The second main finding was that motivational matching offers a stronger advantage

when messages seek to exert a promoting, rather than a limiting, influence. Past research, particularly in the health domain, has argued that promoting healthy behaviors often requires more deliberate processes to enact (e.g., planning to buy more fruits and vegetables), whereas limiting unhealthy behaviors may rely on countering automatic processes (e.g., impulses to smoke, cravings for sweets; Richetin et al., 2011; Rothman et al., 2009). If motivational matching is more successful at addressing the former, this could indicate that the technique better leverages deliberate thought processes (e.g., engagement with a message) over automatic processes (e.g., attention). In exploring this type of question, however, it will be important to keep in mind that some of these effects could also reflect confounding. For example, to the extent that limiting behaviors (e.g., eliminating a habit, avoiding unhealthy foods) is generally a challenging task (Kelly & Barker, 2016), this could attenuate researchers' ability to detect matching effects for such behaviors–an explanation which may not implicate a differential role of deliberate versus automatic thought processes. This type of confounding brings us to our next priority for future research.

Teasing Apart Confounding Influences.

In reflecting on the current review, we are mindful that few motivational matching studies have directly (and causally) examined the propositions and operational factors we outline. Consequently, our evaluations of these variables rely on observational comparisons between studies (i.e., synthesis-generated evidence; Cooper, 2009) rather than on experimental evidence within studies. The implication of this is that, although we can draw strong causal claims about the general effectiveness of motivational matching, there are limitations in our ability to establish how motivational matching varies in effectiveness across moderator variables (e.g., the 10 operational factors).

To illustrate this challenge, we can consider how some behaviors, such as smoking, may be particularly difficult to change (e.g., Nayan et al., 2013). This difficulty could reduce researchers' ability to see strong matching effects in such domains (e.g., due to restricted variance). Notably, many of the smaller estimates from our meta-analysis overrepresent smoking-related trials (e.g., Naughton et al., 2017; Stanton et al., 2015). These studies make up large portions of the works we reviewed that sought to limit (vs. promote) outcomes, provided effects at 6 months or further, studied at-risk populations, or assessed self-reported behavior. Smoking-related studies were also likely to use complex interventions (i.e., long messages, with

multiple exposures, matched to multiple characteristics). Further, the operational factors covered in our review show many substantial associations with one another, with certain attributes even nearly perfectly co-occurring (e.g., 97% of effects following a single-exposure message were assessed the day of exposure to the message). This type of imbalance makes interpreting moderation difficult and adjusting for the influence of confounders using a purely analytical approach is unlikely to be sufficient (and can even risk increasing bias in causal estimation: Hernán et al., 2004; Joyal-Desmarais et al., 2022c; Schisterman et al., 2009). Consequently, design-based methods, such as carefully designed experimental studies, are recommended to examine the influence of these operational factors in the future.

The benefit of examining moderators using experiments can be demonstrated by considering the effect of matching messages to multiple characteristics at a time. Many authors suggest that matching messages to multiple characteristics should lead to stronger matching effects (e.g., Joyal-Desmarais et al., 2020; Strecher et al., 2008). Our subgroup analyses (Figure 9) seemingly show otherwise; the larger the number of characteristics messages are matched to, the *smaller* the benefits of matching. Does this finding contradict the belief widely shared in the field? Not necessarily, as this effect is confounded with between-study patterns. For example, interventions that match messages to a single characteristic almost always assess outcomes on the day of the intervention, whereas studies that match messages to 10 or more characteristics nearly always evaluate outcomes after at least one month had passed. Fortunately, our review also contains a more direct, experimentally-derived, test of the benefits of matching to larger (vs. fewer) numbers of characteristics. This test is reported in Figure 8. Specifically, in our review, studies that used weak positive matches as a comparison group were those which experimentally compared messages matched to a larger number of characteristics ("strong" positive matches) to messages matched to a smaller number of characteristics ("weak" positive matches). These studies provide clear evidence that matching to a larger number of characteristics leads to further *increases* in persuasion (average benefit of $r = .16$; as shown by the point estimate in Figure 8).

Unfortunately, studies on message matching contain few direct causal tests of the moderating factors explored in our review. Consequently, it is our hope that this review will encourage investigators to produce such investigations. This, in turn, will allow future meta-analyses to provide better causal estimates for these factors.

Coordinating Efforts to Fill Empirical Gaps of Practical Concern.

When reporting findings about our three propositions and 10 operational factors, we focused our discussion on estimates that aggregated across outcomes (e.g., attitudes, intentions) and effect type (i.e., main effects vs. interactions). However, we also conducted our analyses broken down by outcome and effect type. These specific findings, which are reported in the Supplemental Files (Sections 5, 6), can be reviewed by researchers to identify gaps in the literature, along with areas of redundancy where new studies are no longer required.

To give an example, our review of the effect of motivational matching on attitudes and intentions was almost entirely limited to assessments made the very same day as when interventions were delivered (Table S28). Of the 5,251 effect sizes extracted, only *one* effect/study evaluated the impact of motivational matching on intentions beyond 6 months, only *nine* effects (from two studies) evaluated impacts on attitudes at 1-6 months (post intervention), and *zero* effects/studies evaluated the impact of interventions on attitudes beyond six months. In contrast, when outcomes were evaluated the day messages were delivered (i.e., usually immediately after messages), we extracted 2,194 effects for attitudes, and 2,322 effects for intentions (from 384 and 387 studies, respectively). Considering these numbers, it is likely that conducting just a few new studies on the long-term impacts of motivational matching on attitudes and intentions will move the field considerably forward in generating better long-term estimates. Doing so would be particularly valuable for areas of motivational matching that hold specific interests in influencing these types of outcomes (e.g., areas interested in using motivational matching to reduce stigma and prejudice; Herek, 1987; O'Brien, 2003). In contrast, producing even 100 new evaluations purely on the impact of motivational matching on attitudes or intentions measured on the day of interventions is unlikely to improve our understanding of the technique. Researchers can attend to patterns like this to prioritize study designs that will close gaps within the literature, while deprioritizing efforts that would be largely redundant with the already published literature.

Recommendations for Practice

For practitioners asking themselves whether to use motivational matching, our results showcase motivational matching as a useful and effective technique to improve interventions that seek to alter attitudes, intentions, and behavior across a wide range of contexts. The technique is very versatile and can be used alone (e.g., within a mass communication campaign) or in conjunction with other techniques (e.g., augmenting a counseling intervention with SMS-based

matched messages). That said, implementing matching can involve added costs and complexities. It is therefore important to consider when matching is most likely to produce desirable effects and carefully consider the degree to which the technique varies in effectiveness. The current work informs this decision both theoretically and empirically.

For instance, throughout the current work, we have emphasized the need to understand the target population of a potential intervention. If implementing a message matching procedure is to be worthwhile–a procedure, which involves developing *multiple* messages, along with the means to *differentially allocate* those messages–it is crucial that the population of interest shows meaningful variability on the characteristic(s) messages are being matched to. If most members of a population are highly similar (e.g., 90% of a group is highly individualistic), developing such a complex procedure is unlikely to be worth the effort. Instead, a generic strategy, in which a *single* message is used (one designed to be congruent with the dominant orientation of the population; e.g., an individualistic appeal) could confer similar benefits at a lower cost. In contrast, the more that a population varies in an underlying characteristic, the more likely that an intervention matched to that characteristic will be effective at improving persuasion, and this might be particularly true if a population is highly polarized (e.g., when a population is composed of two or more large groups with conflicting ideologies or sets of values).

When considering the distribution of characteristics within a population, interventionists may also wish to consider the *typical* messages people are likely to receive in the absence of an intervention, and where such messages lie on our proposed continuum from negative matching to positive matching (Figure 2). Given that motivationally matched messages have the greatest benefit in comparison to negatively matched messages (e.g., Figures 2 and 8), the technique is likely to be most effective when applied to contexts where people otherwise have a high chance of encountering negative matching. For example, if messages around a topic are routinely politicized (e.g., climate change messages often emphasize liberal themes/solutions), some groups (e.g., conservatives) may routinely be exposed to negatively matched messages and have the most to gain from being included in a motivational matching intervention (e.g., Dixon et al., 2017; Campbell & Kay, 2014). Similarly, minority groups (e.g., cultural, religious) that hold values, identities, or other motivational characteristics that conflict with those of a majority group may also routinely be exposed to negatively matched messages and could therefore benefit the most from motivationally matched (e.g., culturally appropriate) messaging.

Finally, there are two aspects of our findings that interventionists should be mindful of. First, the evidence base underlying our synthesis primarily involved assessments of short-term outcomes. The degree to which motivationally matched messages lead to sustained persuasive advantages over non-matched messages remains an important question for future research. Second, the motivational matching studies we reviewed were generally effective, but there was consistently a high degree of heterogeneity in their effect sizes, with a nontrivial number even having negative effects. Interventionists should strongly consider preliminary testing of interventions before implementing them on a larger scale. In doing such work, interventionists may consider consulting formal frameworks such as the ORBIT model (Czajkowski et al., 2015) or the MRC guidelines (Craig et al., 2008), which offer guidance on steps interventionists can take prior to implementing an intervention to maximize the likelihood of success.

Strengths and Limitations of the Current Synthesis

Although the current synthesis is extensive in scope, no synthesis is without limitations, and the literature itself has limitations that cannot be overcome through a synthesis alone.

First, message matching studies continue to be conducted predominantly in a limited range of countries. Although our review included samples from five continents, this included only one sample from South America and no sample from Africa. For each represented continent, specific countries were overrepresented: the United States in North America; The Netherlands and the United Kingdom in Europe; Taiwan, China, and South Korea, in Asia; and Australia in Oceania. Given that these countries represent only a portion of the world's population and overlook important cultural differences (e.g., Arnett, 2008; Henrich et al., 2010; Masuda et al., 2020), it will be important to verify the extent to which our findings extend beyond these contexts—particularly within diverse low- and middle-income countries. Relatedly, our review focused on studies published in the English language, which may have introduced mono-language bias into our review (Johnson, 2021). It is possible that examining reports in languages other than English would provide an opportunity to expand the samples covered. Samples were also drawn more frequently from college/university student populations than from any other sampling frame, making our summary estimates unlikely to represent the diverse demographics that exist within countries. Though there is evidence, both from our own results and from past reviews (e.g., Chandler et al., 2022; Huang & Garcia, 2018; Wadi et al., 2022), that motivational matching can be effective for underserved populations within countries, most

of this data comes from the aforementioned overrepresented countries (e.g., the United States), and the benefits of motivational matching could vary across groups (e.g., we found smaller effects for at risk populations; Figure 9). Increasing efforts to conduct research with diverse samples may, over time, provide correctives to these limitations of the literature.

Second, the literature on message matching is unlikely to be impervious to questionable research practices that exist in other areas of the behavioral sciences. In the current synthesis, our sensitivity analyses were largely inconclusive and pointed in mixed directions. For example, we found some evidence that the use of small sample sizes is associated with larger effect sizes, but that incomplete reporting of outcomes (i.e., reporting bias) may be associated with smaller effect sizes in this literature. Our comparison of gray literature (dissertations and theses) to peer-reviewed journal articles also failed to provide compelling evidence of bias. Published articles, on average, showed slightly larger effect sizes, but this pattern was not significant (and all meta-analytic estimates for unpublished studies were still within the range outlined by hypothesis 1). We supplemented our sensitivity analyses with funnel plots to detect bias, which also found limited evidence of bias, and only for interaction effects. This pattern of inconclusive results is further complicated by the observation that substantial heterogeneity consistently emerged across models—making it difficult to interpret the results of these tests with high confidence (Johnson, 2021). In terms of recommendations, we note that extremely few studies were preregistered beforehand or engaged in other open science practices such as the sharing of data and/or analysis scripts. Adopting such practices is therefore strongly recommended for the field to examine and limit the influence of bias on our inferences.

In terms of strengths, our synthesis represents by far the largest meta-analytic project on message matching to date. It is also the first attempt to provide a comprehensive synthesis of motivational message matching. This scope is notable as our inclusion criteria were more stringent than most pre-existing syntheses. For instance, studies were only included if they made use of experimental designs, whereas previous meta-analyses have incorporated quasi-experiments or have not specified design-based criteria (e.g., Lustria et al., 2013; O'Keefe & Jensen, 2006). Although we excluded many studies based on study design, it allowed our meta-analytic estimates to achieve higher causal validity. Additionally, we report separate estimates for well-defined types of outcomes: attitude, intention, self-report behavior, and observed behaviors. This method contrasts with meta-analyses that only evaluate effects on a singular

"persuasiveness" outcome that mixes these outcomes together and with other variables such as message evaluations (e.g., Carpenter, 2012; O'Keefe & Jensen, 2006). Our method affords greater clarity in the estimates produced in our review.

In addition, conclusions that can be drawn from our analysis are strengthened by the requirement that comparison groups had to receive active interventions targeting the same outcome as the positively matched treatment–for instance, we excluded comparisons that received no intervention, or active treatments that targeted different outcomes than the positive match condition (e.g., having a control group read a message on flossing when the intervention promotes physical activity). Doing so allowed us to better isolate the effect of matching, and contrasts with meta-analyses that have opted for a more inclusive selection of comparison groups (e.g., Huang & Shen, 2016; Krebs et al., 2010; Lustria et al., 2013; Noar et al., 2007).

Other strengths include that the protocol (written to meet PRISMA-P guidelines; Moher et al., 2015; Shamseer et al., 2015) and hypotheses were preregistered beforehand, and that the current report adheres to PRISMA guidelines (Page et al., 2021a; 2021b). Our coding also consistently demonstrated high interrater reliability across variables (Joyal-Desmarais, 2020). Finally, we offer the first message matching meta-analysis to directly account for dependencies between effect size estimates (using a three-level model; Van den Noortgate et al., 2013; 2015).

Conclusions

A major goal of the behavioral sciences is to provide key insights into ways in which human behavior can be influenced to promote desirable change, such as by increasing rates of healthy and altruistic behaviors that contribute to societal welfare. Over the last several decades, motivational message matching–along with other forms of message matching (e.g., message tailoring and message framing)–has emerged across diverse disciplines as a highly promising and widely used persuasion technique. Every year, substantial resources are invested around the world to support motivational matching interventions, and research on the topic has been published in hundreds of scientific journals. However, the success of motivational matching has varied widely, and little consensus exists on when and how to best use the technique.

Consequently, the goals of the current synthesis were to unite and map out the literature on motivational matching, evaluate the average effectiveness of the technique, and provide a set of theoretical propositions to guide our understanding of when it leads to larger versus smaller effects. Through a large-scale three-level meta-analysis of 702 experimental studies, we find that

the average motivational message matching effect is around $r = .20$ on attitudinal, intentional, and behavioral outcomes. This effect is sizable for a communication-based technique, stronger than effects previously attributed to other forms of message matching, appears highly robust across a variety of intervention contexts, and emerges for influencing behaviors across many domains (including health, environment, prosocial, political, and consumer-related behaviors).

As substantial as the average motivational messaging effect is, considerable heterogeneity exists such that there is a nontrivial chance that a given instance of motivational matching will be ineffective. We examine and provide initial evidence for several moderating variables that can allow us to better understand when motivational matching is likely to have stronger versus weaker effects and provide a framework to guide future research on motivational matching. Moving forward, researchers and practitioners will be well advised to consider appropriate moderators in their quest to optimize matching effects in their investigations and interventions and thereby realize the full potential of matching strategies for enhancing persuasion.

References

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Table 1

The Four Techniques for Implementing Message Matching

Part A. Descriptions of the Four Techniques.

Descriptions	Exemplar references
Motivational Matching (M). Motivational matching (also known as "functional matching") focuses on how persuasion can be optimized by matching messages to qualitative differences in the *motivations* (e.g., goals, needs, values, psychological functions) that underlie people's thoughts and behaviors. Differences in motivations can reflect differences between individuals (e.g., matching to people's values, goals), or contextually based differences (e.g., matching to the main motivations associated to an object).	Carpenter, 2012; Clary et al., 1994; 1998; Clary & Snyder, 1999; Gardner et al., 1999; Han & Shavitt, 1994; Joyal-Desmarais et al., 2020; Shavitt, 1989; 1990; Snyder & DeBono, 1985.
Message *Framing* (F). Message framing focuses on how persuasion can be optimized by using message matching principles to specify when different message frames are more persuasive. Common frames include: (1) *gain frames,* emphasizing the benefits obtained by [compliance with] an act; (2) l*oss frames*, emphasizing the costs obtained by [noncompliance with] an act; (3) *non-gain frames* emphasizing the benefits avoided by [noncompliance with] an act; and (4) *non-loss frames*, emphasizing the costs avoided by [compliance with] an act.	Cesario et al. 2013; Gallagher & Updergraff, 2012; Meyerowitz & Chaiken, 1987; O'Keefe & Jensen, 2006; 2007; 2008; Rothman et al., 1993; 2006; 2020; Rothman & Updegraff, 2010.
Message *Tailoring* (T). Message tailoring (or personalized matching) focuses on how persuasion can be optimized by developing, selecting, and/or delivering messages in accordance with data obtained from individual-based assessments (i.e., information *intrinsic* to an individual). Message tailoring is generally mutually exclusive with context matching.	Joyal-Desmarais et al., 2020; Krebs et al., 2010; Kreuter & Skinner, 2000; Kreuter et al., 1999b; Lustria et al., 2009; Noar et al., 2007; Sohl & Moyer, 2007; Teeny et al., 2020
Context Matching (C). Context matching (or context congruity) focuses on how persuasion can be optimized by developing, selecting, and/or delivering messages in accordance with data obtained about the situation and/or context people find themselves in (i.e., information *extrinsic* to an individual). Context matching is generally mutually exclusive with message tailoring.	Hühn et al., 2017; Joyal-Desmarais, 2020; Lee et al., 2015; McCormick & McElroy, 2009; Müller et al., 2017; York et al., 2012a.

Part B. Examples of How Different Combinations of Techniques are Used to Pursue Motivational Matching

Characteristic(s) Matched to	Message Variations	How characteristics are determined for matching	Technique[a,b]				Exemplar references
			M	*F*	*T*	*C*	
Collectivistic or interdependent vs. *individualistic or* independent values	Other-focused appeal vs. Self-focused appeal	Personality assessment or group membership	x		x		Agrawal & Maheswaran (2005); Han & Shavitt (1994); Zhang & Gelb (1996); Uskul & Oyserman (2010)
		Values induced experimentally	x			x	Agrawal & Maheswaran (2005); Uskul & Oyserman (2010);

	Loss or non-loss frame vs. Gain or non-gain frame	Personality assessment or group membership	x	x	x		Sung & Choi (2011); Yu & Shen (2013)
		Values induced experimentally	x	x		x	Chen (2016); Kareklas et al. (2012); Sung & Choi (2011)
Promotion vs. *Prevention* focus	Hedonic appeal vs. Utilitarian appeal	Personality assessment or group membership	x		x		Ashraf et al. (2016); Tran et al. (2020)
		Focus induced experimentally	x			x	Ashraf & Thongpapanl (2015); Ashraf et al. (2016); Chernev (2004); Lin & Shen (2012)
	Loss or non-loss frame vs. Gain or non-gain frame	Personality assessment or group membership	x	x	x		Cesario et al. (2013); Cornelis et al. (2012); Daryanto et al. (2010); Joyal-Desmarais et al. (2020)
		Focus induced experimentally	x	x		x	Cesario et al. (2004; 2013); Bertolotti & Catellani (2015); Borges & Gomez (2015)
Utilitarian vs. *social-adjustive* vs. *value-expressive* vs. *knowledge* function	Utilitarian vs. social-adjustive vs. value-expressive vs. knowledge appeal	Personality assessment or group membership	x		x		DeBono (1987); DeBono & Packer (1991); Paek et al. (2010); Snyder & DeBono (1985)
		Function tied to objects or induced experimentally	x			x	Julka & Marsh (2006); Paek et al. (2010); Shavitt (1990)
Cognitive vs. *Affective* attitude basis or orientation	Cognitive appeal vs. affective appeal	Personality assessment or group membership	x		x		Haddock et al. (2008); Mayer & Tormala (2010)
		Attitude base/orientation induced experimentally	x			x	Fabrigar & Petty (1999); Mayer & Tormala (2010); Millar & Millar (1990)

[a]M = motivational matching; F = message framing; T = message tailoring; C = context matching.
[b]The classification of M depends on whether the characteristic being matched to within a study conveys differences in the types of motivations that drive people's thoughts and behaviors (column 1). The classification of F depends on the message variations being compared (column 2). The classification of T and C depend on how the characteristics used to implement message matching are determined (column 3).

Table 2

Primary Meta-Analytic Results for Motivational Matching by Type of Effect and Type of Outcome

Type of Effect & Outcome	Meta-Analytic Estimate				Synthesis Across			Heterogeneity			
	r	95% *CI*		Sig.	Effect	Study	*N*	I^2	σ	95% *PI*	
		Low	High	(*p*)	#	#		(lv2 / lv3)	(lv2 / lv3)	Low	High
Main Effects											
Attitudes	.209	.180	.237	<.001	1996	364	79,548	52.2 / 38.9	.273 / .235	-.458	.725
Intentions	.191	.167	.215	<.001	2118	369	88,517	46.5 / 41.7	.212 / .201	-.362	.645
Self-Report Behavior	.077	.046	.107	<.001	305	56	46,428	78.7 / 14.4	.163 / .070	-.268	.404
Observed Behavior	.179	.129	.228	<.001	328	62	36,539	35.2 / 54.8	.141 / .176	-.259	.556
Interaction Effects											
Attitudes	.238	.200	.274	<.001	223	180	34,598	18.4 / 72.6	.114 / .227	-.254	.631
Intentions	.231	.201	.260	<.001	256	199	37,911	26.8 / 61.5	.118 / .178	-.184	.576
Self-Report Behavior	.201	.106	.293	.002	7	5	533	0.0 / 0.0	.000 / .000	.106	.293
Observed Behavior	.230	.070	.379	.008	18	15	1,582	0.0 / 84.1	.000 / .277	-.356	.687
TOTAL	**.198**	**.181**	**.216**	**<.001**	**5251**	**702**	**206,482**	**48.4 / 43.2**	**.221 / .209**	**-.375**	**.662**

Notes. *r* = meta-analytic estimate expressed as a correlation; 95 % *CI* = 95% confidence interval; Sig (*p*) = statistical significance level expressed as *p*-value; Effect # = number of effect size estimates aggregated; Study # = number of studies used to derive meta-analytic estimate; *N* = cumulative sample size across studies used to derive meta-analytic estimates; I^2 = Higgin's and Thompson's I^2, where level 2 (lv2) refers to the within-cluster heterogeneity and level 3 (lv3) refers to the between-cluster heterogeneity; σ = estimate of τ, the standard deviation of true effect sizes, where level 2 (lv2) refers to the within-cluster heterogeneity and level 3 (lv3) refers to the between-cluster heterogeneity; 95% PI = 95 % prediction interval. Effects are coded such that positive values for *r* indicate a more successful intervention in the positively-matched message condition relative to comparison conditions (e.g., mismatched messages).

Table 3

Summary of Results from Sensitivity Analyses Examining Risk of Bias on Estimates from Table 2

Variable	Description	Evidence (and Direction of Bias) for Each Risk of Bias Variable[a]
1. Message fully available	Were messages (intervention materials) made fully available by the authors?	**No Evidence**. Single total test is not significant. Zero of seven outcome tests significant.
2. Covariates included	Were covariates included in the analyses to derive the effect size?	**No Evidence.** Zero of three total tests significant. Zero of 15 outcome tests significant.
3. Manipulation confounded	Do intervention conditions differ only in degree of matching (or is the manipulation confounded)?	**Mixed Evidence**: One of three total tests significant: bias (confounded manipulation) associated with *smaller* effects. Three of 18 outcome tests significant, but direction is *mixed*.
4. Selection Bias	Is the randomization process explicitly described as truly random?	**Mixed Evidence**: One of three total tests significant: bias (not truly random) associated with *larger* effects. Three of 16 outcome tests significant, but direction is *mixed*.
5. Performance bias	Could a lack of blinding/masking lead to a bias on participants' actual outcomes?	**Mixed Evidence**: One of three total tests significant: bias (lack of blinding/masking) associated with *larger* effects. Five of 17 outcome tests significant, but direction is *mixed*.
6. Attrition bias	Was the attrition rate between assessment time and randomization less or greater than 20%?	**Some Evidence (Down)**: One of three total tests, and two of 10 outcome tests significant: bias (high attrition) associated with *smaller* effects.
7. Reporting bias	Are results reported (regardless of whether effects could be extracted) for all matching effects and all subgroups?	**Some Evidence (Down)**: Single total test is significant, and one of six outcome tests significant: bias (incomplete reporting) associated with *smaller* effects.
8. Extractable effects	Percent of effects examined in a study that could be extracted by coders.	**Mixed Evidence**: Zero of three total tests significant. Four of 19 outcome tests significant, but direction is *mixed*.
9. Sample size (effect-level)	Sample size associated with each effect size.	**Limited Evidence (Up)**: Single total test is not significant. Three of eight outcome tests significant: bias (smaller samples) associated with *larger* effects.
10. Sample size (study-level)	Sample size associated with the overall study an effect was extracted from.	**Some Evidence (Up)**: Single total test is significant, and four of eight outcome tests significant: bias (i.e., smaller samples) associated with *larger* effects.
11. Publication type	Peer-reviewed journal articles compared to dissertations/theses.	**Limited Evidence (Up):** Single total test is not significant. One of five outcome tests is significant; bias (published findings) associated with *larger* effects.

[a]Could each include up to three tests using total estimates (aggregating across outcome/effect types) and 24 tests broken down by outcome (and effect type). Number of tests depends on whether there were at least four studies per level of a moderator. Conclusions were selected as follows: (a) *some evidence* indicates total tests and specific outcome tests converge in significance and direction, but less than half of tests are significant; (b) *limited evidence* indicates that either total tests or outcome tests were significant (not both), and that significant tests converged in direction; (c) *mixed evidence* indicates significant tests showed conflicting results; (d) *no evidence* indicates lack of any significant test.

Table 4

Summary of Evidence for Overall Benefits of Motivational Message Matching

PICOS Guiding Systematic Review

Population: Human population

Intervention: Positively matched messages

Comparisons: Mismatched messages (including non-matched or negatively matched), generic messages (including mixed appeals), weakly matched messages

Outcome: Attitudes, intentions, self-report behavior, or observed behavior

Study design: Experimental studies (randomized control trials)

Summary of Evidence	**Outcome Evaluated**				
	Overall Effect	**Attitude**	**Intention**	**Self-Report Behavior**	**Observed Behavior**
Meta-Analytic Estimate(s) [95% *CI*s][a]	*r* = .20 [.18, .22]	*r* = .21 [.18, .24]; *r* = .24 [.20, .27]	*r* = .19 [.17, .22]; *r* = .23 [.20, .26]	*r* = .08 [.05, .11]; *r* = .20 [.11, .29]	*r* = .18 [.13, .23]; *r* = .23 [.07, .38]
Alternate Metric (Converted *r* to *d*, *OR*)	*d* = 0.40, *OR* = 2.08	*d* = 0.43, *OR* = 2.17; *d* = 0.49, *OR* = 2.43	*d* = 0.39, *OR* = 2.03; *d* = 0.47, *OR* = 2.37	*d* = 0.15, *OR* = 1.32; *d* = 0.41, *OR* = 2.11	*d* = 0.36, *OR* = 1.93; *d* = 0.47, *OR* = 2.36
N (Study #)	206,482 (702)	79,548 (364); 34,598 (180)	88,517 (369); 37,911 (199)	46,428 (56); 533 (5)	36,539 (62); 1,582 (15)
Certainty of Evidence					
Hypothesis H1: Average effect is $.10 \geq r \leq .30$[b,c]	⊕⊕⊕⊕ **High**[b]	⊕⊕⊕⊕ **High**[b,d]	⊕⊕⊕⊕ **High**[b,d]	⊕⊕⊕○ **Moderate**[c,d,e]	⊕⊕⊕⊕ **High**[b,d]
A given future study or intervention would find a positive effect ($r > .00$)[f]	⊕⊕⊕○ **Moderate**[f,g]	⊕⊕⊕○ **Moderate**[d,f,g]	⊕⊕⊕○ **Moderate**[d,f,g]	⊕⊕⊕○ **Moderate**[d,f,g]	⊕⊕⊕○ **Moderate**[d,f,g]

Notes. *CI* = confidence interval; *r* = Pearson's correlation coefficient; *d* = Cohen's *d*; *OR* = odds ratio; *N* = sample size; Study # = number of studies synthesized. Effects are coded such that positive values for *r* and *d*, or values >1 for *OR*s, reflect a more successful intervention in the positively-matched message condition relative to the comparison condition (e.g., mismatched message). Definitions and detailed decision-making criteria for each level of certainty (ranging from high to very low) are guided by GRADE (Guyatt et al., 2008) and provided in the supplemental materials along with evidence profile tables.

[a]These are the primary estimates from Table 2. When two estimates are present, the first is for main effects and the second for interaction effects.

[b]*High certainty* = high confidence that the average effect size for motivational matching lies within the range of *r* = .10 to *r* = .30.

[c]*Moderate certainty* = moderate confidence that the average effect size for motivational matching lies between *r* = .10 to *r* = .30. The average effect is likely to be close or within this range, but there is potential for it to be substantially different.

[d]Interaction estimate may be biased, but no evidence of bias for the main effect estimate. Both are highly consistent with H1.

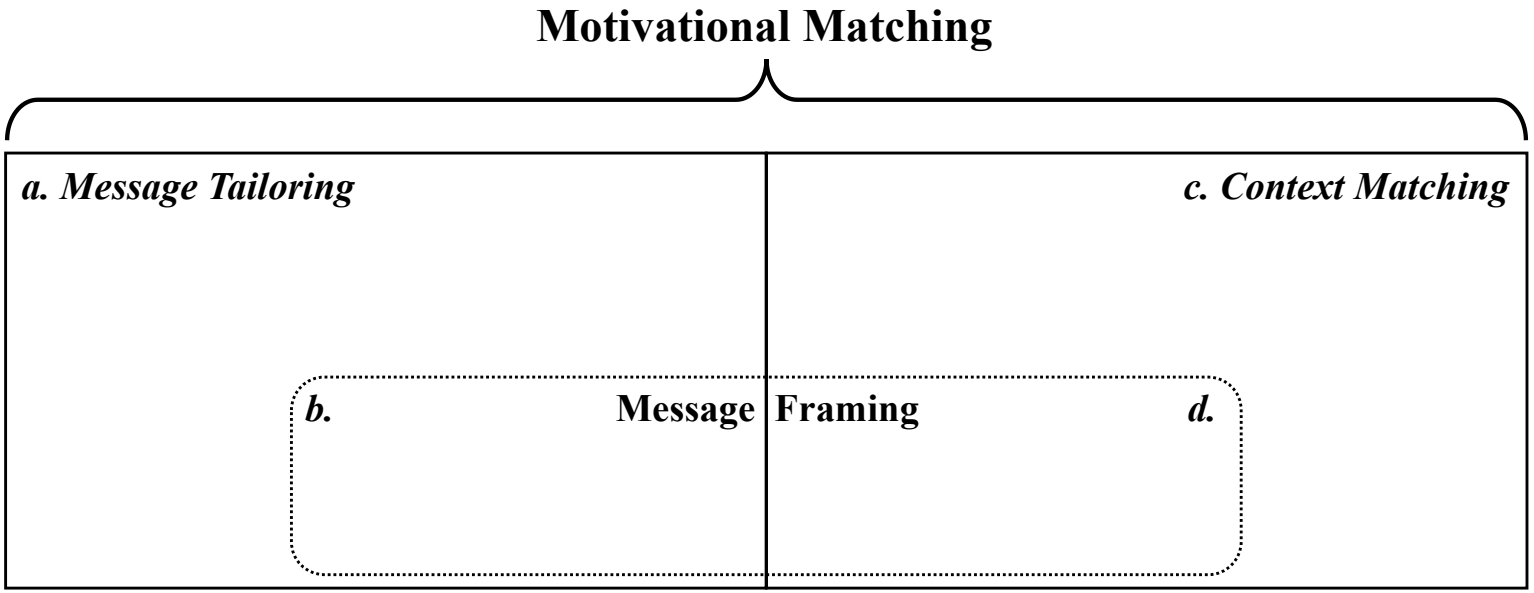
[e]Main effect estimate slightly below expected range, but CI overlaps with expected range (certainty downgraded by 1).

[f]*Moderate certainty* = moderate confidence that a future matching study/intervention would produce an effect that is positive in direction. An effect is likely to be positive, but there is substantial potential for it to be negative in direction or approximately zero.

[g]95% Prediction interval is very wide and extends well into the negative range (e.g., below *r* = -.20; certainty downgraded by 1).

Figure 1

Conceptual and Organizational Map of Different Types of Motivational Matching Effects



Examples of Motivational Matching Studies that Use…

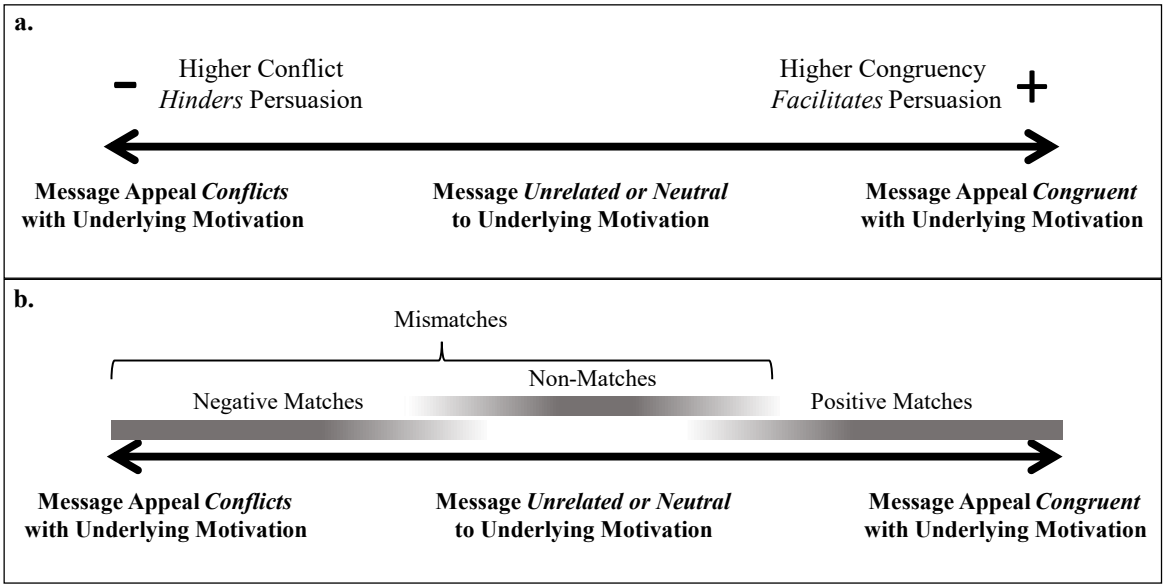
(a) Tailoring without Framing: matching self- vs. other-focused appeals to individual differences in individualistic/collectivistic values (Han & Shavitt, 1994)

(b) Tailoring and Framing: matching message frames to individual differences in regulatory focus (Joyal-Desmarais et al., 2020)

(c) Context Matching without Framing: matching social vs. utilitarian appeals to social (e.g., greeting cards) vs. utilitarian (e.g., air conditioner) products (Shavitt, 1990)

(d) Context Matching and Framing: matching message frames to regulatory focus primes (Cesario et al., 2013)

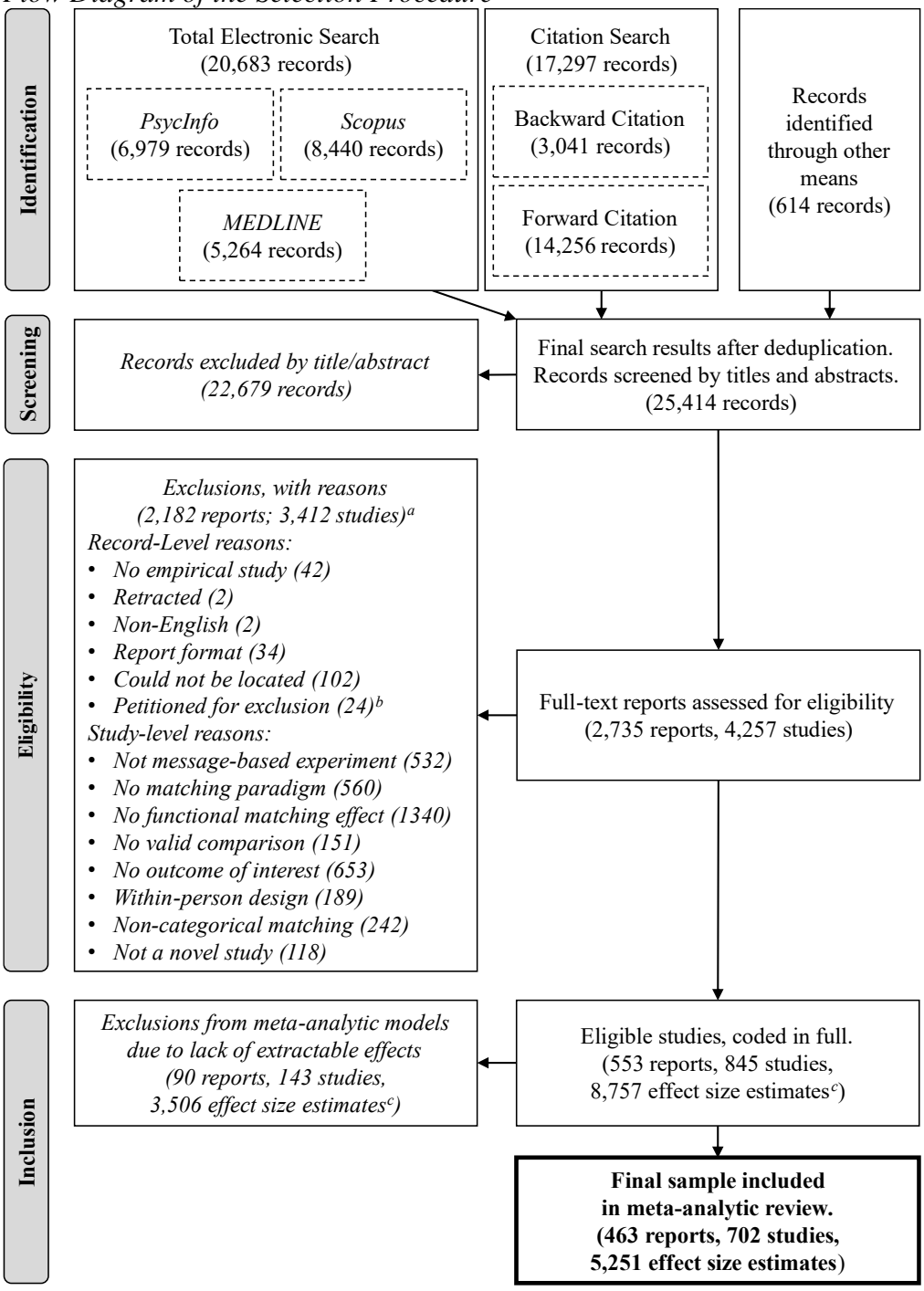
Figure 2
Continuum of Message Matching Effects



Notes. The upper panel (a) reflects the mechanism whereby persuasion depends on the degree to which a message is congruent or in conflict with a person's underlying motivations. The lower panel (b) shows how this continuum maps on to different types of comparison messages.

Figure 3

Flow Diagram of the Selection Procedure



Notes. Flow diagram adapted from Moher et al. (2009). The final dataset for the meta-analysis is indicated by the box with bold font/border. *Italics* indicate excluded records.

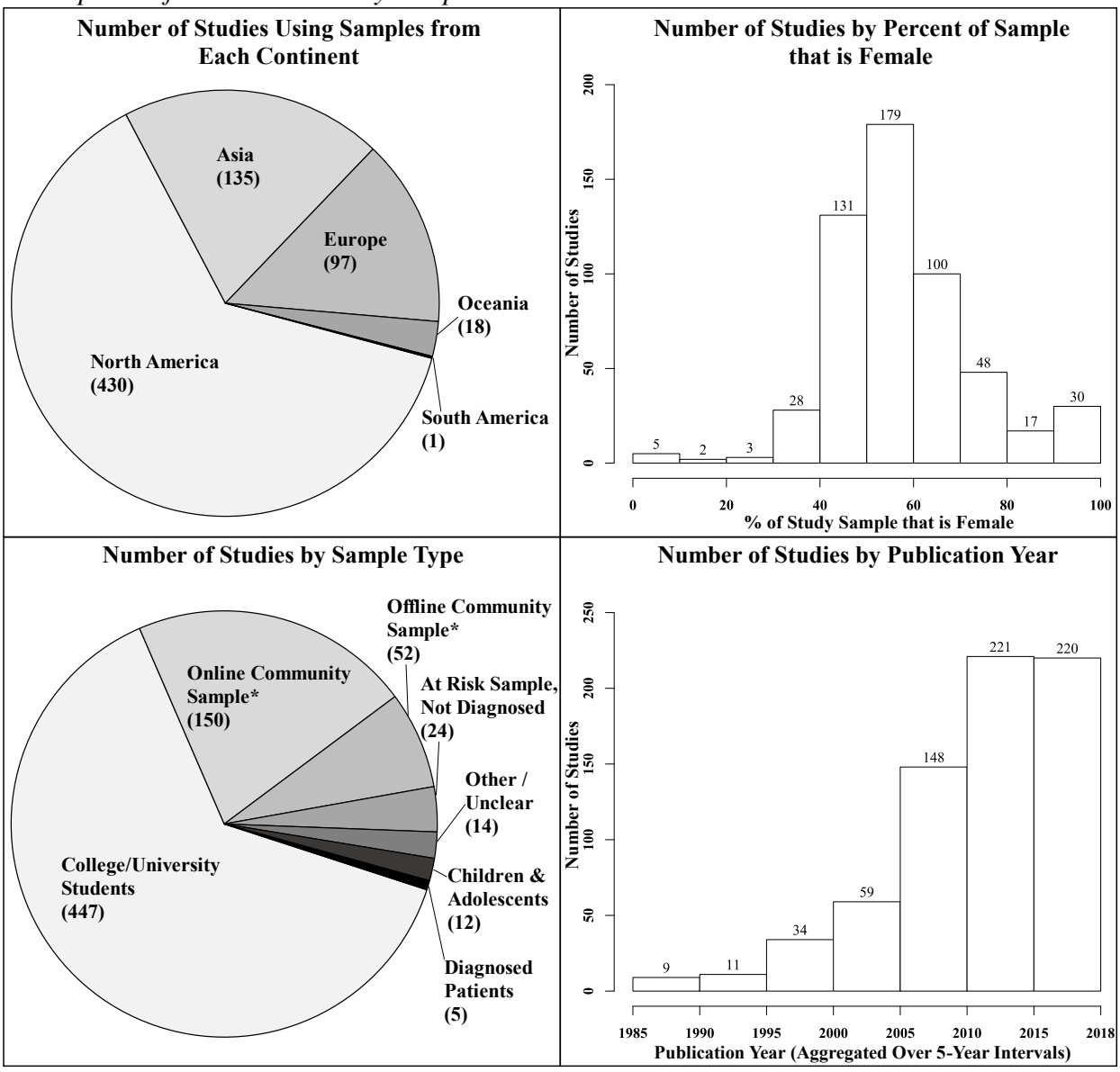
[a]Coders could select more than one reason for excluding reports/studies (see https://osf.io/6f24t/ for a breakdown per report/study).

[b]The main reason for petitioning an article for exclusion was poor quality of writing, making it impossible to reliably code the report. In addition, two articles were excluded for methodological reasons (see Footnote 4).

[c]The number of estimates reflects the effects that could theoretically be obtained given the design of studies. Covers effects from excluded studies, and non-extracted effects from the 702 included studies.

Figure 4

Descriptive Information on Study Samples

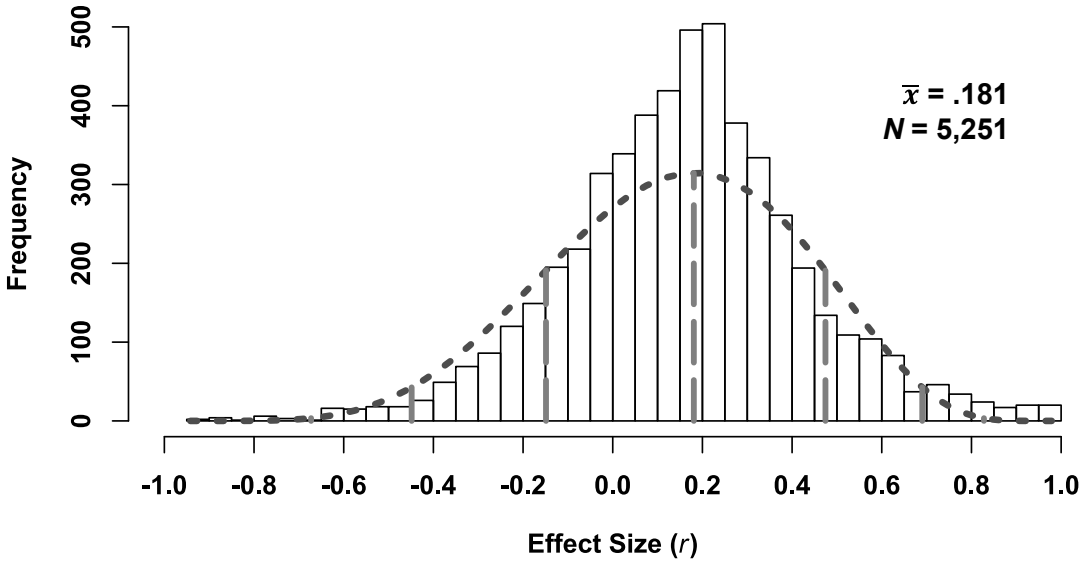


Notes. Figure excludes studies for which no information could be extracted on the country of the study, or regarding the gender breakdown of the sample.

*The two categories of "community sample" were defined as samples drawn from populations that did not meet criteria for inclusion into any other category.

Figure 5

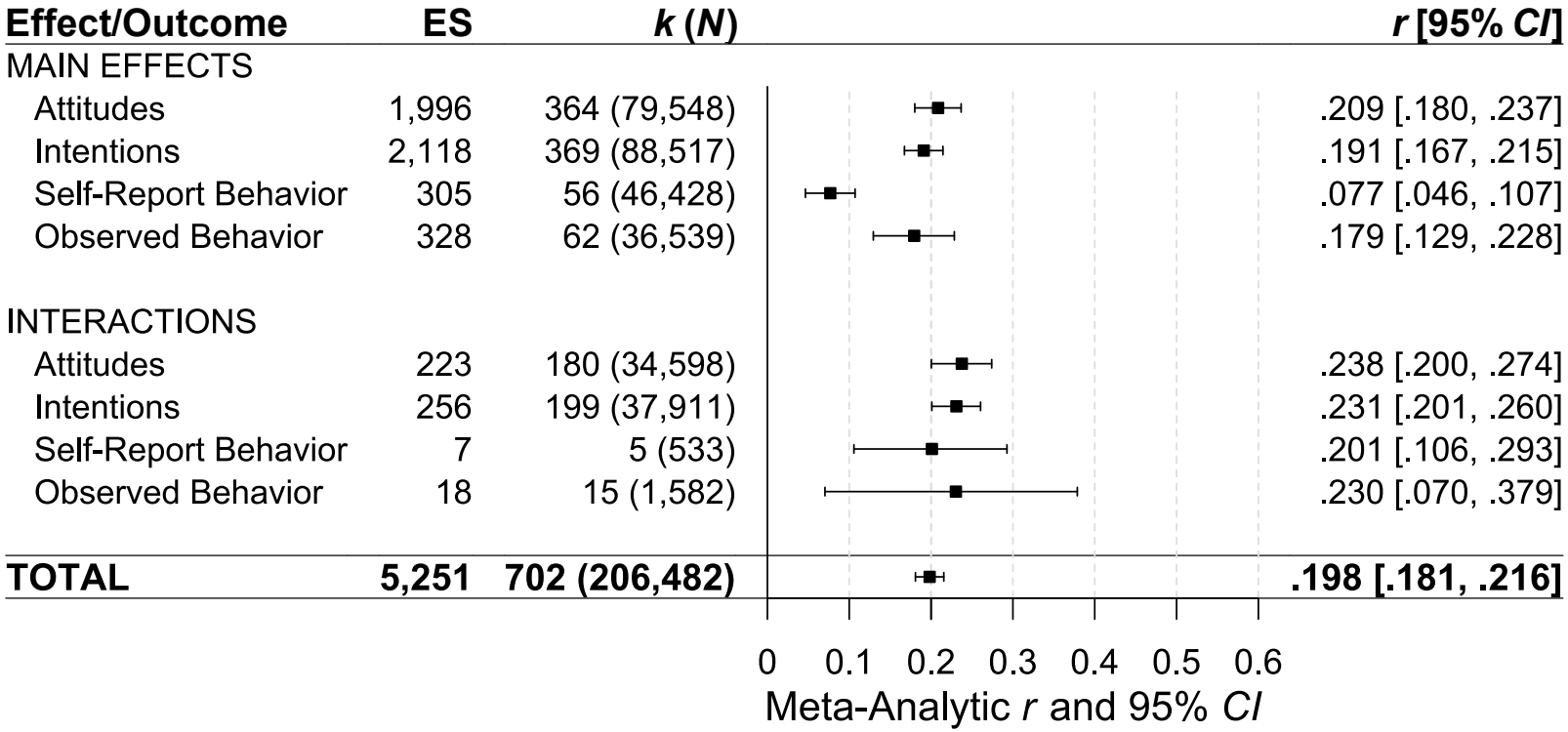
Histogram of the Effect Sizes in this Synthesis



Notes. r = Pearson Correlation; $\bar{x}$ = mean effect size; N = number of effect size estimates extracted. Effects are coded such that positive values for r indicate a more successful intervention in the positively-matched message condition relative to comparison conditions (e.g., mismatched messages). This histogram presents the frequency at which effect sizes were observed at different magnitudes. The dark dotted line presents the expected distribution of scores for a normal distribution of the effects (normal when expressed as Fisher's Z, but plotted here in a metric transformed back to r), when the distribution has similar properties (i.e., mean, standard deviation) to the observed data. The light gray dashed lines locate the mean of the distribution, and scores falling at one standard deviation increments from it.

Figure 6

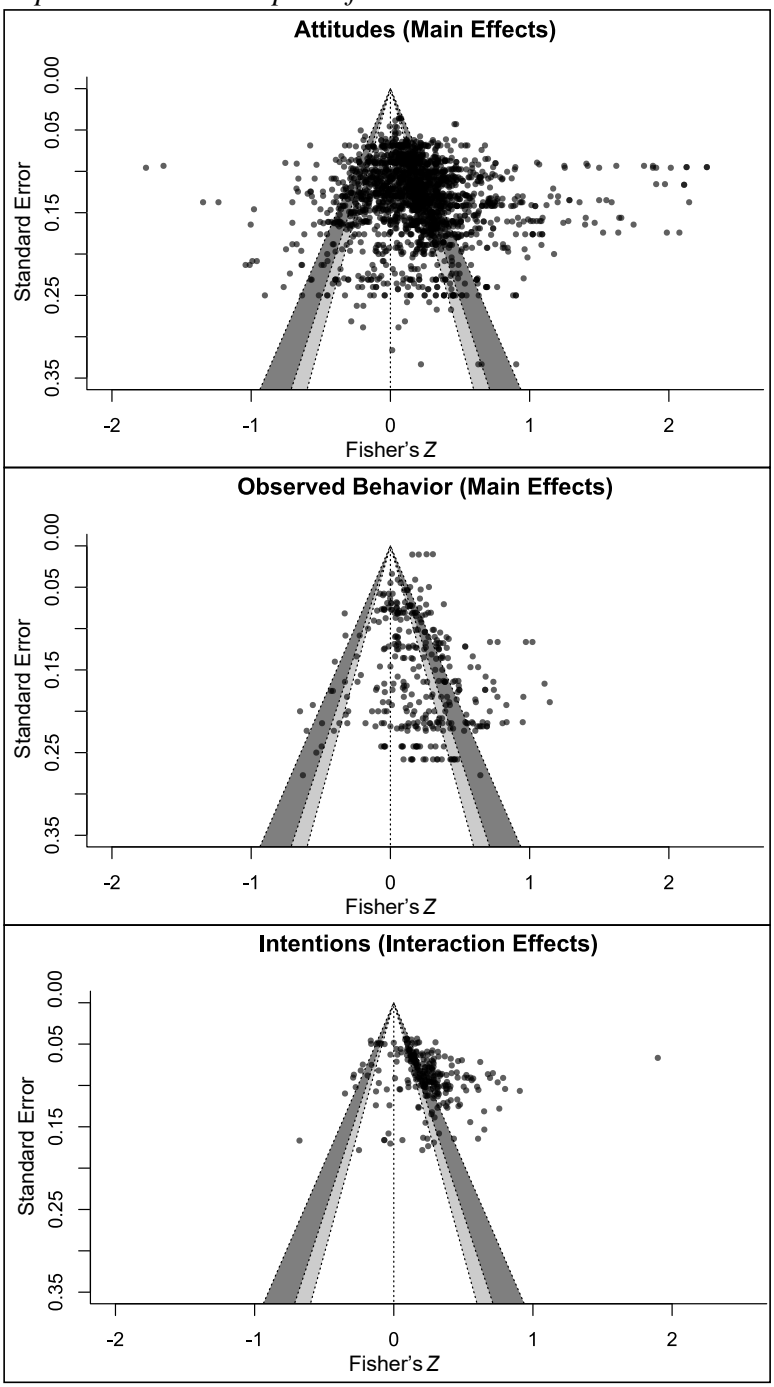
Primary Meta-Analytic Findings for Motivational Matching According to Effect and Outcome Types



Notes. *ES* = Number of effect sizes aggregated meta-analytically; *k* = number of studies aggregated meta-analytically; *N* = cumulative sample size across studies; *r* = meta-analytic estimate expressed as a correlation; *CI* = confidence interval for the meta-analytic estimate. The "total" estimate aggregates across all types of effects/outcomes. Effects are coded such that positive values for *r* indicate a more successful intervention in the positively-matched message condition relative to comparison conditions (e.g., mismatched messages).

Figure 7

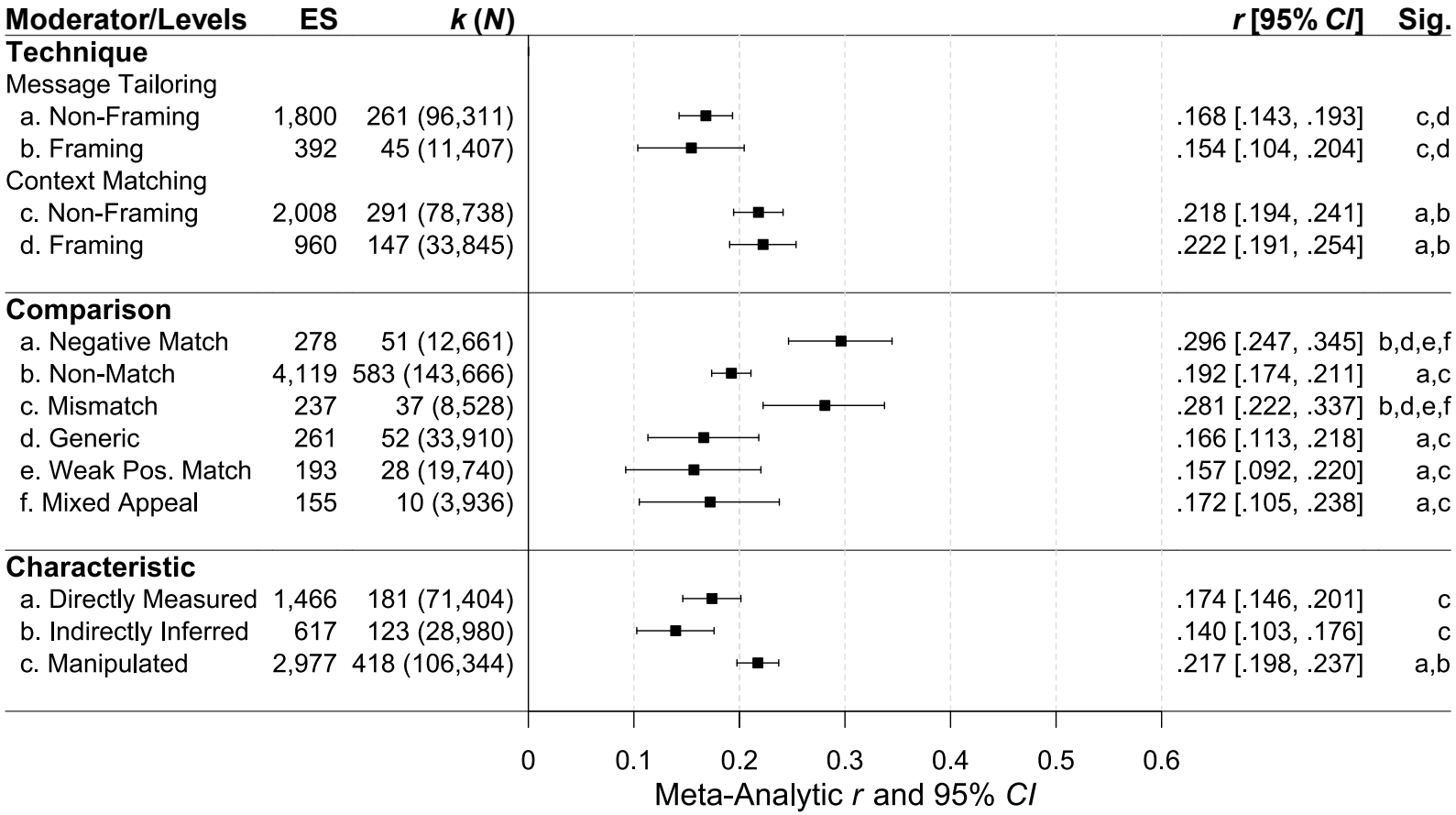
Representative Examples of Contour-Enhanced Funnel Plots of Motivational Matching Effects



Notes. Contour-enhanced funnel plots are centered around an effect size of zero. Effects in the white region within the funnel are not significantly different from zero. Effects in the light gray region correspond to significance values between $p = .10$ and .05. Effects in the dark gray region represent effects significantly different from zero at a level between $p = .05$ and $p = .01$. The white region outside the funnel captures significant effects at $p < .01$. The upper panel presents a funnel plot for main effects on attitudes. The middle panel presents a funnel plot for main effects on observed behavior. The lower panel present a funnel plot for interaction effects on intentions. Additional funnel plots for each of the estimates from Table 2 are available in Section 4 of the Supplemental Materials.

Figure 8

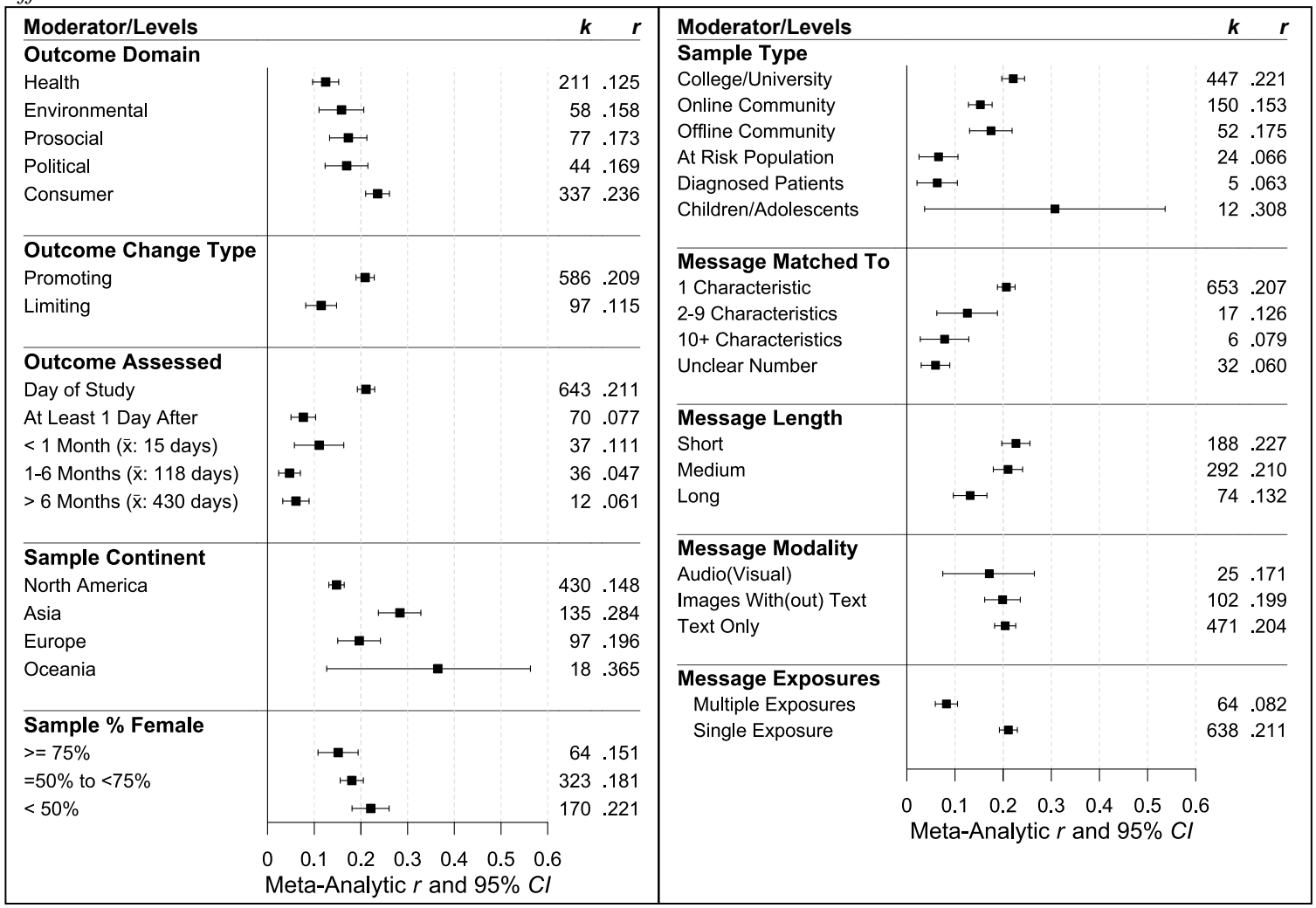
Evidence of Moderation Across the Three Propositions: Matching Technique, Comparison Group, and Characteristic Determination (Meta-Regression Findings)



Notes. *ES* = Number of effect sizes aggregated meta-analytically; *k* = number of studies aggregated meta-analytically; *N* = cumulative sample size across studies; *r* = meta-analytic estimate expressed as a correlation; *CI* = confidence interval for the meta-analytic estimate; Sig. Dif. = Column denoting which subgroups significantly differ from each other through meta-regression tests; for example, when considering the "characteristic" (determination) moderator: "a" signifies the estimate is significantly different than that for the "directly measured" level; "b" signifies the estimate is significantly different than that for the "indirectly inferred" level, and; "c" indicates the estimate is significantly different than that for the "manipulated" level. Effects are coded such that positive values for *r* indicate a more successful intervention in the positively-matched message condition relative to comparison conditions (e.g., mismatched messages).

Figure 9

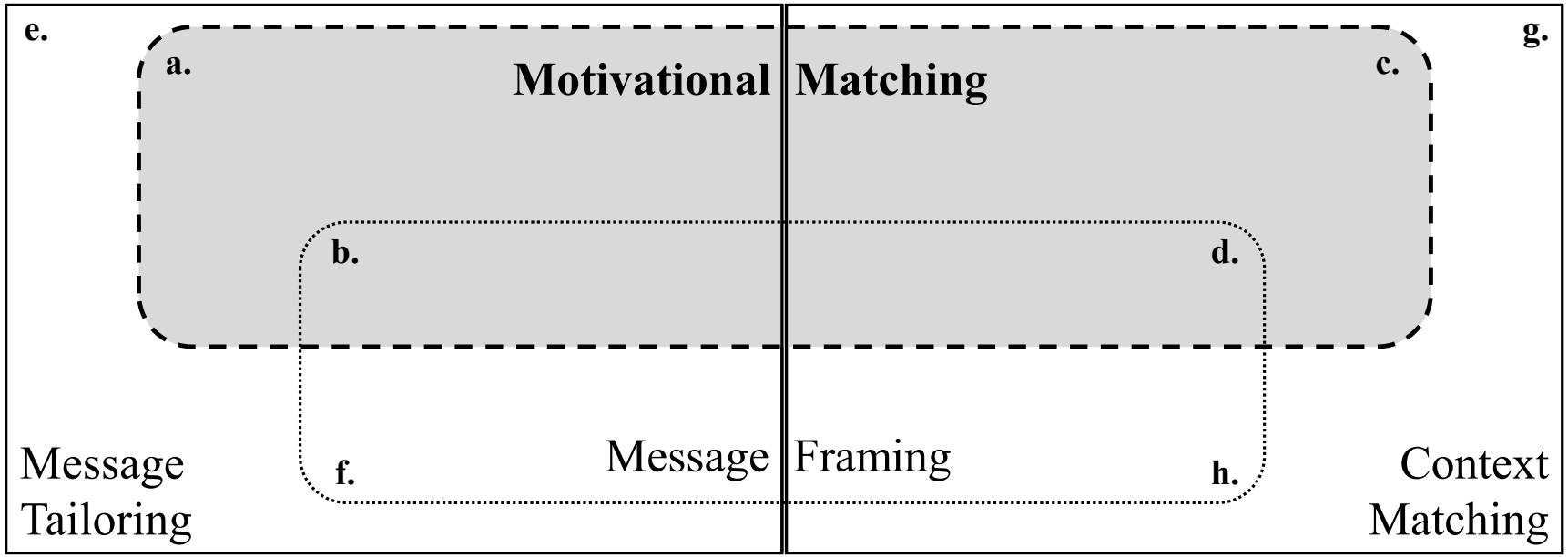
Subgroup Analyses Across 10 Operational Factors to Examine the Breadth of Conditions for Which Motivational Message Matching Effects can be Obtained



Notes. *k* = number of studies aggregated meta-analytically; *r* = meta-analytic estimate expressed as a correlation; *CI* = confidence interval for the meta-analytic estimate.; $\bar{X}$ = average. Effects are coded such that positive values for *r* indicate a more successful intervention in the positively-matched message condition relative to comparison conditions (e.g., mismatched messages).

Figure 10

Expanded Mapping of the Message Matching Literature: Comparing Effect Sizes Across Syntheses of Motivational and Non-Motivational Matching



***Motivational Matching* Studies that Use…**

(a) Tailoring without Framing: Current synthesis finds average meta-analytic estimate at $r = .17$.

(b) Tailoring with Framing: Current synthesis finds average meta-analytic estimate at $r = .15$.

(c) Context Matching without Framing: Current synthesis finds average meta-analytic estimate at $r = .22$.

(d) Context Matching with Framing: Current synthesis finds average meta-analytic estimate at $r = .22$.

***Non-Motivational* Matching Studies that Use…**

(e) Tailoring without Framing: Past meta-analytic estimates typically between $r = .06$ and $r = .10$ (mostly limited to tests conducted in the health domain).

(f) Tailoring with Framing: Meta-analytic estimates not available.

(g) Context Matching without Framing: Meta-analytic estimates not available

(h) Context Matching with Framing: Past meta-analytic estimates typically between $r = .03$ and $r = .08$ (mostly limited to tests of the risk perception approach in the health domain).

Notes. r = meta-analytic estimate expressed as a correlation. Effects are coded such that positive values for r indicate a more successful intervention in the positively-matched message condition relative to comparison conditions (e.g., mismatched messages).