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Revisiting the Rigidity-of-the-Right Hypothesis: A Meta-Analytic Review

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The rigidity-of-the-right hypothesis (RRH), which posits that cognitive, motivational, and ideological rigidity resonate with political conservatism, is an influential but controversial psychological account of political ideology. Here, we leverage several methodological and theoretical sources of this controversy to conduct an extensive quantitative review with the dual aims of probing the RRH's basic assumptions and parsing the RRH literature's heterogeneity. Using multilevel meta-analyses of relations between varieties of rigidity and ideology measures alongside a bevy of potential moderators (s = 329, k = 708, N = 187,612), we find that associations between conservatism and rigidity are tremendously heterogeneous, suggesting a complex—yet conceptually fertile—network of relations between these constructs. Most notably, whereas social conservatism was robustly associated with rigidity, associations between economic conservatism and rigidity indicators were inconsistent, small, and not statistically significant outside of the United States. Moderator analyses revealed that nonrepresentative sampling, criterion contamination, and disproportionate use of American samples have yielded overestimates of associations between rigidity-related constructs and conservatism in past research. We resolve that drilling into this complexity, thereby moving beyond the question of *if* conservatives are essentially rigid to *when* and *why* they might or might not be, will help provide a more realistic account of the psychological underpinnings of political ideology.

Keywords: political ideology, cognitive rigidity, rigidity-of-the-right, heterogeneity, meta-analysis

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Perhaps the most influential psychological account of what distinguishes leftists from rightists is known as the *rigidity-of-the-right hypothesis* (henceforth, RRH; Tetlock, 1983). Put plainly, the RRH suggests that conservative political ideology—which reflects preferences for free-market economics, a limited social safety net, traditional moral values, and conventional cultural norms—is congenial to people who are cognitively, motivationally, and ideologically rigid (Adorno et al., 1950; Jost et al., 2003; Wilson, 1973).

The RRH has received extensive coverage in national news media (e.g., Douthat, 2020) and popular trade books (e.g., Jost, 2021; Lakoff, 2008; Westen, 2007); it is even a mainstay in public discourse concerning partisanship and political polarization (e.g., Hetherington & Weiler, 2018). Nevertheless, the model's validity and usefulness remain a topic of protracted scientific controversy (see Malka et al., 2017; Morgan & Wisneski, 2017; Zmigrod, 2020). On the one hand, several prior meta-analytic reviews have reported positive relations between political conservatism and rigidity-related variables (e.g., Jost, 2017; Jost et al., 2003; Van Hiel et al., 2016), prompting many scholars to champion and refine the notion that leftists and rightists fundamentally differ in their psychological profiles. On the other hand, a number of critiques of the RRH have emerged over the years, including arguments that politically biased thinking is effectively equivalent across the political spectrum (e.g., Ditto et al., 2019), that rigidity characterizes political extremists on both the right and left (e.g., Tetlock et al., 1984; Zmigrod et al., 2020; see also Costello et al., 2021), and that recurrent methodological shortcomings have systematically "stacked the deck" in favor of the RRH (e.g., Malka et al., 2017).

Debates concerning the relative rigidity of conservatives and liberals have endured for decades—but why? We suspect that the

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Thomas H. Costello played lead role in conceptualization, data curation, formal analysis, investigation, methodology, visualization and writing of original draft. Shauna M. Bowes played supporting role in conceptualization, data curation, formal analysis, methodology and writing of review and editing. Matt W. Baldwin played supporting role in conceptualization, formal analysis, methodology, writing of original draft and writing of review and editing. Ariel Malka played supporting role in conceptualization, investigation, methodology, writing of original draft and writing of review and editing. Arber Tasimi played lead role in resources and supervision and supporting role in conceptualization, investigation, writing of original draft and writing of review and editing.

Supporting materials for this article, including data and analytic code, are openly available at https://osf.io/uqexj/.

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vast RRH literature contains a considerable degree of theoretical and methodological heterogeneity, especially in how scholars conceptualize and operationalize "the right" and "rigidity" as psychological constructs (e.g., Malka et al., 2017; Zmigrod, 2020), leading competing camps to chronically talk past one another and leaving the field mired in seemingly perpetual controversy.

Consider, for example, that political ideology can be disaggregated into at least two conceptually distinct dimensions—social and economic ideology—and that these dimensions may be rooted in distinct constellations of psychological processes (e.g., Costello & Lilienfeld, 2021; Duckitt & Sibley, 2009; Federico & Malka, 2018; Morgan & Wisneski, 2017; Treier & Hillygus, 2009). By the same token, consider that the umbrella category of "rigidity" spans dozens of constructs that are unlikely to reflect a single, stable dimension (Cherry et al., 2021), yet these constructs are used interchangeably by proponents and critics of the RRH alike. Reflecting such pervasive definitional wooliness, political conservatism has often been operationalized in many studies using measures that include rigidity-related content, and vice versa (Malka et al., 2017). Moreover, the magnitude and direction of relations between "the right" and "rigidity" seem to vary greatly as a function of context, methodology, and individual differences (e.g., from norms in political discourse to measurement modality to people's degree of political engagement; Federico, 2022; Johnston et al., 2017).

Given these concerns, scholarship primarily focusing on global differences in "rigidity" between "liberals" and "conservatives" is likely to underestimate the complexity of interrelations among psychological processes and political ideology, thereby hindering meaningful, risky tests of the RRH (see Meehl, 1978). Accordingly, for research in this area to advance, political psychologists may do well to move the discussion away from *if* conservatives are more rigid than (and otherwise psychologically distinct from) liberals to (a) *when* politics and rigidity-related processes intersect and (b) *why* the RHH's explanatory power varies across people and places.

Here, we synthesize and meta-analytically parse these questions. Rather than focus solely on reporting and interpreting *point estimates of main effects* (i.e., overall relations between conservatism variables and rigidity variables), we focally emphasize estimates of *substantive heterogeneity* (i.e., the degree of difference in true effects across observations) and *boundary conditions* (i.e., for who, where, and when the RRH holds true).

RRH

The notion that there is a relation between rigidity and conservatism has been with us for many decades (e.g., Adorno et al., 1950; Freud, 1921; Katz, 1960; Kaufman, 1940; McClosky, 1958). During this time, social scientists have conducted hundreds of tests bearing on the RRH, describing left–right differences in domains such as complexity of policy statements made by U.S. Senators and members of the British House of Commons (e.g., Tetlock, 1983; Tetlock et al., 1984), abstract reasoning abilities (e.g., O'Connor, 1952), tolerance of ambiguity (e.g., Block & Block, 1951), general neurocognitive functioning (e.g., Amodio et al., 2007; Nam et al., 2021), and working memory processes (e.g., Buechner et al., 2021).

Although theoretical accounts of the RRH vary (e.g., Adorno et al., 1950; Altemeyer, 1996; Hetherington & Weiler, 2018; Wilson, 1973; see Tetlock, 1983), a particularly influential version of this hypothesis conceives of conservatism as *motivated social cognition* (Jost, 2021). First articulated in a seminal meta-analysis spanning five decades of literature, this motivated social cognition account posits that political conservatism is a consequence of basic cognitive (i.e., pertaining to thinking, reasoning, or remembering) and motivational (i.e., the impetus that gives purpose or direction to behavior) processes concerning certainty/rigidity and safety/threat-sensitivity[1] (Jost et al., 2003). Under this account, people who have a motivational need to simplify reality may satisfy this need by adopting political ideologies that (promise to) foster a sense of order and predictability. Because conservatism ostensibly offers a sense of certainty by way of its support for prevailing social norms and hierarchies, rightists are disproportionately likely to be cognitively, ideologically, and motivationally rigid.

This version of the RRH has served as the frontline for much research within political psychology over the last two decades, stimulating a surge in studies of the psychological correlates (and theorized causes) of left- versus right-wing ideology (e.g., Dean, 2006; Hibbing et al., 2014; Inbar et al., 2009; Mooney, 2012; Oxley et al., 2008; Westen, 2007). This renaissance of theory and research has, in turn, prompted additional meta-analyses of the RRH, which have generally continued to provide strong evidence of positive correlations between rigidity and conservatism measures (e.g., Houck & Conway, 2019; Jost, 2017; Jost et al., 2003; Van Hiel et al., 2016). If taken at face value, these meta-analyses seem to clearly support the conclusion that rightists are more rigid than leftists.

But as foreshadowed above, existing evidence provides less conclusive support for the RRH than may seem at first blush. Hidden moderators, recurring methodological problems, and inconsistent conceptual foundations permeate the literature and raise challenges to the validity and generalizability of the RRH. From our point of view, these wide-ranging concerns and controversies can ultimately be understood as a function of heterogeneity in researchers' answers to two key questions: (a) *What is the right?* and (b) *What is rigidity?*

What Is "The Right"?

Political ideology is typically conceived in terms of a unidimensional left/liberal versus right/conservative political continuum. Generally speaking, the left pole is thought to reflect preferences for egalitarian social and economic change and cultural progressivism, and the right pole is thought to reflect preferences for maintaining social and economic hierarchy and traditional authority (e.g., Caprara & Vecchione, 2018; Johnston & Ollerenshaw, 2020). What this means is that political preferences characteristically regarded as liberal involve government economic intervention, redistributive policy, reduction of inequality, and progressive sexual morality and cultural positions. By contrast, those characteristics regarded as conservative involve favoring free-market economics, limited or

[1] Vis-à-vis safety and threat sensitivity, which we do not focus on in the present work, it is theorized that conservatism satisfies existential needs to preserve safety and security and to reduce danger and threat (Jost, 2017). However, as we elaborate upon in the Discussion section, we believe that much research supportive of this view suffers from the same methodological issues that we describe here (Malka et al., 2017), and that recent findings make this clear (e.g., Brandt et al., 2021; Crawford, 2017; Ollerenshaw & Johnston 2022).

no economic redistribution, tolerance of economic inequality, traditional sexual morality stances, and traditional cultural preferences.

More than anything, the left–right spectrum simplifies reasoning and communication about political preferences (Downs, 1957). Indeed, political conflict largely occurs along a left–right ideological divide in many Western nations (e.g., Benoit & Laver, 2006; Kitschelt et al., 2010; Knight, 1999; McCarty et al., 2016). That said, there are several drawbacks to relying on the left–right spectrum in research concerning the psychological causes and correlates of political ideology (Morgan & Wisneski, 2017). For one, it is not uncommon to hear someone volunteer that they are "socially conservative and economically liberal" or vice versa (Drutman, 2017). Corroborating this observation, factor analytic investigations tend to identify distinct social and economic dimensions of political conservatism (vs. liberalism) that seem to be moored in separate networks of psychological processes (e.g., Claessens et al., 2020; Costello & Lilienfeld, 2021; Duckitt & Sibley, 2009; Federico & Malka, 2018; Feldman & Johnston, 2014; Johnston et al., 2017; Laméris et al., 2018; Pan & Xu, 2018; see Johnston & Ollerenshaw, 2020, for a review). Were political conservatism a (roughly) coherent psychological entity, then we might anticipate social and economic conservatism to be inextricably bound together in most people's minds. Indeed, one popular instantiation of the RRH suggests that economic and social conservatism are psychologically intertwined precisely because both are rooted in rigidity (Azevedo et al., 2019).[2] By contrast, if "the right" is not any one thing, then the RRH (and by extension all models that seek to understand the psychological determinants of unidimensional conservatism) may commit a great error of oversimplification.

Multi–item measures of social and economic ideology are, indeed, highly correlated (e.g., *r*s > .50) within contemporary American samples (e.g., Azevedo et al., 2019), but there are many reasons to expect that this strong link is a product of circumstance and context, rather than a fundamental psychological concordance between the two dimensions. For instance, when one takes a global view by including representative samples from developing and non-Western countries, positive correlations between cultural and economic conservatism are uncommon (Malka et al., 2019). This dovetails with a prominent strain of thinking within political science that suggests most people do not naturally use left–right ideology in a coherent way (Kalmoe, 2020; Kinder & Kalmoe, 2017). Further, the positive correlation between social and economic conservatism in American samples has increased over the last two decades (Kozlowski & Murphy, 2021), which is consistent with the possibility that this strong link is a product of particular people, places, and/or times (Federico & Malka, 2022). For instance, politically engaged individuals are consistently more inclined to structure their social and economic attitudes on the right versus left dimension than politically disengaged individuals (Baldassarri & Goldberg, 2014; Kozlowski & Murphy, 2021), attesting to the role of top-down information environment influences (e.g., cues from elites) on political attitude structure.

Further complicating the picture, many studies show reliable correlations between social conservatism and rigidity indicators, yet relations between economic conservatism and rigidity indicators tend to be directionally inconsistent (e.g., Azevedo et al., 2019; Carl, 2014; Carney et al., 2008; Cizmar et al., 2014; Clifford et al., 2015; Costello & Lilienfeld, 2021; Everett, 2013; Feldman, 2013; Hibbing et al., 2014; Johnson & Tamney, 2001; Kossowska & Van Hiel, 2003; Malka et al., 2014; Sterling et al., 2016; Van Hiel et al., 2004; Yılmaz et al., 2016). Accumulating data suggest that rigidity-related constructs are correlated with left-wing economic preferences among people whose political preferences are not subject to strong environmental pressures, perhaps because government economic intervention is likely to provide security and certainty (Czarnek & Kossowska, 2021; Johnston et al., 2017; Malka et al., 2014; Ollerenshaw & Johnston, 2022). By contrast, rigidity-related constructs are correlated with right-wing economics in the United States and Britain, perhaps because free-market economics are branded as "conservative" in American political discourse. These findings further testify to the possibility that social and economic ideology are not *psychologically* intertwined (e.g., via the bottom-up influence of rigidity) but can be bound together via top-down environmental influences (Federico & Malka, 2018; Layman & Carsey, 2002; Noel, 2014; Zaller, 1992).

Altogether, whether and to what extent "conservatives" are rigid may depend crucially on *how* conservatism is defined and operationalized, *where* the data are collected, and *who* populates the sample. And, indeed, a review of the literature reveals that political ideology is measured in a wide variety of ways. Whereas some researchers use assessments of concrete policy preferences (e.g., Carmines et al., 2012; Everett, 2013), others use psychologically expansive measures premised on theoretical models that posit core orientations underlying ideology (e.g., opposition to vs. acceptance of change, Right-wing Authoritarianism [RWA]/Social Dominance Orientation [SDO]; see Duckitt & Sibley, 2009; Thorisdottir et al., 2007), and others use single-item indicators of partisan or ideological identity (e.g., Federico & Goren, 2009). Meanwhile, sampling practices may be overly narrow. Despite being politically atypical, American samples are vastly overrepresented in tests of the RRH (e.g., American samples represent 59% of all observations in the present review), potentially artificially obscuring psychological differences across social and economic ideology that manifest in most non-American national contexts (Johnston & Wronski, 2015; Malka & Soto, 2015; Malka et al., 2014, 2017). By the same token, demographically representative samples, which may contain a larger proportion of people free from top-down, discursive pressures on ideological preferences, represent a small fraction of tests of the RRH (roughly 8%, by our estimate)—potentially upwardly biasing population estimates for the RRH (Mercer et al., 2017; Xie et al., 2012). Thus, it is important for meta-analytic reviews of the RRH to distinguish between (a) social and economic political ideology, (b) types of political ideology measures, and (c) sampling contexts, and to examine these domains as potential sources of heterogeneity. Ours is the first to do so.

What Is "Rigidity"?

Much like the commonplace practice of collapsing social and economic ideology into a single category (or not measuring them separately at all), prior tests of the RRH have tended to subsume a host of loosely interrelated variables under the broad heading of

[2] Indeed, hypothesized mechanisms underlying the RRH draw from conceptual connections between the *shared* epistemic qualities of social and economic conservatism (e.g., upholding prevailing norms and hierarchies), on the one hand, and rigidity, on the other.

rigidity. Scholars supportive (Hibbing et al., 2014; Jost, 2021) and critical (Johnston et al., 2017; Malka & Soto, 2015) of the RRH have followed this convention,[3] perhaps because little scholarly consensus exists concerning the precise boundaries of rigidity (Furnham & Marks, 2013; Sternberg & Grigorenko, 1997; Zmigrod et al., 2020). Indeed, there are few systematic accounts of conceptual distinctions across variables typically thought to reflect rigidity, let alone empirical evidence to guide the construction of valid and reliable rigidity dimensions. One recent review (Cherry et al., 2021) of the cognitive rigidity literature identified 25 competing conceptualizations assessed across 23 measures. If these constructs are only loosely coupled, which appears plausible given their definitional heterogeneity, they are unlikely to share specific psychological mechanisms linking them to political conservatism.

For this reason, how best to meta-analytically compare (or disaggregate) rigidity constructs remains a matter of open debate (see, e.g., Cherry et al., 2021, for a review; Kipnis, 1997). Several taxonomies of distinctions within rigidity constructs, however, have emerged in recent years (e.g., executive functioning, intolerance of ambiguity, inflexible thinking styles, cognitive complexity; Lauriola et al., 2016; Newton et al., 2021; Stoycheva et al., 2020; Woznyj et al., 2020), providing some basis for distinguishing between rigidity variables in a theoretically informed manner. Based on these provisional taxonomies of rigidity dimensions, we have identified four domains of rigidity that are differentiable in their relations with one another and relevant external criteria (see Supplemental Figure 1): (a) *rigid thinking styles,* (b) *motivational rigidity,* (c) *cognitive inflexibility,* and (d) *ideological rigidity (i.e., dogmatism).*

These four domains, as we discuss below, have little definitional overlap, are not strongly correlated, and tend to be studied in disparate subfields. We suspect that this schema offers an empirically informed and useful means of resolving "the lumper-splitter problem" (i.e., balancing precision and parsimony when placing individual cases into categories; Simpson, 1945) in the absence of an empirically derived taxonomy of rigidity.

Rigid Thinking Styles

Theoretical accounts of human decision-making often distinguish between *intuitive* (i.e., rapid, unconscious, and automatic) and *reflective* (i.e., slow, conscious, and deliberative) cognitive processes (Kahneman, 2011). Dozens of studies have found that individuals vary in *cognitive reflectivity*, and that these individual differences have broad patterns of relevance to myriad behaviors and attitudes (e.g., Toplak et al., 2011; see Pennycook et al., 2015). Drawing from the RRH literature, several authors have suggested that conservatives may be more intuitive (i.e., less analytic) thinkers than liberals (Talhelm et al., 2015; cf. Kahan, 2012). Nevertheless, common operationalizations of cognitive reflectivity, such as the cognitive reflection test and the Need for Cognition Scale (Cacioppo & Petty, 1982), are negligibly related to measures of other rigidity constructs that have been used in tests of the RRH (e.g., need for closure, intolerance of ambiguity, and dogmatism; Newton et al., 2021). We therefore treat rigid thinking styles as a distinct rigidity domain.

Motivational Rigidity

As with rigid thinking styles, motivational rigidity is not highly correlated with other rigidity domains, suggesting that it may bear unique or divergent associations with political ideology (Lauriola et al., 2016). Many such motives are subsumed by *need for cognitive closure*, a widely known construct that broadly reflects ambiguity aversion and desires for clear answers (Kruglanski & Webster, 1996). Many tests of the RRH have revealed a relation between need for cognitive closure (and related motivational needs) and conservatism indicators (see Federico & Goren, 2009). Other constructs potentially indicative of need for certainty, such as risk aversion and cognitive ability, also exhibit modest relations with elements of conservatism (Kam, 2012; Kemmelmeier, 2008). For our primary analyses, we presently collapse motivational rigidity variables, such as need for closure and the motivational elements of ambiguity intolerance.

Cognitive Inflexibility

Cognitive inflexibility can be understood as part of a broader suite of psychological processes involved in executive functioning, which refers to high-level cognitive control functions that are involved in complex mental processes, such as planning, focusing attention, working memory, and multitasking (Diamond, 2013; Miyake & Friedman, 2012). Specifically, cognitive inflexibility is thought to reflect an inability to change perspectives, shift approaches efficiently, and take advantage of unexpected opportunities (Cools & Robbins, 2004). Drawing from the RRH literature, neuropsychological, and behavioral measures of cognitive inflexibility have been leveraged to suggest that leftists and rightists may differ in their basic cognitive architecture (e.g., Buechner et al., 2021; Sidanius, 1978; Zmigrod, 2020). To our knowledge, no systematic data are publicly available concerning the convergence between these cognitive inflexibility measures and measures of other rigidity constructs (e.g., motivations, intuitive thinking, and dogmatism). Moreover, cognitive inflexibility and other rigidity constructs massively diverge in their relations with external criteria (Lauriola et al., 2016; Stoycheva et al., 2020). We therefore distinguish cognitive inflexibility from other rigidity domains.

Dogmatism

The storied construct of dogmatism has variously been defined as generalized authoritarianism (Rokeach, 1960) and, later, as "relatively unchangeable, unjustified certainty" (Altemeyer, 1996, p. 201). Factor analytic investigations have indicated dogmatism is relatively unidimensional and manifests positive

[3] Often, rather than referring to "rigidity" per se, scholars refer to the psychological orientations that may underlie conservatism as "uncertainty intolerance and threat sensitivity" (e.g., Jost et al., 2003), "needs for security and certainty" (Malka et al., 2014), or an "open versus. closed" personality superfactor (Johnston et al., 2017). Indicators of these orientations have included measures focused on fear of death, perceptions of various threats, reversed openness to experience (or facets thereof), conscientiousness (or facets thereof), and values having to do with obedience, conformity, and religiosity (which also, tautologically, often appear in measures of conservatism themselves; e.g., Jost et al., 2007; Johnston & Wronski, 2015; Federico & Malka, 2018). Although these constructs clearly bear theoretical and empirical relations with rigidity, they are only indirectly relevant to rigidity. In addition, recent evidence suggests that in certain contexts where conservatives and liberals are polarized, one's political orientation might motivate one to adopt or present oneself as having what are thought to be ideology-consistent levels of these constructs (Bakker et al., 2021; Ludeke et al., 2016; Margolis, 2018).

correlations with theoretically relevant variables, including belief in certain knowledge, resistance to belief change, closed-mindedness, need for cognition, need for structure, and need to evaluate (Altemeyer, 2002; Crowson, 2009; Crowson et al., 2008). Still, dogmatism is conceptually and empirically distinct from these and other rigidity constructs (Duckitt, 2009; Johnson, 2009; see Rönkkö & Cho, 2022). We therefore treat dogmatism as a stand-alone rigidity domain in the present review.

Circular Measurement: Some Measures of Conservatism Directly Measure Rigidity

Thus far, we have predominantly focused on conceptual and taxonomic reasons that the RRH's evidentiary basis may be less clear-cut than previously thought. Namely, shaky conceptual foundations and a paucity of consensus concerning the nature and boundaries of both "the right" and "rigidity" raise the specter of hidden moderators that are as or more explanatorily relevant than main effects. Yet, theoretical acuity and methodological validity are deeply intertwined. Most critical theoretical obstacles to useful meta-analytic tests of the RRH manifest, in practice, as methodological choices (e.g., measuring ideology as a single dimension).

Perhaps no methodological obstacle in the RRH literature illustrates this dynamic better than criterion contamination (Cronbach & Meehl, 1955; Messick, 1995) in measures of conservatism and rigidity (Malka et al., 2017). Specifically, a large proportion of studies reviewed in prior meta-analyses have used measures of "conservatism" that rest on the theoretical assumption that conservatism is heavily imbued with rigidity or associated nonpolitical content. These measures—which include the Fascism Scale (e.g., Adorno et al., 1950), the RWA Scale (e.g., Altemeyer, 1996), and the Wilson–Patterson Conservatism Scale (e.g., Wilson & Patterson, 1968)—were designed to assess rigidity and conservatism simultaneously (e.g., Wilson, 1973). For instance, the Fascism Scale assesses unquestioned faith in a supernatural power and a critical view of bad manners, the Wilson–Patterson Conservatism Scale includes nonpolitical items that are intended to assess uncertainty avoidance (e.g., dislike of jazz music), and the RWA Scale includes content pertaining to religiosity, aggression, and obsequious deference to authority (Duckitt et al., 2010). Other "conservatism" measures used in RRH research include content pertaining to parent–child relationships, ethnocentrism, dogmatism, basic motivational values reflecting self-enhancement and intolerance of change, religiosity, and political intolerance (see Malka et al., 2017). These imprecise and criterion-contaminated historical measurement practices pose an obstacle to meta-analytic tests of the RRH because, until recently, studies relying on said measures made up a majority of the RRH literature (Costello, Clark, et al., 2022).

Just as publication bias (e.g., "file drawer" effects) and questionable research practices have been shown to systematically distort meta-analytic findings (Rosenthal, 1979; Thornton & Lee, 2000), the presence of rigidity-related content in political ideology measures may yield exaggerated meta-analytic results.[4] Hence, in the present review, we examine the degree to which biased measures inflate effect sizes and estimate rigidity-conservatism relations in a way that is less distorted by content overlap.

Beside the Point Estimate: The Central Role of Heterogeneity

Given the vast range of constructs, measures, and environments that scholars have used to test the RRH, it is perhaps unsurprising that point estimates for conservatism-rigidity correlations reported in peer-reviewed articles range from $r = -.58$ (Durrheim, 1998) to $r = .82$ (Pettigrew, 1958). Attempting to interpret an "overall" effect size estimate for the core psychological mechanism(s) ostensibly underlying such a vast range of effects glosses over the more difficult and, arguably, more interesting questions of *when* and *why* these effects vary. Addressing these questions will entail mapping the substantive variation in true effect sizes across the RRH literature, which is perhaps the chief insight provided by meta-analysis (Higgins & Thompson, 2002).

Thus, point estimates of main effects are only one piece of the puzzle in the present meta-analysis. To illustrate the importance of this distinction (see Wiernik et al., 2017), suppose that we find that the relation between "conservatism" and "rigidity" (e.g., $r = .15$) is half as large as the standard deviation of true effects across all studies (e.g., $SD_r = .30$). Roughly speaking, this would suggest that many samples in the literature reflect a modest conservatism-rigidity correlation, yet in a substantial minority of samples the true relation between conservatism and rigidity is either considerably larger (e.g., $r > .45$) or directionally opposing (e.g., $r < -.15$). By contrast, suppose that the overall relation (e.g., $r = .15$) was twice as large as the standard deviation of true effects (e.g., $SD_r = .075$). This would suggest that a modest positive correlation characterized rigidity-conservatism relations, regardless of where, how, and with whom a given study was conducted. In both cases, however, merely reporting the overall point estimate would not enable readers to draw informed conclusions about the meaning of the RRH literature. Rather, heterogeneity estimates and point estimates should interdependently inform interpretations of meta-analytic findings. Accordingly, we adopt such an interdependent approach in the present review, focally emphasizing estimates of substantive heterogeneity and boundary conditions alongside main effects.

The Present Review

We meta-analytically examine the full body of currently available literature (including peer-reviewed journal articles, doctoral dissertations, master's theses, books, and unpublished data) with the dual aims of probing the RRH's basic assumptions and parsing the RRH literature's considerable heterogeneity. We leverage divergent conceptualizations and measures of political ideology and rigidity to

[4] Content overlap such as this has also taken the form of inclusion of political content in measures of rigidity-related constructs (Malka et al., 2017, pp. 121–122). For example, manipulations and measures relevant to perception of terrorism-related threats are often found to predict conservatism and are consequently taken as support for the RRH (Jost et al., 2007, Study 3; Thórisdóttir & Jost, 2011, Study 2). Further, many studies rely on Rokeach's Dogmatism (D) scale as a rigidity indicator, despite the presence of right-wing political content in this scale (see Conway et al., 2016). Similarly, the long-standing finding that political conservatism is associated with prejudice (see Hodson & Dhont, 2015, for a review) appears to dissipate when groups that are perceived as ideologically dissimilar to political liberals, such as Christian fundamentalists and wealthy individuals are included as targets in measures of prejudice (Brandt & Crawford, 2019; Crawford, 2017).

facilitate these tests, allowing us to clarify the coherence and utility of approaching political ideology and rigidity as unidimensional constructs in the context of the RRH. Further, we examine methodological and conceptual obstacles to substantive tests of the RRH, such as publication bias, hidden moderators (e.g., sample type, nationality, WEIRDness, rigidity measure type, political ideology measure type) and criterion contamination in ideology and rigidity measures. Relative to previous reviews, the current meta-analysis is considerably larger and broader in the number of samples, effect sizes, and participants. What is more, our meta-analysis is the first review of the RRH to statistically model dependencies among effect sizes extracted from the same samples (Van den Noortgate et al., 2015).

Method

Transparency and Openness

Supporting materials for this article, including raw data and analytic code, are openly accessible at https://osf.io/uqexj/.

Literature Search

Studies were obtained using several search strategies (updated a final time in January of 2021). First, we conducted targeted searches of online databases (i.e., ProQuest Dissertations & Theses, PsycINFO, Google Scholar, and the Emory University Libraries search tool, discoverE, which comprises 18 relevant databases). The search terms were developed by the first author and were based on our review of the literature.[5] Searches covered English-language articles, books, master's theses, and dissertations published from 1950 to 2021. Second, we drew from published and unpublished studies included in previous meta-analyses of the RRH. Third, we employed a snowballing procedure that entailed reviewing lists of studies that have cited widely used measures of political ideology and rigidity. Finally, we searched publicly and privately available data sets (e.g., YourMorals.org) to manually calculate effect sizes of interest.

Our initial database search yielded 1,416 studies, and abstracts of these studies were then screened for initial inclusion. A total of 489 studies were deemed appropriate for full-text review; removing duplicates reduced this number to 371. The remaining full texts were read by the first author. For a study to be included, it needed to meet all the following criteria: (a) assessment of one or more of the rigidity constructs of interest; (b) an assessment of political ideology (e.g., symbolic self-placement, support for conservative/liberal policies, party identification, support for conservative/liberal values, vote choice, or some combination thereof); and (c) sufficient data provided for calculating individual effect sizes. Effect sizes that were either observed following an experimental manipulation or reported alongside statistically significant covariates (e.g., β weights from multiple regression analyses) were excluded. No studies were excluded based on participant characteristics (e.g., age, ethnicity).

A total of 140 articles met inclusion criteria and were coded (see Figure 1). Five open data sets that met inclusion criteria were also identified and used to calculate effect sizes. A final round of searching was conducted in January 2021, which resulted in the addition of seven studies. After completing our initial literature review, we expanded our study pool to include any effect sizes from the most comprehensive previous meta-analytic review of the RRH (i.e., Jost et al., 2017) that involved political ideology measures including overt prejudice, authoritarianism, or rigidity content. An additional 102 effect sizes and 6,275 participants were added. Secondary analyses were conducted to facilitate the comparison of our results before and after excluding these effects sizes, affording the opportunity to meta-analytically examine the differences between proxy measures of conservatism and "purer" measures of conservatism.

Twenty-five percent of studies were randomly selected and independently reviewed and coded by the second author to assess reliability of study coding. Interrater reliability coefficients (i.e., κ for categorical variables and intraclass correlation coefficient for continuous variables) are provided below. Coding disagreements were resolved by discussion. An overview of included citations, study characteristics, and effect sizes is provided in Supplemental Table S1, and the full meta-analytic data set is provided at https://osf.io/uqexj/.

Data Coding

Domain of Political Ideology

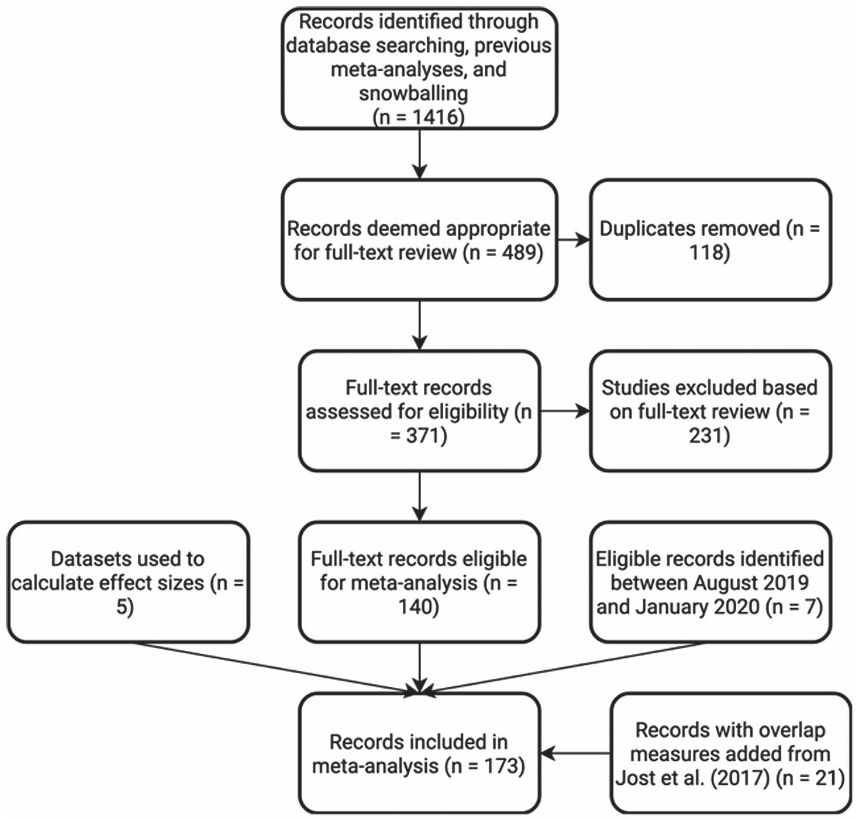
Measures of political ideology were coded as belonging to one of three categories: general ideology, social ideology, or economic ideology. General ideology, which comprised the largest proportion of observations, included generic self-placement items, party affiliation or membership, vote history or preference, and self-report scales that contain both social and economic content but report only a single score (e.g., the Political-Economic Conservatism Scale). Social ideology was measured by self-placement items; self-report measures that rely heavily on content related to the endorsement of traditional values, social rules, and norms (vs. progressive values, rules, and norms); self-report measures that yield a social ideology subscale; and policy preferences for issues related to social ideology (e.g., abortion rights or gay marriage). Finally, economic ideology was assessed in the same manner as social ideology, but with measures and policies that focus on government involvement in private enterprise, redistribution of wealth, and/or the economic choices available to its citizens. Interrater agreement was substantial, κ = .90.

Domain of Rigidity

When coding each observation, we used a two-pronged approach. First, we examined the rigidity constructs individually, coding them based on study authors' designations wherever possible. For instance, if the authors indicated that they had created a composite self-report measure of dogmatism, we coded said measure as "dogmatism." When this was not possible, we relied on the fact that many of the varieties of rigidity used in the current review are tied to "trademark" measures that are most frequently used to operationalize them. For instance, motivations for certainty are typically assessed with the Need for Closure Scale (Kruglanski et al., 1993) and cognitive reflection is typically assessed with the cognitive reflection test (Frederick, 2005). Hence, between the authors' stated designations and this heuristic, most studies in our pool could be categorized straightforwardly. Second, observations were independently coded

[5] Search terms were entered as variations of the following Boolean phrase: "(political AND (orientation OR ideology OR conservatism OR attitudes)) AND (cognitive reflection OR dogmatism OR need for cog* OR need for closure OR rigidity OR flexibility OR inflexibility OR executive function* OR motiv* OR intolerance of ambiguity)".

Figure 1
Flowchart of the Screening Process



Note. The term "record" refers to a discrete source of data (e.g., a study, which may contain many effect sizes, or a data set from which effect sizes can be calculated).

as reflecting the broad categories of rigid thinking styles, motivational rigidity, dogmatic certitude, or cognitive inflexibility (i.e., using the taxonomic scheme outlined in the Introduction section).

Content Overlap

Judgments concerning whether measures of political ideology are marked by content overlap were initially made by the first author based on a careful reading of each measure (and assessed using a small online community sample; see online Supplemental Materials). We then constructed a dummy-coded moderator variable for overlap versus no overlap. The following measures were categorized as containing content overlap: the original C-Scale (e.g., Kirton, 1978), all versions of the F-Scale (e.g., Davids, 1955; Kohn, 1974), all versions of the RWA Scale (e.g., Crowson et al., 2005), all versions of the SDO scale (e.g., Leone & Chirumbolo, 2008), all versions of the System Justification Scale (e.g., Hennes et al., 2012), the Personal Conservatism Scale (e.g., Olcaysoy & Saribay, 2014), and all ad hoc measures that borrowed items from the aforementioned measures.

Sample Characteristics

For each sample, we extracted nationality ($\kappa = .98$) and participant composition (e.g., university students, nonrepresentative internet-recruited, community, nationally representative, government officials; $\kappa = .76$).

WEIRDness

We followed procedures described in the Many Labs 2 project (i.e., Klein et al., 2018; see also Yilmaz & Alper, 2019) to quantify sample WEIRDness via the sample country of origin (see https://osf.io/b7qrt/for/ more detailed information).

Measure of Political Ideology

We coded the political ideology measure used for each observation as a categorical moderator using both broad and narrow coding strategies. Individual measures with $k > 2$ were coded as an individual category. Further, the following specific categories were used: symbolic self-placement (e.g., "on a scale from 1 to 7, how left-wing vs. right-wing are you?"), support for liberal versus conservative issues/policies (e.g., opposition to abortion or raising taxes on the wealthy), having voted for a left-wing or right-wing political party, membership in a left-wing or right-wing political party, ad hoc measures (i.e., designed for purposes of a single study), composites (i.e., a combination of multiple measure types), unspecified self-report (i.e., studies that noted that a self-report measure of ideology was used but did not name it or provide items), and other unspecified (i.e., all other cases where the authors left their measure of ideology unspecified). Including these categories, a total of 23 categories with $k > 2$ were present.

Self-Report Versus Performance-Based Measures

Effect sizes derived from self-report rigidity measures were coded as such (i.e., *self-report*), whereas effect sizes derived from behavioral and/or objectively scored measures were coded as *performance-based* ($\kappa = .89$).

Statistical Analyses

All extracted effect sizes were transformed into Fisher's z (Cohen et al., 2014) to account for the slight negative bias in Pearson's r (Card, 2012), and weighted according to the inverse of their variance (i.e., sampling error), such that larger samples contributed more to the aggregate effect size estimate than smaller ones (Lipsey & Wilson, 2001). We used the *metafor* package (Viechtbauer, 2010) in *R* (Version 4.2.1) to conduct all analyses. The *R* code used to generate our results is provided on open science framework.

The Three-Level Model

To account for dependencies across effect sizes, and particularly for correlated sampling errors due to multiple effect sizes drawn from the same sample, we used a three-level meta-analytic approach with restricted maximum likelihood estimation. In contrast to the traditional (two level) random effects model, in which effect sizes are assumed to vary due to sampling variance and systematic variance between studies, the three-level model also accounts for systematic variance across outcomes from the same sample. Using this approach, we modeled the sampling variance for each effect size (Level-1), variation across outcomes within each sample (Level-2), and variation across samples (Level-3). Although such multilevel models are said to require that residuals at each level are independent, Van den Noortgate et al. (2013) demonstrated in simulation studies that the three-level approach successfully handles dependencies due to correlated sampling errors, resulting in accurate standard errors and point estimates (see also Van Den Noortgate & Onghena, 2003; Van den Noortgate et al., 2015). We chose to use three-level meta-analysis because, unlike most other statistical techniques for handling correlated sampling errors (e.g., multivariate meta-analysis with robust estimation), the three-level approach does not require that correlations among reported outcomes be known.

Heterogeneity

We report several indices of heterogeneity. First, H^2 (Higgins & Thompson, 2002), which represents the difference between the ratio of the observed variance (i.e., Cochran's Q) and the expected degree of variance due to sampling error. Higgins and Thompson (2002) suggest that $H^2 = 1$ indicates that the population of studies is homogeneous, whereas $H^2 > 1.5$ indicates that substantial heterogeneity is present. Second, we report $I^2_{(2)}$ and $I^2_{(3)}$, which describe residual variance relative to the total variance (i.e., variance in true effects plus sampling variance) between-samples and within-samples, respectively. I^2 indicates, in other words, the percentage of total variance not caused by sampling error. Third, we report σ_1^2 and σ_2^2, which describe the variance of the effect sizes in our meta-analytic data set (within- and between-samples, respectively). Fourth, we report the standard deviation of the true effect sizes, σ, which is computed as $\sqrt{\sigma_1^2 + \sigma_2^2}$. Given that σ is on the same scale as the meta-analytic effect size, r, it serves as an easily interpretable metric of substantive heterogeneity (with r and σ being comparable to a mean and standard deviation). Fifth and finally, we report 95% prediction intervals for each estimated effect—the interval within which the effect size of a novel study would fall if said study was selected randomly from the same population as the meta-analytic study pool. Correctly interpreting prediction intervals depends not only on their width, but on the range of correlations that they span (e.g., a prediction interval with endpoints of $r = .50$ and $r = .85$ would always reflect a very large true effect, whereas an equally wide interval with endpoints of $r = -.05$ to $r = .30$ would indicate theoretically meaningful variability; see Wiernik et al., 2017).

Meta-Analytic Models

It is unclear whether either the different types of rigidity or the various domains of conservatism should be conceptualized as comprising two larger constructs. As a means of engaging with this problem, we used the following nested analytic approach.

First, we estimated an overall model (Glass, 2015), collapsing across rigidity constructs and types of conservatism to yield an overall meta-analytic evaluation. Second, we conducted subgroup analyses for each political ideology and rigidity variable across all classification schemes. We then estimated meta-regression models with categorical moderators for these classifications (e.g., social vs. general vs. economic ideology), which we evaluated with omnibus tests of the null hypothesis that all levels of the moderator are equal to zero simultaneously. Finally, we estimated a "full" multiple meta-regression model by simultaneously regressing effect sizes on categorical moderators for rigidity domain and political ideology domain, which we then extended to additional moderators of interest, such as publication status, sample type, author allegiance, and so on. Continuous moderators were mean centered to facilitate interpretation. This produced predicted values for each of the four rigidity domains at the reference level of each moderator, as well as effect size estimates for each nonreference level of each moderator (i.e., how much the predicted values for each rigidity domain would change if the reference level for a given moderator changed). We employed the Knapp and Hartung (2003) adjustment to standard Wald-type tests, which allow for better control of Type I error rate (i.e., tests of sets of model coefficients were F tests).

We interpreted moderators with significant omnibus tests based on (a) t tests of the differences between each level of the moderator and (b) point estimates and confidence intervals of each conservatism-construct coefficient at a reference level of the moderator in question. Still, these models, which include only main effects, carry the assumption that the influence of multiple factors is additive (i.e., that differences between levels of each moderator do not vary across levels of the other moderator[s]).

Publication Bias

To initially investigate reporting and/or publication bias, we created two contour-enhanced funnel plots visualizing (a) the distribution of all effect sizes against their precision (1/*SE*), including the variance from each level of the three-level model, with the reference line set at the estimated overall effect size, and (b) the distribution of internally standardized residuals (i.e., observed residuals in the full model divided by their corresponding standard errors) after accounting for rigidity construct and conservatism type. Next, to further probe, and potentially correct for, asymmetry in the effect size distribution while maintaining the three-level model, we entered either the standard error or variance for each observed effect size into each model as an additional predictor (i.e., moderator). This approach can be considered closely equivalent to the precision-effect test and precision-effect estimate with standard errors method (Lehtonen et al., 2018). Finally, as an additional and more direct means of assessing publication bias, we examined the degree to which published versus unpublished studies influenced the full model via fixed-effects moderator analyses.

Results

The final data set comprised 708 observations, 329 samples, and 173 studies (unique $N = 187{,}612$; individual sample sizes ranged from $n = 12$ to $n = 18{,}817$). Figure 2 depicts the number of effect sizes for each construct, segmented by the frequency of each political ideology type within each construct (see also the full data set provided in online Supplemental Materials). Supplemental Tables S2 and S3 present the number of effect sizes at each level of each categorical moderator and descriptive statistics for continuous moderators.[6] Unless stated otherwise, all results are reported with content overlap effect sizes ($N = 139$) removed, but we report sibling analyses using all effect sizes (i.e., including content overlap) in the online Supplemental Materials. Further, because political orientation is typically assessed using bipolar measures (i.e., with liberalism on one end and conservatism on the other), observations are coded such that positive meta-analytic correlations indicate a positive correlation between conservatism and rigidity.

Model 1: Global Result

Our overall analysis[7] indicated a small statistical association between rigidity and political conservatism, $r = .133$, 95% CI [.12, 15].[8] Importantly, a considerable degree of heterogeneity was present in the model, $Q(565) = 4{,}361$, $p < .001$; $\sigma_1^2 = .005$ and $\sigma_2^2 = .012$; $H^2 = 6.71$; $I^2_{(2)} = 66\%$ and $I^2_{(3)} = 25\%$. In absolute terms, this indicates that the standard deviation in true effects from one study to the next (i.e., σ) is .13, or roughly as large as the overall effect size estimate. The 95% prediction interval was −.12 to .30, which may explain the field's long-standing difficulty arbitrating between proponents and opponents of the RRH: the empirical distribution of true effects in the literature extends well beyond zero in the negative direction at one endpoint yet includes moderate-to-large positive effects at the other endpoint. Accordingly, moderating variables are likely to have a strong impact. As indicated by the I^2 values, the degree of substantive heterogeneity in Level 2 (i.e., across observations drawn from the same sample) was roughly 2.5 times greater than that accounted for by variance in Level 3 (i.e., observations drawn from different samples). Thus, we can broadly expect moderators that tend to occur within samples (e.g., multiple operationalizations of ideology and/or rigidity) to be more explanatorily powerful than that tend to occur across samples (e.g., sample-type, nationality) in the global model.

Model 2: The Multidimensionality of Political Ideology

We adapted the three-level model by dropping the intercept and regressing the observed effect sizes on a set of dummy-coded variables for economic ideology, social ideology, and general ideology, respectively. An omnibus test for moderation was statistically significant, $F(3, 563) = 141.17$, $p < .001$. Residual variance was modestly reduced but not eliminated, Q_E (563) = 3,597, $p < .001$; $\sigma_1^2 = .006$ and $\sigma_2^2 = .009$; $H^2 = 5.35$; $I^2_{(2)} = 55\%$ and $I^2_{(3)} = 34\%$, such that $\sigma = .12$. Table 1 presents estimated effect sizes, alongside 95% confidence intervals, 95% prediction intervals, *k*s, *N*s, *p* values, and within-construct heterogeneity statistics.

Correlational point estimates for all three types of ideology significantly and positively deviated from both zero and one another.[9] More specifically, economic conservatism was less strongly related to rigidity than general conservatism ($t = 5.43$, $p < .001$), which manifested a small-to-moderate positive relation with rigidity, and social conservatism ($t = 9.37$, $p < .001$), which manifested a moderate positive relation with rigidity; general conservatism was less strongly related to rigidity than was social conservatism ($t = 4.61$, $p < .001$).

For economic ideology, σ was twice as large as β, with a prediction interval reflecting, on one pole, a moderately sized negative effect and, on the other pole, a moderate-to-large positive effect. For general ideology, σ was slightly smaller than β, reflecting a prediction interval with a small-to-moderate negative endpoint and a large positive endpoint. Finally, for social ideology, σ was much smaller than β; but, given the large correlational point estimate, the absolute degree of heterogeneity was roughly similar to that for economic and general ideology. Accordingly, the prediction interval endpoints for social ideology ranged from small, in the negative direction, to exceptionally large in the positive direction. For both social and economic ideology, most of this residual variance was explained by differences within rather than across studies, whereas the opposite was the case for general ideology—perhaps speaking to the greater specificity of the former two ideology-types (i.e., measures of general

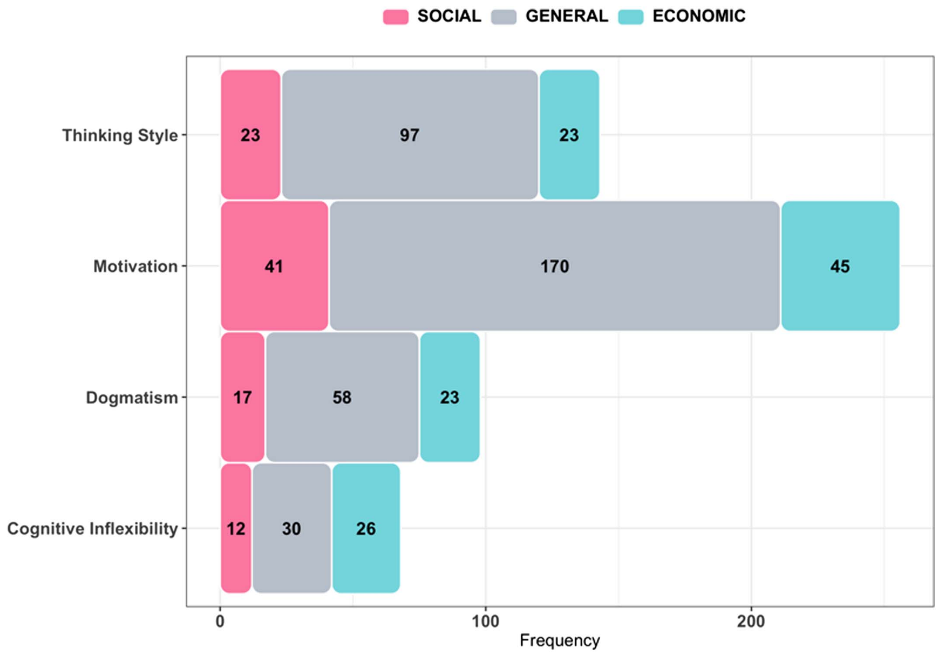
[6] To account for the possibility of outlying observations distorting our conclusions, we removed observations with standardized residuals that deviated from the expected asymptotic distribution. This procedure was done iteratively at both the 95% and 99% confidence levels (visualized with the white and grey areas, respectively, of Supplemental Figure 2). Forty-four observations (7.2%) were removed for $p < .05$ and 18 observations (2.9%) were removed at $p < .01$. Together, the three pools of studies (i.e., raw, trimmed at $p < .05$, and trimmed at $p < .01$) allowed for sensitivity analyses, although the raw pool of studies remained our primary object of analysis.

[7] A three-level model (i.e., with random effects at the sample level) was better fitting than the traditional, two-level model (ΔBIC = 35.25). We proceeded to compute a four-level model (i.e., random effects for each study, each sample within each study, and each effect size), which incremented the three-level model in terms of fit (ΔBIC = 103.90). Nevertheless, the third level of this four-level model did not account for a significant degree of residual heterogeneity, so we next compared the four-level model with a three-level model with random effects for each study and each effect size within each study (i.e., we removed the sample level), which modestly incremented the fit of the four-level model (ΔBIC = 2.00). Hence, we proceeded with this final three-level model.

[8] Results were reduced only slightly (differences in $r < .01$) when outliers were removed at either the 99th and 95th percentile; subsequent analyses were based on the full data set.

[9] Results were effectively unchanged after removing outliers (change in $\beta < .02$ for all three outcomes).

Figure 2
Number of Effect Sizes for Each Rigidity Domain and Political Ideology Dimension



Note. See the online article for the color version of this figure.

ideology are more heterogeneous than measures of economic or social ideology).

Model 3: Rigidity Domains

We next sought to clarify the relations between individual rigidity domains and political ideology, adapting the three-level model by dropping the intercept and regressing the observed effect sizes on a set of nominal variables for each of the four rigidity domains. An omnibus test suggested the presence of moderation, $F(4, 561) = 128.83$, $p < .001$. Relative to the overall model (Model 1), the residual variance was reduced somewhat but not eliminated, $Q_E\ (561) = 3{,}246$, $p < .001$; $H^2 = 4.75$; $\sigma_1^2 = .002$ and $\sigma_2^2 = .011$; $I^2_{(2)} = 74\%$ and $I^2_{(3)} = 14\%$, with $\sigma = .11$ and roughly five times more substantive heterogeneity being attributable to within-sample differences than to between-sample differences.

All rigidity constructs were statistically significantly related to political conservatism (see Table 1), yet main effect sizes were uniformly and, in some cases, substantially smaller than previously reported estimates (e.g., the most recent prior meta-analytic estimate for cognitive inflexibility was $r = .38$, see Jost, 2017; our estimate was $\beta = .07$). Cognitive inflexibility and rigid thinking did not significantly differ from one another ($t = 0.17$, $p = .865$), demonstrating small effects (i.e., βs < .07); motivational rigidity manifested a small-to-modest positive association with political conservatism ($\beta = .15$); and dogmatism manifested a moderately

Table 1
Meta-Analytic Results for Rigidity Domain and Ideology Domain

Focal moderator	k	n	β	95% CI	σ	95% PI	H^2	$I^2_{(2)}$	$I^2_{(3)}$
Ideology domain									
Economic	117	50,750	.055	[.03, .08]	.11	−.17, .28	4.22	56%	31%
General	356	119,063	.136	[.12, .15]	.12	−.11, .37	5.19	34%	54%
Social	93	54,224	.208	[.18., .24]	.13	−.04, .47	7.43	71%	21%
Rigidity domain									
Cognitive inflexibility	68	7,926	.073	[.03, .11]	.14	−.20, .36	2.35	34%	48%
Motivational rigidity	256	50,507	.146	[.13, .17]	.12	−.08, .38	4.31	80%	8%
Rigid thinking style	144	86,410	.069	[.05, .09]	.08	−.09, .23	4.84	63%	24%
Dogmatic certitude	98	27,666	.222	[.19, .25]	.15	−.09, .52	7.40	44%	48%

Note. k = observations; n = unique participants; σ = standard deviation in true effects between observations in a subgroup analysis; β = meta-regression coefficient in a model with the categorical moderator of either ideology domain or rigidity domain; 95% PI = prediction interval (range within which the true effect size of a new study would fall if selected at random from the meta-analytic population); 95% CI = confidence interval.

sized positive association with conservatism ($\beta = .22$). Motivational rigidity manifested a significantly larger effect than both cognitive inflexibility ($t = 3.63$, $p < .001$) and rigid thinking ($t = 5.74$, $p < .001$); dogmatism manifested a larger relation than all other domains (*t*s from 4.11 to 8.47, *p*s < .001).

Nevertheless, these main effects were rivaled in magnitude by the degree of substantive heterogeneity within each rigidity domain. All prediction intervals crossed zero and σs ranged from .08 (thinking style) to .15 (dogmatism). Prediction interval endpoints at the negative pole ranged from moderately sized (cognitive inflexibility) to small-to-moderate (motivations). Positive endpoints ranged from moderately sized (thinking style) to exceptionally large (dogmatism). For motivational rigidity and thinking styles, much of this substantive heterogeneity was attributable to differences within, rather than across, samples, while the heterogeneity was roughly evenly distributed between these two levels of analysis for dogmatism and cognitive inflexibility.

The Full Model

We next regressed all nonoverlap effect sizes on 12 dummy-coded moderator variables, one for each potential combination of ideology-type and rigidity domain (e.g., dogmatism by economic ideology). The test of moderation was statistically significant, $F(12, 544) = 56.20$, $p < .001$. Residual heterogeneity was reduced further but remained present and substantial, $Q_E\,(554) = 2{,}470$, $p < .001$; $H^2 = 3.36$; $\sigma_1^2 = .003$ and $\sigma_2^2 = .008$; $I^2_{(2)} = 62\%$ and $I^2_{(3)} = 24\%$, such that $\sigma = .10$. Most of the substantive heterogeneity was found within, rather than across, samples. Results are presented in Figure 3.

All rigidity variables, except dogmatism, manifested correlations with economic ideology that were not significantly different from zero (using an α level of .01), with σs (ranging from .06 to .16) around three times as large as the β (ranging from .00 to .04), such that prediction interval widths (i.e., the range of the two poles) ranged from .24 units to .68 units. In contrast, dogmatism demonstrated a statistically significant positive correlation with economic conservatism with a σ roughly 2/3 as large as its β. Meta-analytic estimates for relations between rigidity domains and social conservatism were considerably larger than those for economic conservatism (βs ranged from .11 to .32; σs ranged from .03 to .12). A pronounced degree of heterogeneity was present for motivational rigidity and dogmatism's relations with social ideology, with prediction interval widths of .56- and .53-units, respectively; far less heterogeneity was present for cognitive inflexibility and thinking style, which exhibited narrower, largely positive, prediction intervals. Effect sizes for general ideology typically fell between those for economic and those for social (βs from .06 to .22; σs from .08 to .16; prediction interval widths from .19 units to .67 units), with the exception of cognitive inflexibility, which manifested an equivalently sized effect for general and social conservatism.

These results demonstrate that distinguishing between domains of "rigidity" and "the right" offers clear utility in clarifying when and why the intersections between left−right politics and rigidity-related processes vary. Namely, doing so explains 20% of the substantive heterogeneity present in the data set and illustrates clear differences in the magnitude and distribution of effects across domains of rigidity and political ideology. Nevertheless, a stark degree of heterogeneity remains. For example, 10 of 12 prediction intervals crossed zero even as seven of these 12 prediction intervals contained $r = .29$ (i.e., the 75th percentile of field-wide correlational effect sizes magnitudes per Gignac and Szodorai, 2016). This indicates that most combinations of political ideology domain and rigidity domain yield true effects that, depending on yet-unknown key moderators, may be negative or positive—with effect size magnitudes ranging from negligible to incontrovertibly large. A systematic analysis of potential moderators may clarify some of this residual variance.

Moderators

Nationality

Nations/regions with $k > 2$ were Brazil, Canada, Flanders, Germany, Hong Kong, Hungary, Italy, the Netherlands, Poland, South Africa, Sweden, Turkey, the United Kingdom, and the United States. We collapsed countries with $k < 2$ into European, South/Central American, Asian, and Oceanian categories. Results are presented in Supplemental Table S7 and Figure 4. A significant moderation effect was present for nationality in the overall model, $F(19, 522) = 18.99$, $p < .001$. After controlling for rigidity and ideology domain,[10] $F(19, 511) = 1.72$, $p = .03$. Heterogeneity was reduced, but remained high, $H^2 = 2.93$; $\sigma_1^2 = .008$ and $\sigma_2^2 = .003$; $\sigma = .10$; $I^2_{(2)} = 64\%$ and $I^2_{(3)} = 21\%$.

Given that the majority of effect sizes were observed in the United States, we also compared U.S. and non-U.S. samples, which revealed a significant difference, such that $F(2, 540) = 149.23$, $p < .001$, where β(USA) = .15, 95% CI [.13, .17] and β(non-USA) = .09, 95% CI [.07, .12]. As visualized in Figure 5, this difference was driven by variation across economic ideology, such that allowing the binary U.S. versus non-U.S. factor to interact with political ideology domain revealed a significant interaction effect, $F(2, 535) = 5.48$, $p = .004$. Specifically, effects were relatively consistent across U.S. and non-U.S. samples for social ideology (β[USA] = .21, 95% CI [.15, .26] vs. β[non-United States] = .22, 95% CI [.18, .25]), $F = .070$, $p = .792$; only modestly, albeit significantly, different for general ideology (β[United States] = .15, 95% CI [.13, .18] vs. β[non-United States] = .11, 95% CI [.08, .14]), $F = 5.64$, $p = .018$; and substantially and significantly different for economic ideology (β[United States] = .10, 95% CI [.06, .13] vs. β[non-United States] = −.03, 95% CI [−.07, .02]), $F = 16.34$, $p < .001$. Controlling for rigidity domain (and its interaction with United States vs. non-United States) did not reduce the strength or alter the significance of this interaction effect (*p*s < .005).

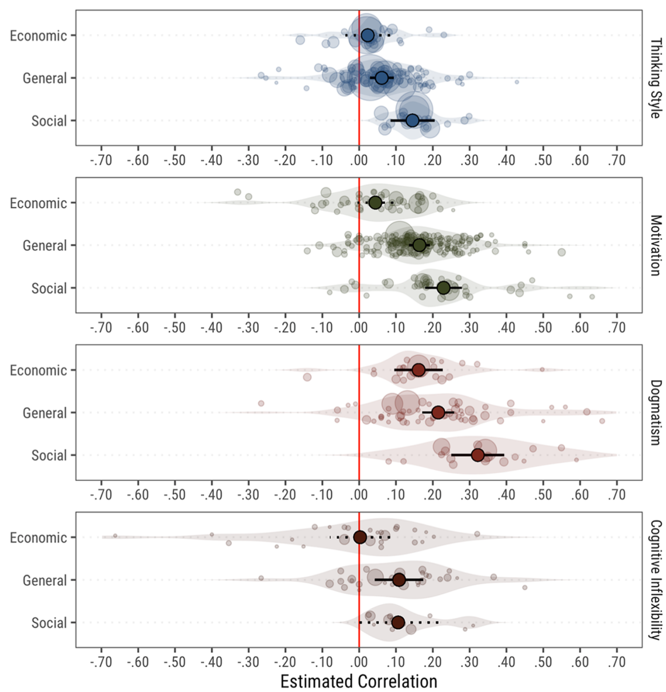
Sample Type

The type of sample from which each observation was collected accounted for a significant degree of residual heterogeneity when entered into the overall model, $F(7, 522) = 66.09$, $p < .001$ (see Figure 4). The smallest effect size was for samples matched to the demographic characteristics of the national population (β = .036, 95% CI [−.01, .07]), and the largest effect size was for government officials (β = .240, 95% CI [.09, .39]). Controlling for ideology and rigidity (i.e., the full model; see Supplemental Table S8) revealed

[10] Although the *p* value exceeds our alpha level of .01, the results of this analysis are of limited interpretability given that our data set only contains one nation with all rigidity and ideology variables: the United States.

Figure 3
Results for the Full Meta-Analytic Model

(A)



(B)

Variables	k	s	N	H^2	$I^2_{(2)}$	$I^2_{(3)}$	σ	95% PI	β	95% CI	p
Economic											
Cognitive Inflexibility	26	8	1,402	2.30	72%	9%	.16	-.35, .33	.00	-.06, .06	= .935
Motivational Rigidity	45	28	11,933	4.06	24%	56%	.11	-.17, .27	.04	.01, .08	= .018
Dogmatism	23	18	8,691	2.23	76%	< 1%	.10	-.05, .36	.16	.11, .21	< .001
Thinking Style	23	21	32,560	1.63	< 1%	80%	.06	-.10, .14	.02	-.02, .07	= .324
General											
Cognitive Inflexibility	30	19	6,707	2.79	< 1%	81%	.13	-.15, .38	.11	.06, .16	< .001
Motivational Rigidity	170	91	36,243	2.53	44%	31%	.09	-.02, .34	.16	.14, .19	< .001
Dogmatism	58	48	18,811	6.88	30%	60%	.16	-.10, .53	.22	.18, .25	< .001
Thinking Style	98	83	64,300	3.11	8%	79%	.08	-.10, .23	.06	.04, .09	< .001
Social											
Cognitive Inflexibility	12	5	1,087	0	13%	< 1%	.03	-.02, .17	.11	.02, .19	= .012
Motivational Rigidity	41	27	12,347	6.09	89%	< 1%	.12	-.06, .50	.23	.19, .27	< .001
Dogmatism	17	13	10,232	4.90	6%	85%	.12	.07, .60	.32	.27, .38	<.001
Thinking Style	23	22	33,386	1.22	< 1%	67%	.04	.06, .23	.15	.10, .19	< .001

Note. k = observations, s = samples, n = unique participants, σ = standard deviation in true effects between observations, β = meta-regression coeffecient.

Note. A = Meta-regression coefficients (large circles, outline in black) with 99% confidence interval and individual observations (translucent circles) in the full model. Results are segmented by political ideology and rigidity. Dotted confidence intervals indicate $p > .01$. B = Table of meta-analytic results for the full model, including heterogeneity and sample sizes. PI = prediction interval; CI = confidence interval. See the online article for the color version of this figure.

similar results, $F(7, 517) = 6.32$, $p < .001$, with residual variance such that $Q_E(517) = 2{,}116$, $p < .001$; $H^2 = 3.00$; $\sigma_1^2 = .008$ and $\sigma_2^2 = .001$; $\sigma = .10$; $I^2_{(2)} = 13\%$ and $I^2_{(3)} = 71\%$. Relative to nationally representative samples ($k = 45$), which are considered least likely to be at risk for bias (Higgins et al., 2019), seven of eight sample-types exhibited significantly larger effects. Namely, results relative to nationally representative samples were larger for students (β increased by .06, 95% CI [+.02, +.10], $k = 266$), nonrepresentative internet-recruited samples (β increased by .09, 95% CI [+.05, +.13], $k = 135$), yourmorals.org (β increased by .06, 95% CI [+.00, +.11], $k = 17$), community samples (β increased by .14, 95% CI [+.10, +.19], $k = 86$), and government officials (β increased by .14, 95% CI [.01, .28], $k = 5$).

Self-Report Versus Performance-Based Rigidity Measures

Performance-based outcome measures yielded significantly smaller estimated effects than did self-report outcome measures in the overall model, $F(2, 661) = 186.65$, $p < .001$, such that performance-based measures of rigidity manifested a trivial statistical association with conservatism (β = .065, 95% CI [.04, .09], $k = 493$) but self-reports manifested a small statistical association (β = .159, 95% CI [.14, .18], $k = 170$). Controlling for political ideology domain did not reduce the magnitude or alter the significance of this effect, $F(2, 560) = 30.41$, $p < .001$.

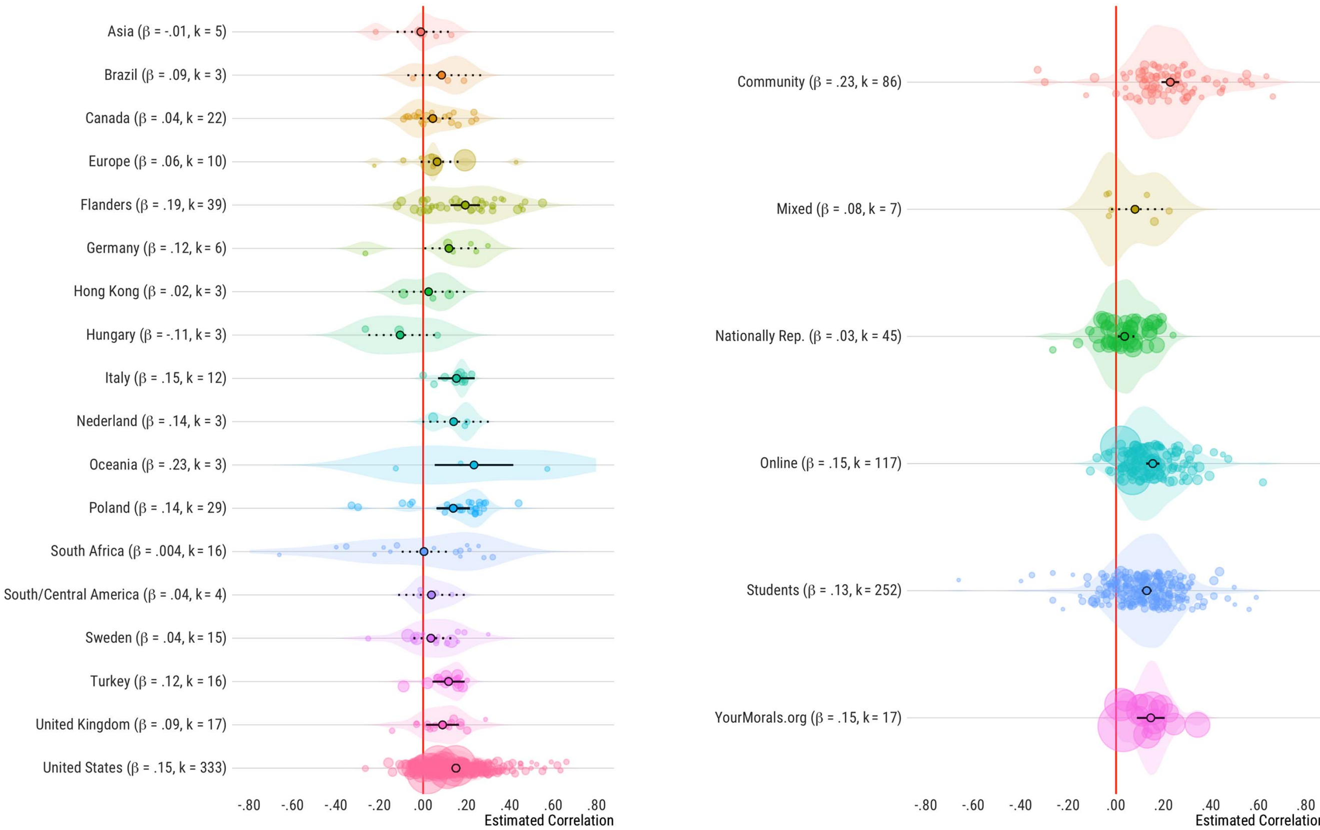
WEIRDness

As shown in Supplemental Tables S2 and S9, nations categorized as Western (β = .136, 95% CI [.12, .15]) demonstrated significantly larger conservatism-rigidity correlations than non-Western nations (β = .079, 95% CI [.02, .14]), $F(2, 536) = 143.00$, $p < .001$; similarly, nations categorized as rich (β = .140, 95% CI [.13, .16]) demonstrated significantly larger correlations than those categorized as nonrich, (β = .067, 95% CI [.02, .12]), such that $F(2, 536) = 149.18$, $p < .001$. Nevertheless, controlling for ideology and rigidity domain reduced these moderation effects to nonsignificance. Further, when standardized scores for industrialization, education, and degree of democracy were individually entered as continuous moderators of the relation between ideology and rigidity, education accounted for a significant degree of residual heterogeneity (intercept = .133, β = .022, 95% CI [.01, .04], $p = .003$), whereas industrialization and democracy did not. Once again, however, after controlling for ideology and rigidity type, education did not account for a significant degree of residual heterogeneity (see Supplemental Table S4, for details).

Content Overlap and Political Measures

Introducing political measures with rigidity-related content (i.e., "content overlap") into the meta-analytic data set revealed significant variation in conservatism-rigidity relations across political

Figure 4
Meta-Analytic Results by Country and Sample-Type



Note. Left = Model coefficients (large circles, outline in black) with 99% confidence interval and individual observations (translucent circles) based on subgroup analyses for each nation-category. Right = Model coefficients (large circles, outline in black) with 99% confidence interval and individual observations (translucent circles) in subgroup analyses for sample-category. See the online article for the color version of this figure.

ideology measures with- and without-criterion contamination, $F(2, 668) = 277.72$, $p < .001$, such that nonoverlap political measures manifested only a small association with rigidity, $\beta = .136$, 95% CI [.12, 15], whereas overlap measures manifested a large statistical association with rigidity, $\beta = .375$, 95% CI [.34, .41]. Controlling for rigidity domain[11] only slightly diminished the effect, $F(1, 663) = 125.76$, $p < .001$,—content overlap measures yielded larger effect sizes such that the difference in $\beta = .217$, 95% CI [.18, .26]. A large degree of heterogeneity remained in this expanded data set, $Q_E\,(663) = 5{,}907$, $p < .001$, $H^2 = 7.84$; $\sigma_1^2 = .016$ and $\sigma_2^2 = .005$; $\sigma = .14$; $I^2_{(2)} = 22\%$ and $I^2_{(3)} = 71\%$.

Further, we found significant differences among the noncontent-overlap political ideology measures, $F(16, 530) = 23.57$, $p < .001$ (see Figure 6, for point estimates of all political ideology measures and Supplemental Table S6), but this finding did not hold true after controlling for rigidity and ideology domain, $F(16, 519) = 1.93$, $p = .016$, such that $Q_E\,(519) = 2019$, $p < .001$; $H^2 = 2.86$; $\sigma_1^2 = .003$ and $\sigma_2^2 = .007$; $\sigma = .10$; $I^2_{(2)} = 63\%$ and $I^2_{(3)} = 22\%$. Moreover, little easily interpretable heterogeneity was evident across measures. Relative to symbolic ideology, only three of the 16 measures (with $k > 2$) yielded significantly different point estimates, one of which was a measure of social ideology, specifically, whereas the other was a catch-all category for self-report measures that were not specified in the study text.

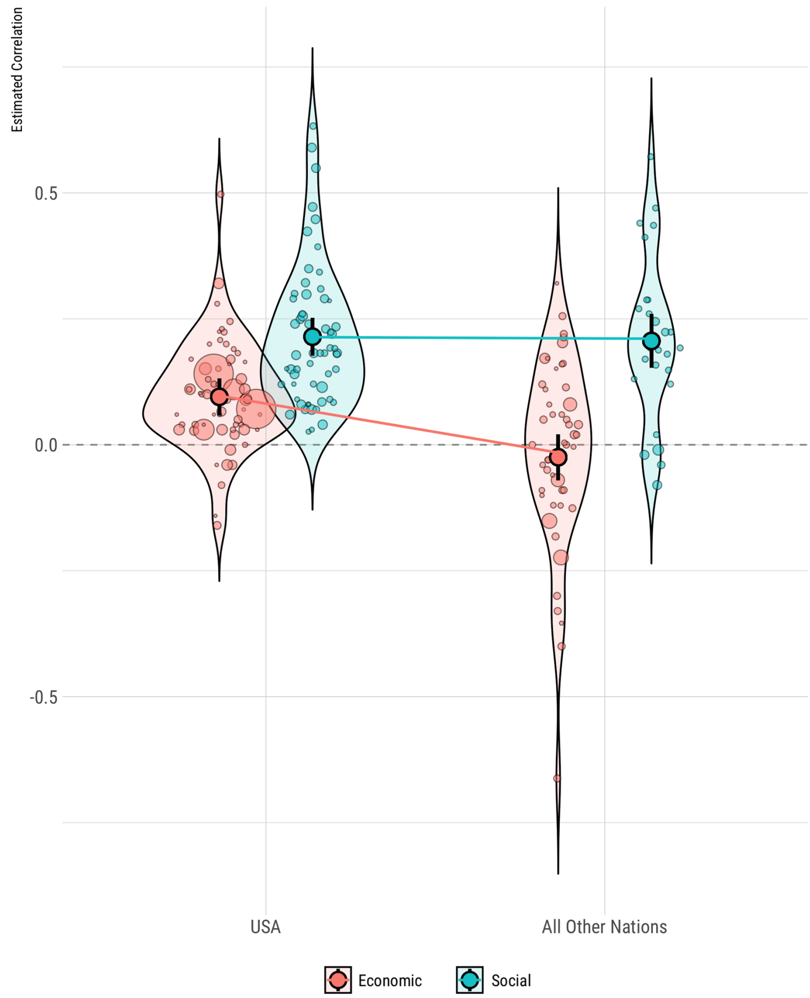
Publication Bias

We first examined the distribution of study outcomes via two contour-enhanced funnel plots (see Supplemental Figure 2). These analyses were conducted with content overlap effect sizes removed from the study pool, which may otherwise give the false appearance of publication bias. In the first plot, many effect sizes were outside of the anticipated range given their *SEs*, which is to be expected in the presence of considerable heterogeneity. Still, there was no clear asymmetry in the distribution of these outliers. When considering the second plot, in which a greater degree of substantive heterogeneity is accounted for, far fewer outliers were present and those outliers that remained were relatively symmetrically distributed. As such, neither plot provided clear evidence of publication bias. Nevertheless, a power analysis indicated that only 34.5% of studies

[11] We did not control for political ideology domain as content overlap measures typically could not be classified as reflecting economic, general, or social conservatism.

Figure 5

Social and Economic Ideology's Correlation With Rigidity in the USA Versus Other Countries



Note. See the online article for the color version of this figure.

were sufficiently powered to detect an effect size of $\rho = .10$, while a true effect of $\rho = .18$ would be necessary to achieve Cohen's (1965) widely accepted power benchmark of 80% (see Supplemental Figure 3).

PET analysis indicated that there was no statistically significant association between effect sizes and their standard errors for any of the four primary models (i.e., no moderators, political ideology domain moderators, rigidity domain moderators, and political ideology + rigidity domain moderators). Still, a significant moderation effect was present for publication-type, $F(2, 563) = 161.43$, $p < .001$, such that relative to effect sizes drawn from initial peer-reviewed journal articles ($\beta = .14$, 95% CI [.13, .16], $k = 459$), all other effect sizes were smaller ($\beta = .11$, 95% CI [.09, .14], $k = 246$). This result was reduced to nonsignificance after controlling for rigidity and political ideology domain, $F(1, 584) = 4.66$, $p = .031$, such that non-peer-reviewed studies contained effects smaller in magnitude than those from peer-reviewed studies, with the reduction in $\beta = -.03$, 95% CI [−.05, −.00].

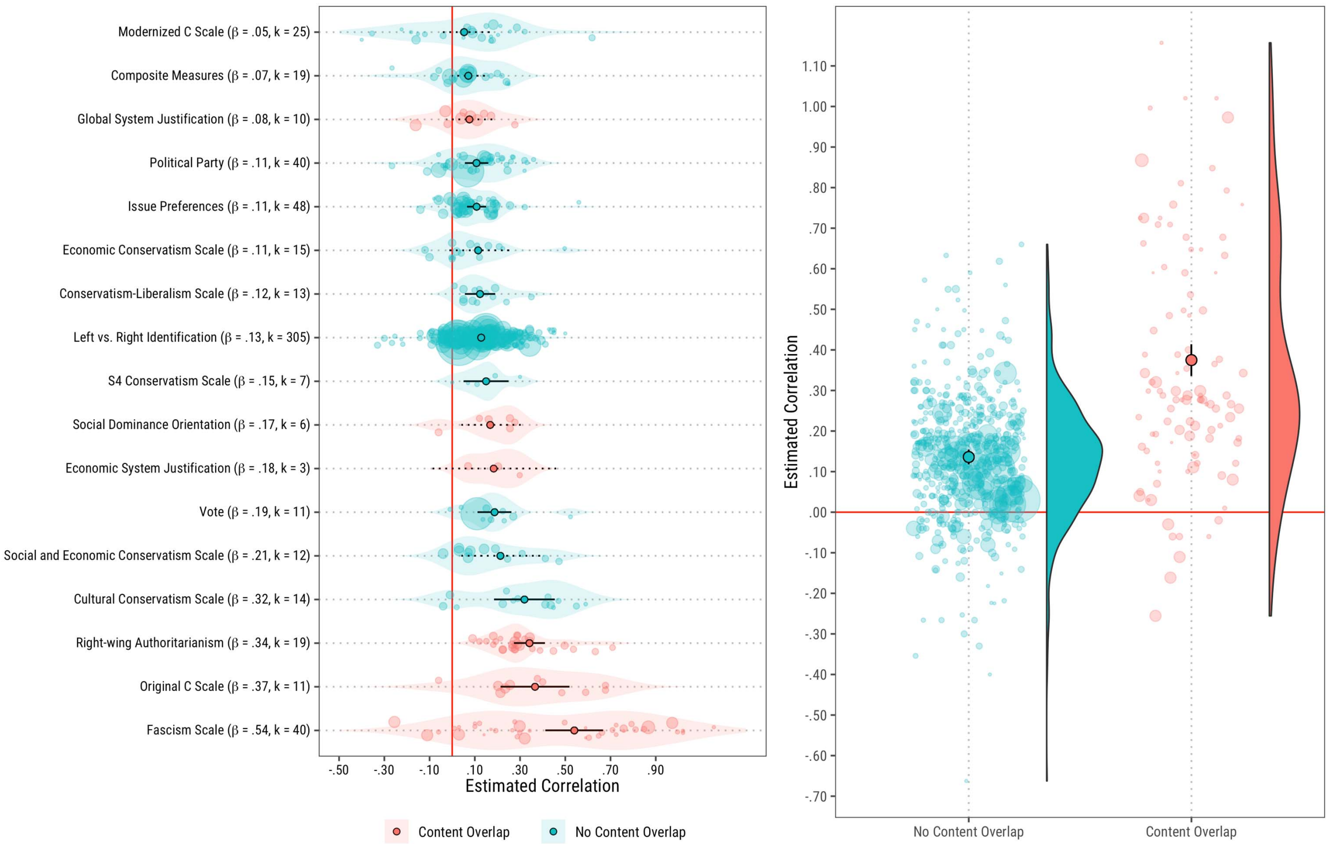
Discussion

The RRH, which posits that politically conservative beliefs appeal to people who are cognitively, motivationally, and ideologically rigid, has been subjected to decades of debate. The present meta-analytic review, which spanned 313 independent samples, 704 effect sizes, 35 nations, and more than 180,000 unique participants, (a) provides precise estimates of the magnitude and direction of the relation between conservatism and rigidity, (b) catalogs the extent to which true effect sizes in the literature are distributed around these meta-analytic estimates, and (c) elucidates moderator variables that account for these differences across studies.

The current review offers several central concrete takeaways, but the foremost is this: the variation in true effects (heterogeneity) among the population of studies in the RRH literature is both objectively large and theoretically consequential (Linden & Hönekopp, 2021). Although the overall correlation between conservatism and rigidity was $r = .13$, a random study drawn from the

Figure 6

Differences Across Measures of Political Ideology With- and Without Rigidity Content Overlap



Note. See the online article for the color version of this figure.

RRH literature is about equally likely to yield a small negative effect ($r \sim -.10$) as it is a large positive effect ($r \sim .30$). This finding seems to partially explain the RRH's controversial status among social scientists. We also think it raises two key, interlinked questions: *when* and *why* do relations between rigidity and conservatism vary?

The "When": Main Effects, Heterogeneity, and Boundary Conditions

Estimated population correlations for relations between economic conservatism and the rigidity variables (apart from dogmatism) tended not to statistically differ from zero, whereas estimated population correlations between social conservatism and the four rigidity variables were larger and uniformly statistically significant. Further, rigid thinking styles and basic cognitive mechanisms and processes manifested modest-to-small meta-analytic correlations with both general and social conservatism. Population estimates were larger for motivations to avoid complexity and ambiguity and largest for ideological rigidity (dogmatism).

Accordingly, titrating the conceptual and methodological precision of our meta-analytic models by distinguishing between political ideology in the social/cultural versus economic domains, on the one hand, and between constructs reflecting distinct classes of rigidity, on the other, reduced total heterogeneity by 20%. Yet, much theoretically provocative heterogeneity remained—made evident, most plainly, by 95% prediction intervals that crossed zero for 10 of the 12 combinations of rigidity-type and conservatism-type.

By incorporating moderating variables such as nationality, culture, sampling context, and measurement approach into our meta-analytic models, we successfully sourced some of this residual heterogeneity. For example, positive relations between rigidity and economic conservatism did not generalize beyond the United States, whereas social conservatism was significantly positively related to rigidity across national contexts. Relatedly, demographically representative samples yielded negligible effects, whereas community samples and samples of government officials yielded large effects. When it came to measurement, performance-based measures of rigidity (e.g., cognitive tests) manifested a small statistical association with conservatism, whereas self-reports manifested a larger statistical association. By contrast, most political ideology measures did not differ significantly from one another, with the major exception of ideology measures that reflect rigidity or rigidity-adjacent constructs, which yielded considerably larger effects. See "The 'Why?': Theoretical Implications for the Psychological Underpinnings of Ideology" below for a discussion of these results.

Answering the question of "when" requires us to situate main effects (population estimates) alongside the degree of variance in the true effects underlying them and moderators that in part explain said variance (Schmidt & Hunter, 2014). Given that the most generative and stark differences in the present meta-analysis were identified across social and economic ideology—and that many canonical findings in the ideological asymmetries literature are based on global assessments of political ideology (Jost, 2021)—we now provide such an interwoven account of the population correlation estimates, heterogeneity, and moderators for social versus economic ideology. This account also engages with the implications of differences across rigidity variables' relations with social versus economic ideology.

Putting It All Together: Social Versus Economic Ideology

Let us first consider social conservatism (vs. liberalism), which manifested meta-analytic population correlations with rigidity-related constructs that ranged from r = .11 to r = .32 (overall r = .22). Correlations of this magnitude have traditionally been characterized as small-to-moderate in terms of their theoretical implications (Cohen, 1992; cf. Götz et al., 2022), yet they are typical-to-large for individual differences research (i.e., falling between the 25th and 81st percentiles of all such differential psychology sizes; Gignac & Szodorai, 2016). Given that many scholars have deemed effects of these magnitudes to possess theoretical and practical importance for other notable predictors of social conservatism (e.g., low educational attainment; Lipset, 1959; Schoon et al., 2010), it seems reasonable, at first blush, to conclude that rigidity is meaningfully related to social conservatism. Moreover, even certain "small" effects can have profound real-world implications (Abelson, 1985; Funder & Ozer, 2019) and some authors posit that most psychological phenomena are an outcome of the additive influence of small effects, which may be meaningful in aggregate and/or longitudinally (Götz et al., 2022; Primbs et al., 2022; see Supplemental File 3, for an in-depth discussion of our effect size magnitudes).

Still, the heterogeneity accompanying social conservatism's population estimate was sizable. For instance, the range of true effects for motivational rigidity veered into the negative domain (i.e., including negative point estimate with a magnitude in the 12th percentile of all in differential psychology; Gignac & Szodorai, 2016), while its larger (positive) effect sizes extended to the 98th percentile of effect size magnitudes. Although social conservatism's effects varied across sampling contexts and measurement modalities (as described above), these moderators were not powerful enough to account for such a large degree of residual heterogeneity. All told, in any given new study of the relation between conservatism and rigidity, moderately sized positive correlations between social conservatism and rigidity can be expected, both large correlations and null correlations are less common but not entirely atypical, and both small negative correlations and exceptionally large positive correlations will be rare. Taken in concert with our finding that relations between rigidity and social conservatism are robust across nations—which was not the case for economic conservatism—our view is that the present meta-analysis corroborates a "rigidity-of-the-social-right" hypothesis. Nevertheless, further research is needed to recognize the precise circumstances and variables that amplify (vs. nullify) these effects to such a considerable degree (Cumming, 2014).

When considering economic conservatism, a markedly different story emerges. Population estimates for economic conservatism and rigidity in motivations, cognitive abilities, and thinking styles were consistently small (rs from .00 to .04)—and would arguably contravene the RRH if considered in isolation (Ferguson & Heene, 2021; Lykken, 1991; Meehl, 1978). That said, the population estimate for ideological rigidity, or dogmatism, was far larger (r = .16). True effects were widely distributed around zero, amounting to a similar degree of heterogeneity as was present for social ideology (e.g., most starkly, the 95% prediction interval for basic cognitive processes included both −.30 and +.30, or the 75th percentile of effect size magnitudes per Gignac & Szodorai, 2016]). Accordingly, when it comes to economic conservatism, the RRH holds under some circumstances, is rejected under others, and is directionally *incorrect* under others still. Moderator analyses identified several such boundary conditions. Positive correlations for economic conservatism were overrepresented in American samples, which yielded an estimated effect size of r = .10, whereas non-American samples yielded an estimated effect size of r = −.03. Similarly, in the handful of U.S. nationally representative samples where economic conservatism was assessed, the main effect was reduced to nonsignificance, r = −.02, 95% CI [−.13, .07], k = 5. Considering that meta-analyses of the RRH (including the present investigation) vastly overrepresent American samples, we wish to underscore the importance of this finding—a "rigidity-of-the-economic-right hypothesis" does not seem to hold much water beyond nonrepresentative samples in the United States.

The "Why?": Theoretical Implications for the Psychological Underpinnings of Ideology

Where do the present findings leave us? One plausible account of our findings is that mechanisms and/or epistemic features specific to social conservatism drive conservatism-rigidity relations, and that economic conservatism is merely "along for the ride" in certain environments (Malka & Soto, 2015). This conclusion is consistent with prior evidence demonstrating that social and economic conservatism are negatively correlated, or effectively orthogonal, in many nations around the globe (Malka et al., 2019) and that features of the information environment and its associated partisan pressures, such as cues from elite political coalitions (and one's degree of exposure to them), seem to drive these instances of coherence versus incoherence (Baldassarri & Goldberg, 2014; Kozlowski & Murphy, 2021; Layman & Carsey, 2002; Malka et al., 2019; Noel, 2014; Zaller, 1992; cf. Azevedo et al., 2019). Our results are consistent with this conclusion insofar as (a) social conservatism was related to rigidity with relatively large effect sizes that are robust across nations and sampling contexts; (b) economic conservatism was not correlated with rigidity on average but *was* correlated with rigidity in the United States, where social and economic conservatism are structured together (Federico & Malka, 2022); and (c) rigidity did not predict economic conservatism in representative national samples (which include many politically disengaged individuals), even within the United States.

Despite the popularity of the left versus right political spectrum among researchers, the psychological antecedents of political ideology may be better understood in the context of their correspondence to differing social and economic ideologies than in terms of a global left versus right distinction (Duckitt & Sibley, 2009; Saucier, 2013). Consequently, any conceptual resonance between the philosophical tenets of broad-based political conservatism (e.g., system justification) and psychological characteristics are probably of limited use for explaining links between rigidity and conservatism (e.g., Costello, Clark, et al., 2022). Note that this conclusion runs counter to nearly every instantiation of the RRH but is unavoidable given that both economic and social conservatism are "right-wing" belief systems (i.e., both ostensibly promise to justify extant hierarchies and systemic inequalities; Jost, 2021). Analyzing the philosophical and practical incongruities of social and economic conservatism (vs. liberalism), perhaps including emphases on governmental protection versus individual freedoms, may offer a fruitful explanatory lens with which to understand psychological differences in rigidity (Federico & Malka, 2018).

Another significant aspect of the present review is our comparison of ideological, motivational, cognitive, and neurocognitive rigidity. Our results clearly show that social conservatism is, on average, a positive correlate of all four rigidity domains. Still, clear differences across rigidity domains emerged. Dogmatism and motivational rigidity yielded considerably larger main effect sizes and heterogeneity than did thinking styles and cognitive inflexibility. The former domains (dogmatism and motivational rigidity) demonstrated sizeable overall correlations with social conservatism embedded within sweeping ranges of true effects and the latter domains (thinking styles and cognitive inflexibility) demonstrated modest overall correlations embedded within narrow ranges of true effects. These findings are consistent with the possibility that rigidity in higher order mental phenomena (i.e., ideology and motivations) transacts with the political environment to a greater extent than does rigidity in basic cognitive processes. Specifically, dogmatism and motivational rigidity may be related to politics in a way that is especially malleable and responsive to contextual moderators. Understanding the causes of these differences in volatility *and* magnitude across rigidity domains is a promising endeavor for future research (Bryan et al., 2021).

Additional grist for the interpretive mill may be had by examining the proportion of heterogeneity attributable to variation within- versus between-samples. In the full model (i.e., accounting for differences across political ideology and rigidity types), roughly 2.5 times more substantive heterogeneity was attributable to observations drawn from the same sample than from observations drawn from different samples. This indicates either that yet-unknown individual-level moderators/confounds shape relations between specific pairings of ideology-type and rigidity-type (e.g., perhaps religiosity, which accompanies social conservatism, amplifies dogmatism specifically) or that our coding scheme was not sufficiently narrow to identify meaningful distinctions within our ideology and rigidity typologies (e.g., perhaps motivational rigidity meaningfully fractionates into need for order vs. ambiguity intolerance, and so on, and these facets manifest divergent relations with political ideology). In both cases, however, developing mechanistic accounts of the nexus between ideology and cognition seems to require that social scientists abandon blunt, foggy conceptualizations of "conservatism" or "rigidity" in favor of specific, narrow constructs that lend themselves to piecemeal, cumulative theory development (Fried, 2020).

Parsing and classifying these variegated influences on rigidity-ideology relations is a crucial task that we believe has been obscured by a dominant focus on mean-level differences between the left and right (Hanel et al., 2019). Researchers who downwardly mine instances where the RRH does (vs. does not) hold will be well placed to clarify mechanisms linking psychological rigidity to ideologies and attitudes (i.e., including those other than conservatism; Zmigrod et al., 2021). In other words, asking questions like "why do people who score highly on dogmatism tend to endorse social, but not economic, conservatism in most of the world?," or "what are the circumstances under which social conservatism *is not* meaningfully associated with cognitive inflexibility?"—rather than "are conservatives more rigid than liberals?"—may offer a meaningful path forward.

Limitations

To our knowledge, this article describes the most comprehensive quantitative synthesis of literature bearing on the RRH. Nevertheless, the following important limitations should temper the strength of inferences drawn from our findings.

Causality

Evidence is accumulating that many constructs once thought to be exogenous to political ideology are, in part, influenced by political ideology, at least in certain contexts (Bakker et al., 2021; Egan, 2020; Jost et al., 2014; Luttig, 2021; Margolis, 2018; Vargas Salfate et al., 2022). Given that the present review comprises entirely observational research, often involving only a single measurement occasion, our findings do not inform us about temporal antecedence, let alone causality. Multiple causal explanations are consistent with the results.

Conceptual and Methodological Scope

We only examine a subset of psychological variables that are commonly framed as causes or correlates of political ideology in the present investigation. Perhaps most importantly, we did not include measures relevant to threat sensitivity and existential needs, from physiological indicators (Bakker et al., 2020) to negativity bias (Johnston & Madson, 2022) to trait neuroticism (Federico, 2022), which features prominently in several explanatory accounts of political ideology (Hibbing et al., 2014; Jost, 2017). That said, the literature on these accounts is murky and either does not appear to replicate (Bakker et al., 2020; Johnston & Madson, 2022) or differs in both direction and magnitude across threat-sensitivity indicators, conservatism indicators, and contexts (Bergh & Brandt, 2022; Brandt & Bakker, 2022; Kahn et al., 2022).

Further, our chosen rigidity domains do not include the large literature concerning integrative complexity—a kind of rigidity assessed via the complexity of spoken or written thought or explanations for one's beliefs that has been widely used to test the RRH (e.g., Suedfeld et al., 1992). Houck and Conway (2019) recently meta-analytically examined the relation between political ideology and integrative complexity, finding that most liberals and conservatives do not differ from one another in integrative complexity ($r = -.01$, 95% CI [−.07, .05]), but conservative political leaders, specifically, use much less complex language than their liberal counterparts ($r = -.37$, 95% CI [−.47, −.26]). This finding is broadly consistent with differences in rigidity-conservatism correlations across national and sampling contexts revealed in the present investigation—which we have attributed to environmental moderators pertaining to political norms and information.

To that end, another limitation of our review is that we did not code for differences across levels of political engagement. As we have described, mounting evidence shows that relations between rigidity and conservatism tend to be stronger, and are sometimes exclusively found, among people who are politically engaged (Federico & Goren, 2009; Feldman & Johnston, 2014; Johnston et al., 2017; Kalmoe, 2020; Ollerenshaw & Johnston, 2022). Although we did not explicitly test this moderation effect, our findings and theoretical conclusions concerning the nature of ideology, and its psychological origins, are consistent with the growing literature on the importance of political information environment.

Relatedly, the RRH literature is predominantly composed of American samples, and as such our meta-analysis does not support precise conclusions about differences in support for the RRH across countries other than the United States. (Henrich et al., 2010). The clearest drawback of this overrepresentation of American samples is that the United States is characterized by both an unusual degree of partisan polarization and merely two (large) political parties that provide little differentiation between economic and social policy (e.g., governments comprised many viable parties typically include those with an admixture of socially liberal and economically conservative policies, or vice versa; Noel, 2014). Perhaps consequently, we found that the United States displays a small but globally unusual link between rigidity and economic conservatism. In future research, it would be ideal to examine whether cross-national differences in levels of economic development (Inglehart & Welzel, 2005), historical cultural and religious traditions (Benoit & Laver, 2006), and ecological features (Conway et al., 2020; Götz et al., 2020) account for variation in the psychological correlates of ideology.

Our meta-analysis also did not examine plausible theoretical alternatives to the RRH. The ideological extremity hypothesis, which has seen a growing degree of evidentiary support in the literature, predicts a curvilinear relation between ideological extremity on both the left and right, on the one hand, and rigidity, on the other, rather than simple left–right differences (e.g., Costello & Bowes, 2022; Zmigrod et al., 2020). A meta-analytic test of curvilinearity in the relation between political conservatism and cognitive rigidity would require access to the raw data used to calculate each effect size to derive semipartial correlations (e.g., Williams & Livingstone, 1994), which was not feasible for the current review. With the rise of open science and the increasing frequency with which authors make raw data publicly available, however, future meta-analytic research will hopefully be able to test for the presence curvilinearity.

Measurement and Construct Validity

Another limitation of the present work is the underdeveloped validity of many or most rigidity constructs and measures. For instance, the idiosyncrasies of information processing are not easily accessible to introspective observation (see Kahan, 2016). Indeed, cognitive psychology and neuropsychology typically rely on behavioral tasks to assess cognition (e.g., cognitive ability or memory are rarely measured using self-reports), in part because self-assessments of cognitive performance are frequently inaccurate (Furnham, 2001; Kruger & Dunning, 1999). Yet, ostensibly "objective" measures of cognition traits are often unreliable and/or demonstrate poor construct validity for individual differences, perhaps because of their high situational specificity and resultingly poor replicability (e.g., Epstein & O'Brien, 1985; Hedge et al., 2018). The most likely possibility may be that behavioral measures and self-reports each have their own sets of psychometric strengths and weaknesses and tap related but distinct constructs.

Further, while we accounted for the presence of rigidity content in ideology measures, we did not code for the presence of ideology content in rigidity measures. For example, the primary measure of dogmatism in the literature, the Doggone Old Gnu (DOG) Scale, seems to be confounded with religiosity and social conservatism (Conway et al., 2016; Duckitt, 2009). Thus, our findings concerning dogmatism's outsized correlations with conservatism, relative to other rigidity variables, may be attributable to measurement error. While important to consider, this possibility may not be especially explanatorily powerful given that only a handful of items seem to be confounded in a way that has been empirically documented for similar measures (e.g., Stanovich & Toplak, 2019). Still, future work addressing measurement invariance and test bias across the political left and right for dogmatism measures would be useful. A related limitation involves "jingle-jangle" fallacies (Block, 1995). For example, although we opted to aggregate Rokeach's (1960) Dogmatism Scale and Altemeyer's (1996) DOG Scale in our analyses, the two measures may operationalize dogmatism quite differently. Still, our use of multiple meta-analytic models that differ in specificity may buffer against interpretative errors owing to loose nomological networks and jingle-jangle fallacies.

The Pitfalls of Meta-Regression

We predominantly relied on meta-regression, rather than subgroup analyses, to address the problem of confounded moderators (Lipsey, 2003). As underscored by Schmidt (2017), the statistical and meta-scientific obstacles of traditional regression also apply to meta-regression. For instance, because we did not correct for measurement error in our observed *rs*, the true strength of all values (but especially those of our moderators) may be larger than our estimates (Schmidt & Hunter, 2014). Moreover, our more complex analyses may suffer from low statistical power. Although we had a relatively stable sample size ($k > 700$, or roughly the size of a moderately sized internet-recruited sample), some nonsignificant categorical moderators with few observations at certain levels (e.g., esoteric measures of political ideology) may be false negatives. We also did not test for many three-way statistical interactions when examining potential moderators (i.e., rigidity construct by conservatism-type by third moderator) owing to inadequate statistical power. Our approach therefore typically assumes that each moderator's impact on the relation between a given level of political conservatism (e.g., economic) and a given rigidity variable (e.g., dogmatism) is equivalent to that moderator's impact on all other levels of conservatism for all other rigidity variables. A related problem is that we did not preregister the potential moderators to be examined in meta-regression, or how they would be operationalized, which raises the specter of data dredging (referred to by Higgins and Thompson (2002) as "the principal pitfall in meta-regression"). The next meta-analysis of the RRH and/or the psychological causes and correlates of political ideology should ideally follow a prospectively registered analysis plan and protocol (Quintana, 2015).

Conclusion

We hope that the present review allows for more nuanced accounts of the psychological correlates of political ideology to emerge. Our meta-analysis suggests that research on psychological dispositions and ideology ought to put the distinction between social and economic conservatism front and center (Claessens et al., 2020), address contextual factors that condition ideological coherence (Jost et al., 2009; Federico & Malka, 2018, Johnston et al., 2017; Malka & Soto, 2015), and formulate and evaluate theories on the basis of accounting for heterogeneity (in addition to the traditional yardsticks; Linden & Hönekopp, 2021). We welcome and anticipate challenges to our conclusions, but hope, at the very least, that psychological science will be somewhat closer to reconciling an intellectual conflict that has worn on for well-over half a century.

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