The relationship between team diversity and team performance: reconciling promise and reality through a comprehensive meta-analysis registered report

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Authorship declaration

Lukas Wallrich designed the project and wrote the Stage 1 Report. Victoria Opara conducted the literature search and led on the initial screening. Miki Wesołowska and Lukas Wallrich conducted the full-text screening and coding, with contributions from Ditte Barnoth and Victoria Opara to the coding of the non-English literature. Sayeh Yousefi reviewed and synthesized the articles testing non-linear relationships. Lukas Wallrich conducted the analyses. Lukas Wallrich wrote the Stage 2 Report and the Supplementary Materials, which were reviewed and edited by all authors.

Notes:

Supplementary Materials: All supplementary materials are available on https://lukaswallrich.github.io/diversity_meta/.

Data and code availability: All data and reproducible analysis code are available through this repository: https://github.com/LukasWallrich/diversity_meta. An interactive web application that allows for further exploration and analysis of the dataset can be found here: https://lukaswallrich.shinyapps.io/diversity_meta/.

Abstract

Workforce diversity is increasing across the globe, while organizations strive for equity and inclusion. Therefore, research has investigated how team diversity relates to performance. Despite clear arguments why diversity should enhance (some types of) performance, and promising findings in individual studies, meta-analyses have shown weak main effects. However, many meta-analyses have failed to distinguish situations where diversity should have a positive impact from those where its impact is more likely to be negative, leaving boundary conditions unclear. Here, we summarized the growing literature across disciplines, countries, and languages through a reproducible registered report meta-analysis on the relationship between diversity and team performance (615 reports, 2,638 effect sizes). Overall, we found that the average linear relationships between demographic, job-related and cognitive diversity, and team performance are significant and positive, but insubstantial ($|r| < .1$). Considering a wide range of moderators, we found few instances when correlations were substantial. However, context matters. Correlations were more positive when tasks were higher in complexity or required creativity and innovation, and when teams were working in contexts lower in collectivism and power distance. Contrary to expectations, the link between diversity and performance was not substantially influenced by teams' longevity or interdependence. The main results appear robust to publication bias. Further research is needed on how diversity climates and team cultures affect these relationships, and when there may be non-linear relationships – yet for the moment, promises of wide-spread performance increases may not be the strongest arguments to promote diversity initiatives. We discuss further implications for researchers and practitioners, and provide a web app to examine subsets of the data: https://lukaswallrich.shinyapps.io/diversity_meta/.

Keywords: diversity, team performance, creativity, problem-solving, meta-analysis

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In light of increasing workforce diversity (e.g., Tavernise & Gebeloff, 2021) and an increasing focus on teamwork within organizations (Cross et al., 2016), there has been a growing interest in the relationship between diversity and team performance. Whether diverse teams outperform homogenous teams evidently does not change the moral and legal case for the creation of equal opportunities. However, an understanding of that relationship can inform diversity management and staffing practices. Research into the diversity-performance nexus has been shaped by two competing theories: social identity theory (Tajfel et al., 1979) leads one to expect that diverse teams will experience more conflict, less cooperation, and thus reduced performance. Conversely, approaches that focus on cognitive resources posit that team diversity should increase performance by increasing the range of ideas and perspectives that are available to the group (e.g., Bohman, 2006; S. Page, 2019, though the idea dates back to John Stuart Mills and Aristotle).

Given these conflicting expectations, it is not surprising that empirical studies have obtained conflicting results. Some studies find clear benefits of diversity. For instance, in a field experiment where student teams started real companies, teams that were diverse in terms of their cognitive abilities outperformed those that were less diverse, regardless of the average level of cognitive ability (Hoogendoorn et al., 2017). However, other studies found negative relationships, for instance between ethnic diversity and the performance of public sector institution (Pitts & Jarry, 2009). Several meta-analyses have attempted to aggregate the evidence, and have generally found very weak links between diversity and performance (e.g., Bell et al., 2011; Horwitz & Horwitz, 2007; Triana et al., 2021). However, these meta-analyses suffer from conceptual and methodological limitations, which we discuss below. To set the context, though, we first delineate the key constructs of interest.

Delineating diversity

Diversity refers to differences between members of a collective on any particular characteristic. These characteristics include some that are easily observable (e.g., gender, ethnicity) and others that are less visible (e.g., personality traits, values). Some are stable (e.g., first degree), others change continuously (e.g., age). What is common to all of them, however, is that they are likely to have influenced the life experiences individuals have had, and that they will thus influence how individuals conceive of and approach any given task (Sulik et al., 2021).

For the purpose of this meta-analysis, we will group types of diversity into three broad dimensions, following van Dijk et al. (2012): demographic diversity (e.g. age, nationality), cognitive diversity (e.g., personality traits, education level) and job-related diversity (e.g., function, tenure), with a focus on the first two dimensions.[1]

Delineating team performance

More and more work in organizations is conducted by teams rather than individuals (Cross et al., 2016) and the performance of teams varies drastically – by one estimate, being part of a high-performing team can make team members five times as productive as they would be in an average team (S. Keller & Meaney, 2017). Teamwork in organizations aims at all kinds of outcomes, from simple production tasks on an assembly line to complex multi-stage problem-solving, for instance by executive teams. Therefore, team performance takes many shapes.

[1] As any effects of job-related diversity are likely to be highly context- and task-specific, this dimension appears to be of limited theoretical interest. Nevertheless, it is the one that can most easily be influenced by HR and management practices. Therefore, the understanding of any consistent patterns in the evidence matters for practitioners and we include it here.

Classifying performance tasks and types

Performance tasks can be classified in a myriad of ways. When it comes to testing the relationship of performance with diversity, three aspects appear most relevant, as outlined in a recent review (Sulik et al., 2021): *(a)* the complexity of the task, *(b)* the question of whether creativity, and particularly divergent or convergent thinking is required, and *(c)* the level of interdependent cooperation that is required of the team members. Relatedly, team performance can take different shapes, including proximate measures such as productivity and creativity and more distant measures such as the financial performance of the product/unit managed by the team.

Diversity and performance: Theoretical expectation

Theoretically, diversity might be predicted to enhance performance as it corresponds to greater collective cognitive resources. Team members that differ in their abilities, experiences and attitudes evidently contribute more to the common 'toolkit' than team members whose contributions largely overlap. They are thus more likely to be able to explore the full solution space and less likely to suffer from collective blind spots (Hong & Page, 2004). Similarly, they are likely to be more accurate in predictions, such as those of revenues or costs, because the aggregation of diverse estimates tends to reduce errors (S. Page, 2019; Sulik et al., 2021).

Conversely, diversity might dampen performance due to its potential link to intergroup divisions. This goes back to the foundations of social identity theory (Tajfel et al., 1979), according to which individuals seek to create a distinctive social identity for themselves and derive psychological benefits by striving for positive distinctiveness. This 'ingroup love' then frequently leads to privileged treatment of and preferential attachment to others who share a common identity, which might result in communication barriers or even in open conflict within diverse teams. It should be noted that some of these difficulties might not be all bad – in fact, some communication barriers have been suggested to improve the quality of deliberation as they require team members to articulate their hidden assumptions, which then enables their closer inspection (Phillips et al., 2009). However, intergroup tensions might also play out in less explicit ways, for instance when ‘ambient cultural disharmony’ increases anxiety and reduces creativity (Chua, 2013).

Both accounts of the effects of diversity are based on ideas backed by strong evidence as well as common sense – so they each describe plausible pathways from team diversity to team performance. They are each more closely aligned with some dimensions of diversity than others: demographic diversity is particularly likely to trigger intergroup divisions, while cognitive diversity most immediately brings greater cognitive resources. However, demographic attributes are often closely associated with differences in lived experiences, which again results in greater collective cognitive resources. Conversely, cognitive attributes such as values can trigger social identity processes where ‘bird of a feather flock together’ (Ertug et al., 2022). Therefore, our interest is less in the dimensions of diversity per se than in boundary conditions that shape the observed relationships between diversity and performance. Contextual factors concerning the team task, type or setting are likely to influence the relationship between diversity and performance. Furthermore, regional, cultural and methodological differences may matter, yet have been underexplored to date.

Diversity and performance: Heterogeneous evidence

Even though popular business books (Syed, 2019) and management magazine articles (Rock & Grant, 2016) tout the promise of diverse teams, empirical findings are mixed. For many facets of diversity and performance, there are large studies that arrive at contrasting results. For instance, ethnic workforce diversity has been associated with better (Moon & Christensen, 2020) and worse (Pitts & Jarry, 2009) performance of US federal agencies.

Given the plethora of studies, there is a need to both aggregate the evidence and to identify moderators that explain when positive/negative effects are likely to emerge.

Existing meta-analytical work and its limitations

Over the past two decades, various researchers have attempted to synthesize the burgeoning literature on diversity and team performance meta-analytically, as summarized in Table 1. Three key results emerge from that work: *(1)* any overall relationships between diversity and performance appear very small (i.e. $|r| < .1$), *(2)* while job-related diversity tends to have positive associations with performance, demographic and cognitive diversity tend to have negative associations, and *(3)* effect sizes are highly heterogeneous.

In a context in which theory leads one to expect substantial effects, yet aggregate effects are small and heterogeneous, the focus of evidence aggregation should be on identifying boundary conditions, i.e. moderators (e.g., Moon & Christensen, 2020; Sulik et al., 2021; van Dijk et al., 2012). Knowledge about boundary conditions can help with evaluating and developing theory, shape future research and inform diversity management practices. Accordingly, all previous analyses have considered moderation, yet the results suffer from two critical limitations. Firstly, many analyses test many potential moderators with low statistical power, which suggests that false positives and false negatives might well outnumber true discoveries. For instance, in the most recent comprehensive meta-analysis of the diversity-performance link, van Dijk et al. (2012) report tests of moderation based on a median number of effect sizes (*k*) of 13. In Triana et al.'s (2021) meta-analysis, this median had increased to 22, though the tests did not concern team performance but rather specific hypothesized mediators. However, this indicates that evidence for better powered analyses is now available. Secondly, the meta-analyses to date tested moderators individually, and rarely reported associations between them. Testing multiple individual predictors of the size of an effect, without taking into account their association, would evidently never be acceptable in primary research. In the meta-analytic context, this could not have been done differently with the small datasets available to early meta-analyses, yet the development of meta-regression (Gonzalez-Mulé & Aguinis, 2018) and meta-decision tree (X. Li et al., 2020) techniques and the growth of the evidence base allows us to run more rigorous and informative analyses. This also allows for a consideration of changes over time, which are missing from extant meta-analyses, yet critical given that intergroup biases and the discourse around diversity in organizations and society more broadly has changed in recent decades (Charlesworth & Banaji, 2019; Ely & Thomas, 2020).

In considering boundary conditions, it is also important to consider the global reach of the evidence. Meta-analyses to date have focused on English language sources, and (while this is rarely explicitly reported) thus been dominated by WEIRD samples (Henrich et al., 2010), with the exception of one Chinese-language meta-analysis (X. Wei et al., 2015) that has largely been ignored by the English-language literature. Therefore, we lack evidence and transparency regarding the generalizability of results, which is critical for theory development and for practitioners working in a wide range of cultural contexts. We make use of rapidly improving machine translation tools (J. L. Jackson et al., 2019) to conduct searches in 13 major languages and thus base our analyses on a broader, more diverse and more inclusive evidence base.

Conceptually, meta-analyses to date averaged across *subjective* (i.e. self-reported) and *objective* measures of performance. Van Dijk et al. (2012) suggested that this may distort result and argued that subjective ratings should only be deemed valid indicators of performance if tight conditions are met. Nevertheless, their subsequent analyses of moderators, as well as all other evidence syntheses, are dominated by effect sizes based on self-reports, so that we know little about the link between diversity and objective measures of performance, and nothing about moderators of *that* link. While our analyses also include subjective measures, for a range of reasons discussed below, we report on their robustness to the inclusion of objective measures only, and test for differences between objective and subjective measures in a more rigorous way.

When it comes to effect sizes estimates, in line with most research, past meta-analyses relied on null-hypothesis significance testing to determine whether we have reason to believe that the association between diversity and performance is non-zero. However, that question appears to be of limited relevance to researchers and practitioners alike. Equivalence tests are an approach that is growing in popularity because it allows to test whether we have reason to believe that an association is substantial or insubstantial – or whether the evidence is still inconclusive (Lakens et al., 2018). This can allow for more nuanced conclusions, particularly where estimates are small or not significant, and thus forms our focus here.

Finally, most meta-analyses to date do not offer a rigorous treatment of publication bias. This is often insufficiently explored in organizational psychology (Siegel et al., 2021), even though substantial bias is present in at least some sub-fields (O'Boyle et al., 2014) and can skew results. This is comprehensively assessed here, in line with methodological research that highlights the need for triangulation between different methods (Rodgers & Pustejovsky, 2021). Similarly, we consider a range of methodological moderators that yield insights into the robustness of the evidence, shed light on the viability of specific theories and highlight directions for future research.

Table 1. *Meta-analyses to date*

		Scope/definitions		Results	
Reference	**Timespan, number of studies (N) & effect sizes (*k*)**	**Diversity**	**Performance**	**Main effects[a]**	**Moderators[b]**
[current work] (2024)	1961 – 2023 N = 615 *k* = 2,638	- demographic - job-related - cognitive	*Measure:* Objective v subjective *Type:* General performance Creativity & innovation Productivity	[Substantial/insubstantial/inconclusive] link of [demographic/job-related/cognitive] diversity with performance, *see results section*	Task-related moderators Contextual moderators Methodological moderators
Triana et al. (2021)	1961 – June 2019 N = 94 *k* = 280	Deep-level, i.e. personality, values and culture	General performance (+ mediators: positive emergent states, positive team processes, team conflict)	No link with performance (ρ = -.01), but links with positive emergent states (ρ = -.09), positive team processes (ρ = -.13) and team conflict (ρ =.14)	Only reported for mediators. Stronger negative effects on *process* within executive teams
Wei et al. (2015) – *in Chinese*	1984 - 2014 N = 137 *k* = 345	Demographic diversity (separation, variety or disparity)	Innovation performance General task performance	Positive relationship between variety and performance (ρ = .07), n.s. for separation (ρ = -.04) and disparity (ρ = .00)	*Performance type:* stronger links with innovation performance *Culture:* more positive link in Eastern than Western countries *Team type:* More positive link for executive and R&D teams
van Dijk et al. (2012)	1989 – 2011 N = 146 *k* = 612	- demographic - job-related - deep-level	Objective Subjective (judged by member, internal team leader, external team leader)	N.s. for demographic (*r* = -.02) or deep-level diversity (*r* = -.01), small sig. positive link for job-related diversity (*r* = .05)	Significant effects only for subjective performance measures (demographic: -.05, job-related: .04). No sig. link with objective measures (poss. due to lower power). Strongest associations for rating by external leader, rather than team members. More positive effect for innovation rather than in-role performance.
Bell et al. (2011)	1980 – 2009 N = 92 *k* = 323	- task-related - bio-demographic (focus on conceptualization:	General performance Creativity & innovation Efficiency	Negative link with gender and ethnicity, particularly for innovation, positive links with functional (and	Team type: strongest positive effects for design and product development teams.

		separation, variety or disparity)		partly educational) background diversity. (*r*s <= .1)	Site: lab studies show no links with gender/ethnicity, while field studies show clear negative link
Stahl et al. (2010)	1966 – 2006 *k* = 108	Cultural diversity, measured at surface and deep-level, intra-nationally and cross-nationally	General performance Creativity (+ process measures)	No significant link with performance (*r* = -.02), positive link with creativity (*r* = .16)	No significant moderators of diversity-performance or diversity-creativity link found.
Hülsheger et al. (2009)	Until 2007 *k* = 23 (for diversity-performance)	- job-relevant - demographic	- objective - self-rating - independent rating	Positive link with job-relevant diversity (ρ =.15). Negative link with background diversity (ρ = -.13).	Measurement: Self-reported performance not linked to job-relevant diversity, in spite of positive links from objective measures and independent raters.
Joshi & Roh (2009)	1992-2009 N = 39 *k* = 117	- relations-oriented (ethnicity, gender, or age) - task-oriented (education, functional background, or organizational tenure)	Various	No overall association (*r* = -.01), but small significant negative association for relations-oriented (*r* = -.03) and positive association for task-oriented diversity (*r* = .04)	More negative effect of gender and ethnic diversity in homogeneous sectors. Also, more negative effect of relations-oriented diversity if teams are interdependent or together for the long rather than short term.
Horwitz & Horwitz (2007)	1985-2006 N = 35 *k* = 78	- task-related - bio-demographic	Quality and quantity	Task related diversity predicts performance (*r* = .1) Bio-demographic diversity n.s.	Rater: Link with self-reported performance more positive than with manager-rated outcomes
Peeters et al. (2006)	1997 – 2003 *k* = 28 (for variability)[c]	- Big Five personality traits	Various	Variety in agreeableness (ρ = -.12) and conscientiousness (ρ = -.24) predicts lower performance	Some small differences between student and professional teams, but main findings consistent
Webber & Donahue (2001)	1980 – 1999 N = 24 *k* = 45	- “less job-related diversity” - “highly job-related diversity”	Various (and cohesion as proximal outcome)	No significant relationships	No difference between top-management teams and lower-level teams
Bowers et al. (2000)	1961 - 1998 N = 13 k = 57	Ability, personality and gender	Various	Ability, personality and gender diversity not significantly linked to performance	Task difficulty: homogenous teams outperform on “simple” tasks, diverse teams on “difficult” tasks. Relatedly, homogenous teams outperform on production tasks low in cognitive demand.

[a] ρ refers to true-score correlations after corrections for measurement error and are reported where available.

[b] Only selected significant moderators are included here. Many others were tested, but due to the low power for almost all tests, null results are uninterpretable. Conversely, non-null results, which are not corrected for multiple comparisons, are likely to have very high false-positive rates.

[c] This paper addressed a wide range of factors that might explain team performance across a total of 104 studies. However, only a small fraction of them included measures of variability/diversity.

The present research

We conducted a comprehensive meta-analysis to test the link between team diversity and performance. In that, we focused on identifying moderators that shape the link between the key dimensions of diversity (demographic, cognitive and job-related) and team performance. Throughout, we did not merely focus on the statistical significance of effects – instead, we used equivalence tests to identify where there is evidence for a substantial association or evidence for *the lack of* a substantial association, and where the evidence is inconclusive (Lakens et al., 2018).[2] The analysis was guided by four research questions, each giving rise to some specific hypotheses:

RQ 1: Does team diversity predict team performance? How does this differ between the dimensions of diversity and the performance task under consideration?

In line with previous literature, we expected weak main effects of diversity on team performance on all three dimensions, yet through equivalence testing, we can determine whether this constitutes evidence for the absence of a substantial (rather than statistically significant) association. Therefore, we initially ask:

RQ1a: Is the link between diversity and team performance insubstantial (i.e., $|r| < .1$)? Does this differ between the dimensions of diversity?

In any case, we expected substantial heterogeneity, and hypothesize that this can partly be explained by characteristics of the performance task, primarily by its complexity, the required degree of interdependence and the importance of creativity, and particularly of divergence rather than convergence.

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[2] For that, we need to define the Smallest Effect Size of Interest (SESOI). This is inevitably subjective (and often left implicit), but we believe that the main effects of diversity on team performance are only of interest if they explain more than 1% of variance in team performance, i.e. when $|r| > .1$. Moderators, conversely, are only of substantial interest when they explain at least 5% of the (between-studies) heterogeneity in effect sizes, i.e. when $\Delta R^2_{Meta} >= .05$. Readers might disagree, and a supplementary online app will allow them to rerun the analyses with their own SESOI.

Regarding **task complexity**, it appears self-evident that more complex tasks rely more heavily on a group's cognitive resources. Given that the benefits of diversity are expected to come about because of the greater collective cognitive resources it brings (S. Page, 2019; Sulik et al., 2021), we hypothesize that

H1: Diversity has a substantial positive association with performance when the task is high in complexity.

While this appears particularly pertinent to the dimension of cognitive diversity, we expect this to hold over the three dimensions we consider.

Regarding **interdependence**, the theoretical expectations are less clear. Complementary cognitive resources (e.g., skills) might be particularly beneficial when team members work interdependently, while identity conflicts that raise communication barriers might be particularly harmful in such situations. However, in line with the contact hypothesis (Allport, 1954), the interactions that interdependence requires might improve intergroup attitudes. Indeed, while individual studies have suggested that interdependence harms the performance of demographically diverse teams (e.g., Timmerman, 2000), findings of a (small) meta-analysis suggest that interdependence improves the relationship between team diversity and team members' performance (Guillaume et al., 2012).[3] Therefore, we hypothesize that across all dimensions:

H2: Diversity has a more positive association with team performance when the task requires a high level of interdependence.

Furthermore, **the success criterion (determined by the type of task)** matters. Informational benefits of diversity are only likely to matter when a task requires some

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[3] This meta-analysis focused on the effects of team diversity on the performance and experience of individual employees, so that their evidence base and our evidence base are entirely distinct (unless studies report effects at both levels). Therefore, it is not included in Table 1.

creativity or problem solving (Sulik et al., 2021). There is no reason to expect that diversity per se would have any positive impact on pure production tasks, where both the output and the strategy are well defined. Therefore, we hypothesize that:

H3a: Diversity has a more negative link to performance in tasks that focus on maximizing production of an output with a pre-defined strategy.

Conversely, where creativity or problem-solving are required, the benefits of diversity are likely to be most pronounced when divergent thinking is needed, i.e. when the multitude and variety of ideas is of paramount importance. Conversely, conflicts are most likely to arise when convergence on a single best idea is needed, as different values and perspectives might result in conflicting evaluations (S. Page, 2019). Furthermore, convergence requires that team members effectively build on each other's contributions, which has been found to be negatively associated with diversity because cognitive diversity makes knowledge integration more challenging (Harvey, 2013). Therefore, we hypothesize that:

H3b: Diversity has a more positive link to performance in tasks where performance depends on creative divergence rather than convergence.

The second overarching research question concerned the macro-level context, where we asked:

RQ2: How does the relationship between diversity and performance differ across space and time?

Apart from Wei et al. (2015), meta-analyses and reviews to date rarely considered space, and none explicitly considered time. This limits our understanding regarding the generalizability of any findings. Therefore, we report on the association between diversity and performance for each world region. To begin to understand drivers of the expected differences, we ask:

RQ2a: Is the relationship between team diversity and performance related to a country's level of collectivism versus individualism?

Theoretically, this association might be expected to go either way. The presence of distinct identities might lead to greater conflict, and thus to reduced performance, where team cohesion is prioritized. Conversely, a focus on team cohesion might weaken the import of individual identities, and thus enable the effective use of cognitive resources. We are not aware of research that assessed this relationship in an intercultural context, so that we refrain from making a directional hypothesis.

Regarding time, we note that demographically-based intergroup bias has broadly declined, for instance when it comes to race, skin tone and sexual orientation in the United States (Charlesworth & Banaji, 2019), or to women's participation in the workplace across a range of countries (Charlesworth & Banaji, 2021). Similarly, diversity management has become widespread, aiming (among other purposes) to create conditions under which diversity contributes to performance (Köllen, 2021). Together, these developments lead us to hypothesize that:

H4a: The relationship between diversity (particularly demographic diversity) and team performance has become more positive over time.

Given this hypothesis, it appears likely that the main effects of the different dimensions of diversity have become positive. Therefore, we expect that:

H4b: The relationship between diversity and team performance is positive and substantial (i.e. $r > .1$) in evidence from the past decade (2012-2022).

RQ3: How do contextual factors influence the relationship of diversity with team performance?

In addition to the nature of the performance task, characteristics describing the team and its setting are also likely to shape the link between diversity and performance. Here, we consider both variables describing the culture of the team (i.e. diversity climate, psychological safety and authority differentiation) as well as those determined by the organization (i.e. teams' longevity and virtuality).

Diversity climate. The potential benefits of diversity are only likely to be realized when team members are willing to bring their unique perspectives to the table and when others are willing to learn from them (Ely & Thomas, 2001). This appears to be more likely in an organizational culture that explicitly values diversity, i.e. that has a positive diversity climate (Goyal & Shrivastava, 2013). Accordingly, individual studies have suggested that a positive diversity climate improves the association between team diversity and performance (e.g., Kadam et al., 2020; Moon & Christensen, 2020), yet this has not been tested meta-analytically. Therefore, we hypothesize that:

H5: Diversity has a more positive link to performance when the team works in a context that has a positive diversity climate.

Psychological safety. When individual team members do not dare to engage in counter-stereotypical behaviors, identity-based conflict is exacerbated and the potential for cognitive benefits reduced. A recent review of the dynamics within diverse teams has suggested that stereotyping processes such as this might explain the mixed and somewhat disappointing results in the diversity literature (van Dijk et al., 2017). While stereotyping has rarely been measured in team diversity research and can thus not yet be meta-analyzed, van Dijk et al. (2017) proposed that psychological safety might make it easier for team members facing stereotypes to act in counter-stereotypical ways, and thus improve the association of diversity with team performance. Furthermore, psychological safety is a key predictor of team performance per se (Newman et al., 2017). Therefore, it appears valuable to assess how it interacts with diversity. Individual studies appear to point in a positive direction, for instance finding that psychological safety improves the link between nationality diversity and performance (Kirkman et al., 2013) or the link between team cognitive diversity and innovation (Cho, 2022), even though some studies yield mixed results (Martins et al., 2013). Therefore, we hypothesize that:

H6: Diversity has a more positive link to performance when teams experience high levels of psychological safety.

Authority differentiation. Decision-making power can be variously distributed within a team. In the case of high authority differentiation, some team members have the authority to make decisions on behalf of their team, while low authority differentiation corresponds to more consensual decision making (Hollenbeck et al., 2012). Based on the finding that authority differentiation increases the importance of trust in teams (B. A. De Jong et al., 2016), and the common finding that trust is harder to build in diverse teams (Ertug et al., 2022), we hypothesize that:

H7: Diversity has a more positive link to performance when the team is low in authority differentiation than when it is high in authority differentiation.

Team virtuality. Given the recent rise of remote and hybrid working, possible effects of team virtuality need to be considered. Team virtuality is here understood as the degree to which face-to-face collaboration is restricted because team members work in different places or at different times (B. A. De Jong et al., 2016). Evidently, teams higher in virtuality need to place greater reliance on communication methods that limit the transmission of non-verbal cues, which can increase communication difficulties (Miles & Hollenbeck, 2013). Such difficulties might be expected to both exacerbate identity conflict and reduce the benefit of the combination of diverse cognitive resources. Likely for the same reason, it has been shown that trust is particularly important in virtual teams (Breuer et al., 2016), which is (at least initially) harder to build in diverse teams due to homophily (Ertug et al., 2022). Conversely, it has been suggested that the more limited range of cues communicated virtually might decrease social categorization and thus identity-based conflict (Staples & Zhao, 2006), thus weakening this negative pathway. Accordingly, a small-scale meta-analysis found that team dispersion was associated with *less* conflict and *greater* social integration in diverse teams (Stahl et al., 2010). However, due to the limited sample they could not test for a link with performance. Considering these contradictions, we ask:

RQ3a: How does the link between diversity and performance differ depending on teams' level of virtuality?

Longevity of the team. Given that diversity might trigger identity conflicts, it appears likely that the link to performance depends on the longevity of the team. However, the direction of that relationship is unclear: short-lived teams might be better suited to focus on harnessing the diverse cognitive resources while ignoring demographic fault lines, which become more problematic in longer-lived teams (A. Joshi & Roh, 2009). Fault lines may even only emerge after process failures occurred for unrelated reasons which are then attributed to demographic differences (Srikanth et al., 2016). Conversely, long-lived teams might have more opportunities to interact, get to know the individuals beyond the stereotypes and thus to reduce intensity of intergroup conflict and thereby the negative effects of diversity (Choi & Jarrott, 2021). Correspondingly, the empirical evidence has been mixed, with some studies finding negative effects of team longevity (e.g., Boerner et al., 2011a; Schippers, Den Hartog, et al., 2003), others finding positive effects (e.g., Kearney et al., 2009; Pelled et al., 1999), and some finding no relationship (Kearney & Gebert, 2009). Therefore, we ask:

RQ3b: How does the link between diversity and performance differ depending on the longevity of a team?

RQ4: How do methodological choices influence the relationship of diversity with team performance?

In order to understand the state of the evidence, to identify potential limitations on its reliability and to inform future research, we test whether important methodological factors influence the observed effect sizes. In that, we consider the effect of measurement choices. Regarding the **measurement of performance**, a key decision is whether performance is measured objectively (e.g., as the winning percentage of sports teams, Timmerman, 2000) or subjectively, by asking team members (e.g., Liao & Long, 2016) or their supervisors (e.g., Kearney et al., 2009) to rate their performance. Objective measures might be influenced less by (positive and negative) expectations regarding the effects of team diversity and, accordingly, one meta-analysis has found that their relationships with team performance are weaker (van Dijk et al., 2012). However, another meta-analysis found stronger relationships between objectively-measured performance and job-related diversity (Hülsheger et al., 2009), which is in line with the suggestion that diverse teams underestimate their performance due to the (productive) friction they encounter (Phillips et al., 2009). However, objective measures are easier to implement for some types of performance, so that an exclusive focus on objective measures (or a simple subgroup comparison between subjective and objective measures) would ignore that, e.g., productivity is more likely to be measured objectively and creativity to be measured subjectively. Therefore, we do not restrict the analyses to objective measures, and believe that any impact of measurement choices can only be meaningfully assessed when controlling for the type of performance. Since this has not been done to date, we refrain from stating a directional hypothesis. Instead, we ask:

RQ4a: How does the link between diversity and performance differ depending on whether performance is rated subjectively or measured objectively?

Regarding the **measurement of diversity**, we deem it important to distinguish measures that focus on variety from those that measure separation, as they reflect different theoretical emphases (Harrison & Klein, 2007).[4] Variety might be operationalized as the number of categories (on categorical variables) or the range (on numerical variables) present within the team, while separation also considers the distance between team members, conceptualized as the relative share of categorical groups, or the standard deviation on a numerical variable. Theoretically, one might expect that variety more closely predicts the breadth of collective cognitive resources, while separation more closely predicts the potential for the emergence of fault lines within the team. Correspondingly, Bell et al.'s (2011) meta-analysis found that job-related diversity as variety had a more positive relationship with performance than diversity as separation did, which Wei et al.'s meta-analysis (2015) confirmed across diversity dimensions. Therefore, we predict that:

H8: Diversity will have more positive associations with performance where it is measured as variety rather than separation.

Furthermore, we look for **indications of effect size inflation**. These might arise because publication bias is prevalent in economics (Andrews & Kasy, 2019), social psychology (Lovakov & Agadullina, 2021) and organizational psychology (O'Boyle et al., 2014) and it appears likely that Questionable Research Practices that inflate effect sizes are used with some regularity (Kepes et al., 2022). Similarly, at least the field of social psychology is made up largely of politically progressive researchers, who might be motivated to find and highlight positive effects of diversity, and thus fall prey to confirmation bias and

[4] We list this as a methodological rather than substantive moderator since researchers rarely justify their choice to measure diversity as variety (e.g., range) or separation (e.g., standard deviation). Evidently, the result can both inform research practice and the interpretation of results.

related processes (Duarte et al., 2015). Given that these pressures are most likely to apply to the main hypotheses in a paper, we hypothesize that:

> **H9**: Studies where the link between diversity and performance is the focal hypothesis will report larger (H8a) and more positive (H8b) effect sizes than studies where this is an auxiliary or descriptive result.

To further understand the potential impact of publication bias, we will also test for differences in effect sizes between published and unpublished studies, in addition to dedicated analyses assessing the presence of publication bias outlined in the methods. Given that only experimental and quasi-experimental research can yield evidence for a causal relationship of diversity with performance, we will compare the effect sizes obtained from observational, quasi-experimental and experimental studies. Their divergence, if any, can help inform both the interpretation of results and the shape of future research.

As an exploratory analysis related to the assessment of the evidence base, we will test whether the number of citations of an article is correlated with its effect size and with its level of significance. Seeing citations as an indicator of the visibility of evidence within the scientific community, the former would indicate that the evidence is seen as more positive than it is, while the latter would indicate that the evidence is seen as less uncertain than it is.

Coding of moderators and additional analyses

In addition to the moderators discussed so far (summarized in Table 2), we coded the specific types of diversity (e.g., race/ethnicity, function, values) and report their associations with effect sizes whenever there were at least five observations per cell. We also coded further exploratory moderators that were identified during the literature search and report on their relationship with the observed effect sizes in the section on exploratory analyses.

Table 2. *Hypothesized moderators and their levels*

Moderator	**Coding / levels**
Diversity dimension (further sub-categories may be added during coding)	Demographic (age, gender, race/ethnicity, nationality, sexuality) Cognitive (educational level, degree, values, personality, intelligence, neurodiversity[a]) Job-related (function, tenure)
Time	Year of data collection (if reported), otherwise year of publication
Task complexity	High / medium / low
Interdependence	High / medium / low
Country[b]	Country (+ multinational)
Success criterion	Divergence (e.g., many ideas) / Convergence (e.g., best idea) / Production (of pre-defined product) / Other
Diversity climate	Positive (> midpoint on measurement scale *or* experimentally induced) Negative (< midpoint on measurement scale *or* experimentally induced) Not reported
Psychological safety	High (> midpoint on measurement scale *or* experimentally induced) Low (< midpoint on measurement scale *or* experimentally induced) Not reported
Authority differentiation	High / mixed / low
Team longevity	Unit best describing lifespan of team until performance was measured: hours/days/weeks/months or years
Team virtuality	Virtual (i.e., no routine face-to-face interaction) Hybrid-work (i.e., alternating virtual and physical interaction, with >=20% each) Hybrid-members (i.e., some team members are co-located, others fully remote) Physical (i.e., co-located, with routine face-to-face interaction)
Diversity measure	Variety / Separation / Other
Performance measure	Objective / Subjective (by team members) / Subjective (by team leader) / Subjective (by external rater)
Study design	Observational / experimental / quasi-experimental[c]
Article focus	Is link between diversity and performance: focal hypothesis / auxiliary hypothesis / descriptive result
Citation count	Retrieved from Google Scholar (as no other sources covers all included languages)

[a] Neurodiversity evidently spans across the demographic and cognitive dimensions. Therefore, we planned to run all analyses without effect sizes concerning neurodiversity and report on any differences as robustness checks – yet we found no literature linking neurodiversity with team performance.

[b] Countries' levels of individualism vs collectivism were then taken from Hofstede's cultural dimensions data matrix (Hofstede, 2015)

[c] The quasi-experimental category encompasses any techniques that lack randomization but aim to estimate causal effects, such as difference-in-differences, propensity-score matching and related approaches.

Methods

Open science and disclosures

Recent analyses have highlighted that too many meta-analyses are not reproducible (Polanin et al., 2020) and that a lack of pre-registration affords researchers high degrees of freedom that can inflate the false discovery rate (Gelman & Loken, 2014). Therefore, we chose the path of a registered report to increase transparency and robustness, building on a template by Fillon and Feldman (2021), and follow the APA JARS reporting standards throughout (Appelbaum, 2018). We also make all data and analysis code available, so that others can reproduce and build on our work, particularly when it comes to extending and updating the evidence base (Lakens et al., 2016). In addition, we provide a web app that allows readers to explore the impact of changes to inclusion/exclusion criteria and model specifications, and to rerun analyses with updated data.

We share all procedures, materials, datasets, and analysis code on the Open Science Foundation (https://osf.io/hpsz8/?view_only=ac39b1cd3759402bba246ec81968604c), and in Supplementary Materials (https://anonymous.4open.science/w/diversity_meta-5DC0/). Systematic data collection did not begin prior to the acceptance of the registered report. There are no other unreported or unlinked pre-registrations for this meta-analysis project.

Eligibility criteria

Studies reporting associations between team diversity and team-level performance were included in our analysis if they were accessible to our searches concluded on [20/01/2023]. In that, we included studies that addressed diversity in terms of demographic, cognitive or job-related factors. We restricted our focus to studies that reported performance measures at the team level, except for studies concerning top-management teams, where we included studies that correlated their diversity with organizational performance. We excluded studies that purely considered diversity as disparity (e.g., status, authority, salary), or that only included outcomes that reflect team processes rather than performance (e.g., satisfaction, retention, etc.). Furthermore, we excluded studies concerned with perceived rather than measured diversity and with median team sizes below 3 (as dyads constitute an interpersonal rather than intergroup context) or above 25 (as the members of such "teams" are unlikely to be able to personally interact with each other on a regular basis, cf. van Dijk et al., 2012, who used the same team size criteria).

Because we were interested in the impact of diversity within teams in the workplace, we excluded any studies that used student samples, unless their performance measures were clearly organizational (e.g., the performance of student-led start-ups). Furthermore, we excluded any studies that did not report sufficient data to extract or calculate Pearson's *r* and the sample size and where the authors did not provide either these details or the raw data upon request.

Lastly, retracted studies pose a challenge for meta-analyses that is too often overlooked (Fanelli et al., 2021). Given that we include unpublished manuscripts in the meta-analysis and that some retractions are due to factors that do not raise doubts about the reliability of the descriptive statistics, we did not exclude all retracted studies. However, we excluded any studies that were retracted due to concerns with the data and report the impact of the remaining retracted studies on our conclusions in the section on robustness checks.

Search strategy

Database searches. To identify articles that were potentially relevant to our topic of investigation, we searched the most relevant electronic databases, namely PsycInfo, Business Source Premier and Google Scholar (the most comprehensive source in the Social Sciences and in Management, Martín-Martín et al., 2021). To include further unpublished literature, we accessed OpenDissertations, NDLTD and the Social Sciences Research Network (SSRN).

The use of Google Scholar poses some special challenges; details on our search strategy there are included in SM 1a.

For team diversity, we used the following keywords: *diverse, diversity, heterogeneous, heterogeneity, individual differences* and *team composition*. For team performance, the keywords were *performance, productivity, creativity, innovation* and *effectiveness*. Finally, we included *team* or *group*.[5] In order to move beyond the English-language evidence, we ran translated searches on Google Scholar in 12 additional languages, namely Chinese, Japanese, Korean, Indonesian, German, French, Spanish, Italian, Portuguese, Polish, Russian and Ukrainian. Since there appears to be no data on publication languages for social science research, these were based on the intersection of the languages included in the Google Scholar Metrics (since this appears to indicate substantial coverage of that language) and the Top 10 languages found with our search string in PsycInfo and Business Source Premier. Details and translated search strings can be found in SM 1.

Further searches. We also extracted all articles included in previous meta-analyses on the diversity-performance link and related questions (Bell et al., 2011; Bowers et al., 2000; Bui et al., 2019; Horwitz & Horwitz, 2007; Hülsheger et al., 2009; A. Joshi & Roh, 2009; Peeters et al., 2006; Stahl et al., 2010; Triana et al., 2021; van Dijk et al., 2012; Webber & Donahue, 2001; X. Wei et al., 2015), as well as those cited in narrative reviews (Bunderson & Van der Vegt, 2018; Sulik et al., 2021; K. Williams & O'Reilly, 1998; Yadav & Lenka, 2020). Then we extracted the reference lists of all articles selected for inclusion (using *GROBID*, 2008), as well as all articles citing one of the previous meta-analyses (using both Scopus and OpenCitations). Finally, we systematically contacted the 579 authors of the

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[5] Therefore, the following search pattern was our main string: *(diverse OR diversity OR heterogenous OR heterogeneity OR "team composition" OR "individual differences") AND (team OR group) AND (performance OR creativity OR productivity OR innovation OR effectiveness).*

articles identified for inclusion to ask for further sources, particularly unpublished ones, and issued a call for unpublished findings on Twitter and Mastodon to find further relevant data.

Screening of studies

All results were exported from the respective databases and loaded into R, using a largely automated retrieval process (described in the analysis code, C1). They were then de-duplicated using the ASySD package (Hair, 2022), which shows best-in-class performance based on our benchmark (SM 1B) against the results of a recent review (McKeown & Mir, 2021). The results from searches in other languages were automatically translated using the Google Translate API.

After deduplication, the screening of results took place based on title and abstract using an approach assisted by machine-learning in the *ASReview* software. By dynamically sorting results based on their similarity to results included so far, it achieves 100% recall after screening between 7.4% and 58.6% of records based on simulation studies (van de Schoot et al., 2021). We used this to initially screen 25% of records and then continued for as long as at least 1 in 50 was included for full-text screening (this should result in at least 98% recall at substantial time saving). After screening the search results, we used the same approach to screen the references contained in the articles identified for inclusion. Given that machine translations might result in substantially different terminology, we conducted this process separately for each language. The percentage of articles screened manually was 27.7% in English and ranged from 25.3% to 42.6% for the remaining languages.

In the final step, the methods and results sections, as well as tables and figures of the candidate article identified in the previous steps were screened to decide on inclusion. The PRISMA flow diagram summarizing the process can be found in Figure 1; the articles included in the meta-analysis can be found in the reference list where they are marked with an asterisk. The full list of articles (after deduplication) that were screened can be found in the Supplementary Materials. In total, we found 70,327 records before deduplication, representing an estimated 50,000 unique sources,[6] out of which 615 were eligible for inclusion.

[6] The registered approach to automated deduplication identified only 3,625 duplicates. However, manual deduplication of the entries selected for full-text screening suggested that an additional 14.1% of records retrieved from databases may be duplicated. When it comes to backwards citations, the share of duplicates that could not be identified automatically is likely to be higher due to the limited consistency of data extraction, so that we estimate that approximately 25% of the original results will have been duplicated.

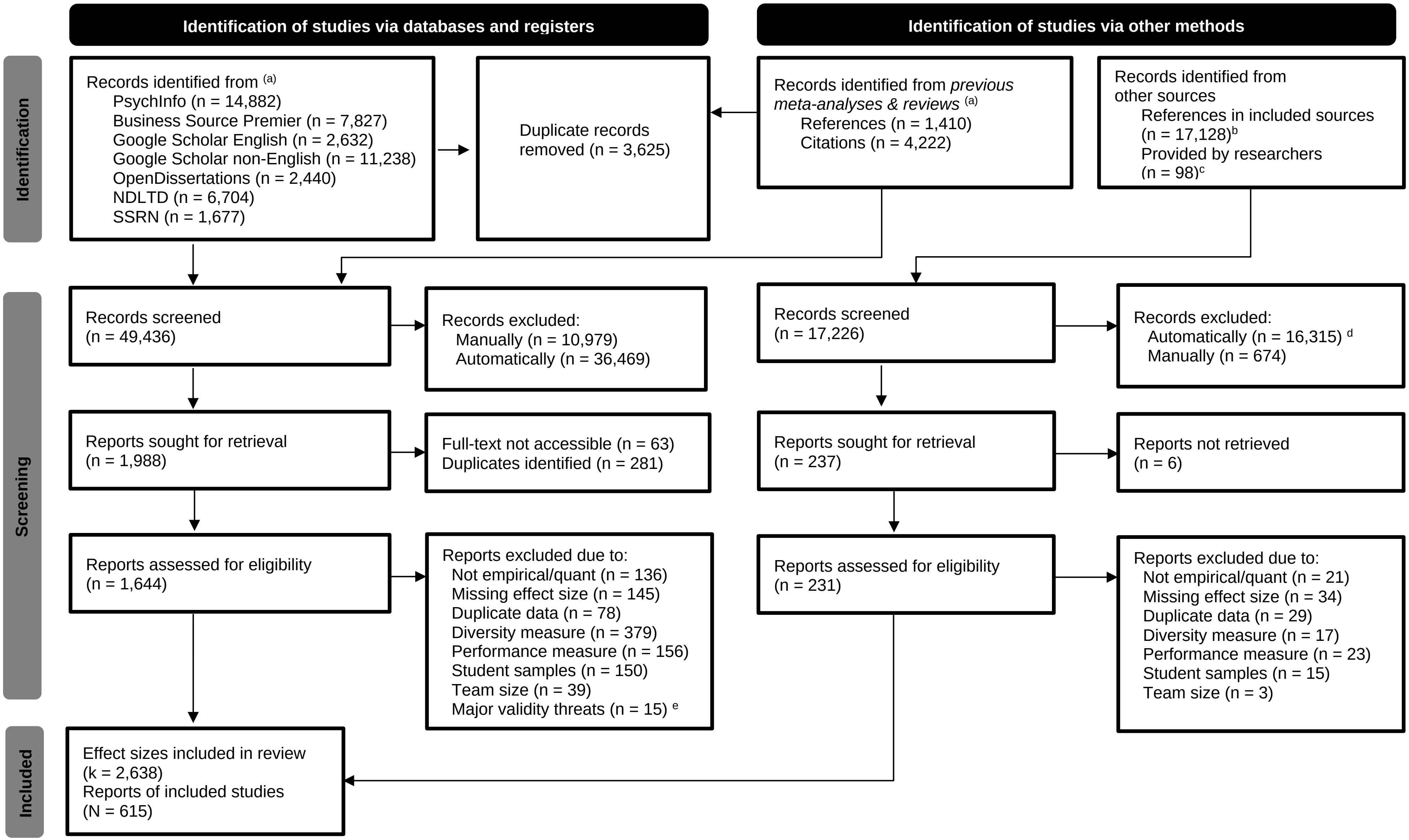

Figure 1. Search and selection flow diagram in accordance with PRISMA 2020 (M. J. Page et al., 2021)

[a] After internal duplication within the listed data sources. [b] After automatic deduplication within these references, and with the databases. However, poorer data quality here implies that a larger share of duplicates remains, only to be identified after the screening. [c] After deduplication (done continuously as articles were submitted). Includes 78 sources (mostly in Chinese) shared by the authors of a recent Chinese meta-analysis (C. Ma et al., 2022). [d] This screening process is detailed in SM 1C. In short: abstracts were added to records in as far as possible, then duplicate title-abstracts were treated as duplicates (due to inconsistent author extraction, resulting in 957 exclusions). Then we automatically screened the remaining entries using GPT 3.5 and running the original search query against them. [e] Mostly due to diversity measures that are very closely related to team size (e.g., counts of categories in teams of widely varying sizes) or to selective reporting, where reports state that only the significant relationships were reported. For details, see coding sheet (SM 1D).

Coding

Data extraction from the included studies was conducted on the final pass of the screening process. For each study, all available correlations between team diversity and measures of performance were extracted, as well as details on the measures for diversity and performance, their reliabilities, the sample size and all candidate moderators.

Initially, 25 studies were coded by both authors, any discrepancies were resolved through discussion and used to clarify the coding sheet. Then both authors coded an additional 20% of all studies. We registered that if overall agreement was found to be below 95% and agreement on the coding of any moderator below 80%, all studies were double-coded, either entirely or with regard to the affected moderators. Otherwise, Author 2 would complete the remaining coding. All authors hold graduate degrees in social psychology or social research, and have considerable experience in reading, reviewing and conducting research.

Coding of the moderators. Moderators were coded based on the categories listed in Table 2. In the case of task complexity and task interdependence we followed the coding rules used by Kleingeld et al. (2011). For *task complexity*, we relied on forming analogies between the performance tasks to Wood's (1986) task complexity scale for individual tasks. *Low* complexity referred to tasks that succeeded based on criteria such as reaction time or brainstorming output. *Medium* complexity referred to more demanding tasks, such as anagrams or sewing machine work. Finally, *high* complexity, finally, referred to more specialized tasks, such as technical work, or scientific tasks. For *task interdependence*, we classed tasks in which performance was pooled or sequential as *low*, where it was reciprocal (i.e., involved turn-taking) as *medium* and those where interaction was more intensive as *high* in interdependence. Examples for these levels are included in SM 1E.

Analysis

We used *R* 4.3.2 (R Core Team, 2022) for statistical analyses, primarily relying on the *rcrossref* (Chamberlain et al., 2021) and *pybliometrics* (Rose & Kitchin, 2019) packages for the literature search, the *ASySD* (Hair, 2022) package for deduplication, the *metafor* package for conducting the meta-analysis (Viechtbauer, 2010) and the *clubSandwich* package for robust variance estimation to handle dependent effect sizes (Pustejovsky, 2022). Data processing and visualization continuously relied on the *tidyverse* package suite (Wickham et al., 2019). We used the *groundhog* package (Simonsohn & Gruson, 2023) to reproducibly use all package versions as of July 9, 2023. Finally, we used *metaUI* (Wallrich & Röseler, 2024) to create an interactive webapp that allows readers to further explore results.

Given the range of different facets of both diversity and performance, as well as the results of past meta-analyses, we expected the heterogeneity in the sample to be relatively high. Thus, a random effects model with a REML estimator was used for all the relationships (Gonzalez-Mulé & Aguinis, 2018).

Effect sizes

We used Pearson's *r* as the main indicator of effect size. Whenever available, we used correlations obtained directly from original papers, or converted equivalent effect sizes (such as Cohen's *d* or odds ratios) using the formulae provided by the Campbell Review (Polanin & Snilstveit, 2016). If only regression weights were reported, we converted them to correlations using the method outlined by Harrer and colleagues (2021). As a last resort, we contacted the authors to request correlation coefficients or raw data.

Correlations were corrected for measurement error by using the formula $r_c = \frac{r_{obs}}{\sqrt{r_{xx'}}\sqrt{r_{yy'}}}$. Correspondingly, sampling error variances were adjusted as follows: $SE_{r_c}^2 = SE_{r_{obs}}^2 * \left(\frac{r_c}{r_{obs}}\right)^2$ (Wiernik & Dahlke, 2019). Even though indices of internal consistency (e.g., Cronbach's alpha) only capture one source of measurement error, these are usually the only reported form of reliability in the fields this meta-analysis draws on. Missing reliability estimates were bootstrapped (i.e. sampled with replacement) from within the same category of diversity/performance measures and scale length (categorized based on the number of scale items into terciles of short, medium and long scales). Single-item measures were not corrected.

Dependent effects

Frequently, studies report more than one relevant effect size derived from the same sample, for instance when different facets of diversity (e.g., multiple personality traits) were considered. Evidently these results are not independent, so that an assumption of traditional meta-analysis models is violated. Therefore, most meta-analyses concern themselves with averages (e.g., van Dijk et al., 2012) or linear combinations of effect sizes (e.g., Triana et al., 2021). However, there is a consensus in the methodological literature that this reduces statistical power and risks introducing bias, so that *all* effect sizes should be used in evidence aggregation (Tipton et al., 2019). This requires the use of newer meta-analytic models that take the dependence into account. These use either reported or assumed correlations between effects sizes obtained from the same sample to correct standard errors. Here, in line with Harrer (2021), we used the correlated and hierarchical effects (CHE) working model for the meta-regression (Pustejovsky & Tipton, 2021) and assumed a correlation between effect sizes within the same study of .6. Then cluster-robust standard errors provided by the *clubSandwich* package were used for all inferences about average effects (Pustejovsky, 2022).

Decision-tree approaches such as meta-CART, which supplement meta-regressions in our analyses (see below) cannot yet handle dependent effect sizes. Therefore, we created linear combinations of effect sizes within the same sample where all moderators had the same value, and then randomly sampled one of the combined effect sizes per sample for further analyses (e.g., if a sample yielded correlations based on two subjective and two objective measures of performance, we would create separate linear combinations for the subjective and objective measures, and then randomly sample either the subjective or objective measure for further analysis.)

Heterogeneity and moderation

Initially, we conducted the Q-test to assess the presence of heterogeneity across the effect sizes and discuss both the 80% credibility interval and the I^2 index as they provide distinct information regarding the amount of heterogeneity (Gonzalez-Mulé & Aguinis, 2018). When assessing whether the hypothesized moderators explain a share of this heterogeneity, we then needed to account for associations between moderators. This is frequently ignored in meta-analyses, when a series of separate subgroup analyses is presented, yet that is akin to presenting a sequence of correlations rather than a multiple regression when testing multiple predictors in a primary study. To that end, we used two complementary methods here: (multi-level) meta-regression and meta-CART.

Meta-regression is akin to multiple regression in that it estimates how different predictors affect the observed effect size while controlling for all other predictors (Gonzalez-Mulé & Aguinis, 2018). *meta-CART*, on the other hand, results in decision trees that iteratively split the sample on one of the predictor variables until homogenous parcels of effect sizes are obtained (X. Li et al., 2017). This allows one to draw conclusions about *combinations* of moderators that result in high/low observed effects without having to specify interaction terms in meta-regression, which cannot be estimated with reasonable power within the usual constraints of a meta-analysis. (In the meta-regression, we only specify interaction terms between each moderator and the diversity dimensions to assess to what extent results differ for demographic, cognitive and job-related diversity. Similarly, we will run separate meta-CART models for demographic, cognitive and job-related diversity to ensure that differences between these dimensions become evident.)

Both approaches rely on complete data on moderators, which is rarely given in meta-analyses. Instead, moderators can often only be coded for specific studies as the context might not be described in sufficient detail in others. Typically this results in subgroup analyses that are performed on different sets of studies, or in the exclusion of studies with missing data, which evidently carries a great risk of bias (Tipton et al., 2019). To do better, for both meta-regression and meta-CART, missing values need to be filled in. Generally, the best procedure for dealing with missing data is multiple imputation, in that it can result in unbiased estimates across a wide range of situations (Rubin, 2004). Its use has been advocated specifically in the context of meta-regression (Ellington et al., 2015) and some studies have started to use it (Hedger et al., 2016). Thus, we used it for the meta-regression. However, this is not possible for meta-CART as multiple decision trees cannot be combined analytically. Instead, we followed Hedger and colleagues (2016) and estimated a best-case and a worst-case model. For the best-case, missing data was imputed with the correlations obtained from the observed data, which will under-estimate standard errors. For the worst-case, conversely, missing data was imputed with values randomly selected from the observed values, which will over-estimate standard errors. The results of both analyses are reported, and only common patterns are treated as clearly supported by the evidence.

Exploratory Analyses

We expected to include more variables that are not listed in the pre-registered coding sheet as possible moderators as we examine the literature. During that stage, we added teams' industry and function, as well as the specific diversity measure used. Analyses involving these are presented separately in the section containing exploratory results, as well as analyses diving deeper into the specific performance measures. In that section we also discuss any evidence regarding non-linear relationships between diversity and performance.

Publication Bias

Before assessing publication bias, we corrected for measurement error as outlined above since this can bias any test of publication bias (Wiernik & Dahlke, 2020). We then had to decide how to deal with dependent effect sizes, given that most methods to detect publication bias rely on independent effect sizes and that ignoring the dependence leads to drastically inflated Type I error rates (Rodgers & Pustejovsky, 2021). In line with the simulation results and recommendations by Rodgers and Pustejovsky, we used two methods to test for publication bias. Firstly, we used an Egger's regression test to assess the asymmetry of the funnel plot, with Robust Variance Estimation (RVE) taking care of dependence between effect sizes. In order to strike an appropriate balance between statistical power and Type I errors, we followed the common practice highlighted by Siegel and colleagues (2021) and interpreted *p*-values below .1 as evidence for publication bias. Secondly, we used the 3-parameter selection model (3PSM) to directly estimate whether non-significant results have a lower chance of being published than significant findings. This cannot presently be extended to account for dependent effect sizes but sampling one effect size per sample results in a test that combines comparatively high power with a predictable Type I error rate. Therefore, we bootstrapped 3PSM with effect size sampling, and report the median results and distribution of 5,000 bootstrap resamples. Given that an alpha level of .05 is associated with a Type I error rate of up to 10%, we relied on this threshold (Rodgers & Pustejovsky, 2021).

Analyses of publication bias become less reliable in the presence of heterogeneity – and are ultimately also less informative. Therefore, we report separate assessments for each dimension of diversity (demographic, cognitive, job-related). Also, we restrict our analysis of publication bias to published studies (while results for our full meta-analytic sample are reported in the Supplementary Materials).

Statistical sanity checks

Simple statistical checks can be used to identify some instances of misreporting and thus help calibrate trust in the reliability of specific sources. Where means and standard deviations are reported based on integer measurements (e.g., Likert scales), the GRIM (Brown & Heathers, 2017) and GRIMMER (Anaya, 2016) tests can identify impossible means and standard deviations respectively. These were used to validate any measures of diversity or performance based on a sample size below 500 and derived from either a single integer measurement or a scale of at most three items, based on the functions implemented in the *rsprite2* package (Wallrich, 2021). Similarly, the *statcheck* R package was used to identify possible instances of misreporting of statistics (e.g., instances where reported *t*-values and *p*-values are incongruent), as proposed by Nuijten and Polanin (2020). As a robustness check, all analyses were repeated without effect sizes flagged by either of these methods, and divergences reported.

Procedural clarifications and deviations

During the process of conducting the meta-analysis, we had to clarify some aspects of the protocol, and modify others. We do not expect that any of these reduced the severity of our tests. Here, we report the clarifications and deviations by stage:

Search and screening

- We did not contact authors of papers more than 20 years old to request details as we deemed it unlikely that they would still have access to the data – and as it would have been difficult to obtain current contact details in most cases (in line with Reimer & Sengupta, 2020).

- Initially, we did not specify how to screen references in included papers. As automated extraction resulted in 18,000 records, at most a very cursory title screening would have been possible, and ASReview has not been validated for titles only. Therefore, we pursued a two-pronged approach: we ran our search query over the references, and used the Open AI API to assess whether references might refer to empirical work on the diversity-performance link. Candidate references identified by either route were then manually screened. The full approach can be seen in SM 1C.

Coding

- After double-coding an initial 20% of English-language studies, we failed to achieve the required interrater agreement that we had registered as a condition for single-coding – however, many deviations were due to systematic differences that seemed fixable through a clarification of coding rules. Therefore, we clarified the coding rules and double-coded another 20% of English-language studies. At that point, we achieved the pre-registered required interrater reliability, with over 80% agreement on all variables and 92.4% agreement overall, so that we proceeded with a single coder.
- When it came to design, we dropped the quasi-experimental category. While some studies aimed to derive causal estimates (e.g., with instrumental variables), the reported *correlations* are still purely observational in these cases, so that it would not make sense to test whether they differ from other observed correlations.
- A substantial number of studies used the percentage of minority group members as an indicator of diversity. We only included these only if the ‘minority’ group was in the minority in most of the teams, so specifically if the mean + 1 standard deviation of the percentage was below 50%.
- For task complexity, we had planned to code by analogy to Wood’s (1986) task complexity scale. However, this proved to be too ambiguous to support reproducible

coding. Therefore, we operationalized task complexity using the level of education or professional expertise required to perform the task, split into *low* (requiring no tertiary education and limited professional expertise), *medium* (requiring undergraduate education or substantial professional expertise), and *high* (requiring postgraduate education or extensive professional expertise). Note that the inclusion of professional expertise led us to classify professional sports teams as engaging in tasks high in complexity, so that this is not merely a coding of educational requirements. Overall, it appears to tap the same levels as the registered definition but achieves adequate reliability.

Analysis

- In our specification of the metaCART approach, we failed to appreciate that the method is based on cross-validation, and thus subject to random variations between runs. Therefore, we sought guidance from the developers of the method (E. Dusseldorp, personal communication) and followed their advice to run the model 50 times and then select the modal number of nodes. As we were doing that, it seemed to make sense to also move away from drawing a single sample of independent effect sizes to reduce the influence on randomness, so that we ran 50 trees on each of 10 different samples drawn from our dataset.
- We planned to use the 3-PSM to consider publication bias. However, this would have been mis-specified as it tests whether selection occurs based on both significance *and* direction. Given that we expected positive and negative correlations, we used an extension of the selection model that allows for selection for significance *regardless* of direction.
- We did not consider how to deal with sample sizes in special circumstances, specifically for repeated measures, and for samples of outputs produced by

overlapping teams. In either case, using the reported sample size would not do justice to the weight of the evidence, under-estimating it in the former and over-estimating it in the latter case. Therefore, we calculated an effective sample size for studies using repeated measures (for categories where we could estimate a meaningful year-on-year correlation, see SM 2A for details) and capped the sample size of the (very large) studies that sampled team outputs such as patents or articles rather than teams to be in line with the largest samples of teams.

Results

Sample description

The present meta-analysis is based on a diverse sample of 2,638 effect sizes, derived from 646 samples. Figure 2 presents the breakdown by location, industry section, function and diversity sub-domains. It is worth noting that a majority of studies come from the United States (32%) and China (21%), with the remainder spread fairly widely, mostly across industrialized countries (see Panels A and B). Nearly half of the studies (48%) concerned management teams, which were mostly top management teams (89% of management teams, and thus 43% of the dataset), with the remainder spread over a wide range of functions (see Panel C). When it comes to the specific sub-domains of diversity, it is worth noting that demographic diversity largely referred to gender (38%) and age diversity (35%) rather than race/ethnicity (9%), while cognitive diversity predominantly referred to educational levels (32%) and degrees (28%). Job-related diversity, finally, largely concerned diversity in tenure (42%) and function (41%, see Panel D).

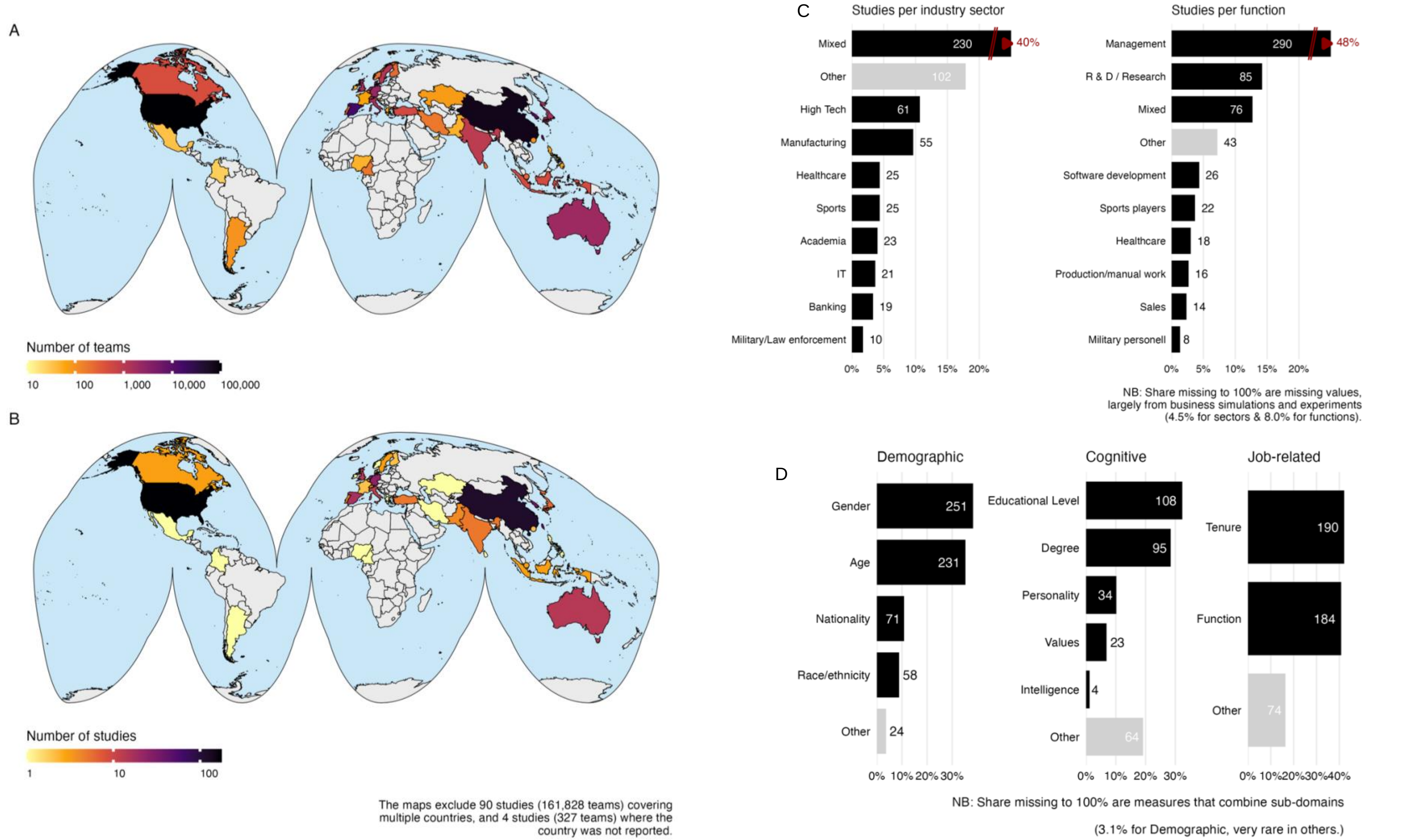

Figure 2. Overview of the sample by location (Panels A & B), industry sector and function (Panel C), and diversity sub-domain (Panel D).

Main effects

The random-effects meta-analysis with robust sandwich standard errors showed that diversity (across all dimensions) had significant positive associations with team performance (see Table 3). However, based on the registered smallest effect size of interest, these associations were insubstantial, as the hypothesis that they were smaller (in absolute value) than .1 was supported for all domains, with *p*s < .001. Also, while the average correlation differed significantly between diversity domains, $F(2, 2635) = 5.84$, $p = .003$, this association was insubstantial, $R^2 = 0.2\%$ [0.00%, 0.72%] and significantly below the pre-specific smallest effect size of interest of 5%, $p < .001$. When considering the results per sub-domain (shown in Figure 3), it is worth noting that no sub-domain showed a substantial correlation between diversity and performance. The estimated correlations were significant and insubstantial (i.e. significantly below an |r| of .1) for diversity in gender, nationality, degree, function and tenure, not significant and insubstantial for diversity in age, race/ethnicity, educational level and values, and inconclusive for the remaining sub-dimensions (personality and intelligence).

Table 3.

Relationship between diversity and team performance as per RE RVE meta-analysis

			Equivalence tests		
Domain	***k***	***r***	|*r*| < .1	|*r*| < .05	**80% Credibility interval**
Overall	2,638	.024 [.015, .033] ***	< .001	< .001	[-0.167, 0.215]
Demographic	1,105	.014 [.001, .026] *	< .001	< .001	[-0.178, 0.205]
Cognitive	747	.020 [.001, .039] *	< .001	.001	[-0.171, 0.212]
Job-related	786	.042 [.025, .058] ***	< .001	.161	[-0.150, 0.233]

*Note*s. Values in square brackets following *r* indicate 95% confidence intervals. All significance tests and intervals are based on cluster-robust standard errors to account for clustering of effect sizes within samples.

† *p* < .1, * *p* < .05, ** *p* < .01, *** *p* < .001

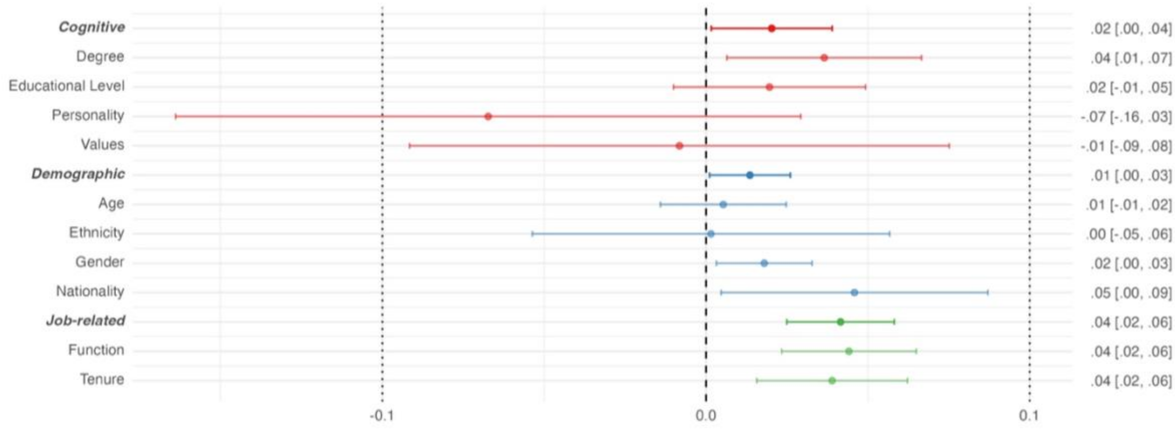

Figure 3. Estimated correlation between diversity and team performance depending on diversity domain and sub-domain. Error bars show 95% confidence intervals; dotted lines indicate threshold for small effects ($|r| < .1$). Only correlations investigated in at least 5 samples are shown.

Moderation

After accounting for differences between diversity domains, a significant amount of heterogeneity remained, QE(2635) = 23600.60, $p < .001$. The credibility intervals shown in Table 3 show that substantial positive and negative correlations regularly appear for all domains. A decomposition of the I^2 statistic suggested that 1.7% of this large variance in effect sizes could be attributed to between-sample differences (Level 3), while 91.7% of the variance could be attributed to differences between the effects studied (Level 2). All three indicators suggested that tests for moderation were in order.

However, not all pre-specified moderators could be tested. Due to limitations in the data, we could not meaningfully include diversity climate or psychological safety, as these were very rarely reported, and if so, were always positive. Given the lack of variance, the missing data could not be imputed. Similarly, we could not meaningfully include virtuality and authority differentiation, as these were rarely reported, and associated with very specific types of teams. Specifically, sports teams were among the few that were explicitly working in the same space, while low authority differentiation was primarily found in student project teams. These associations, together with the fact that other reported data was reported because it was deviant, and missing data thus missing *not* at random, suggested that imputation would again produce misleading results. Therefore, these four moderators had to be dropped from our analyses. Finally, the criterion for performance could not meaningfully be imputed, as many measures are inherently ambiguous – therefore, we only considered this in univariate analyses, but dropped it from the multivariate meta-regression. As an *exploratory* replacement for authority differentiation, we considered whether countries' cultural power distance would moderate the relationship between diversity and performance.

Table 4.

Univariate (multivariate) tests of moderators

			Significance tests				Overall effect size	
Moderator	**N_S**	**k**	**Overall**	**Demographic**	**Cognitive**	**Job-related**	**R^2**	**R^2 < 5%**
Complexity	578	2,393			* (†)	*	0.00% [0.00%, 0.36%]	< .001
Interdependence	539	2,229					0.89% [0.66%, 2.28%]	< .001
Longevity	542	2,298				*	0.15% [0.00%, 0.75%]	< .001
Diversity measure	640	2,609					0.04% [0.00%, 0.76%]	< .001
Performance rater	644	2,623				* (*)	0.00% [0.00%, 0.09%]	< .001
Design	646	2,638					0.00% [0.00%, 1.06%]	< .001
Article focus	646	2,638	** (**)	*		†	0.77% [0.49%, 1.84%]	< .001
Perf. criterion	90	268					0.00% [0.00%, 4.36%]	.031
Year of data coll.	646	2,638			†	*	0.08% [0.00%, 0.63%]	< .001
Collectivism	549	2,328			** (*)		0.20% [0.04%, 0.99%]	< .001
Power distance	549	2,328	*		*		0.59% [0.23%, 2.16%]	< .001
Objective rating	634	2,623		*		**	0.00% [0.00%, 0.07%]	< .001
Country	646	2,596		*			3.91% [1.51%, 6.89%]	.392
TMT	646	2,638			†	*	0.15% [0.01%, 0.96%]	< .001
Student sample	569	2,638					0.09% [0.00%, 0.39%]	< .001
Industry sector	595	2,397					2.19% [1.26%, 5.90%]	.257
Team function	644	2,492	*				2.96% [2.56%, 5.13%]	.755

Note. N_S indicates the number of samples including data on that moderator, k the number of effect sizes. Values in square brackets indicate 95% confidence intervals. Significance tests are based on cluster-robust sandwich standard errors to account for the clustering of effect sizes within samples, while R^2 confidence intervals are BCa, estimated from 5,000 bootstraps from the multi-level meta-analysis model as estimated by *metafor*. They do not use cluster-robust sandwich standard errors and are thus more liberal than what was used for significance testing. R^2 < 5% show equivalence tests, testing whether R^2 is significantly smaller than the smallest effect size of interest. Upper block shows pre-registered moderators, while the lower block shows exploratory moderators.

† $p < .1$, * $p < .05$, ** $p < .01$, *** $p < .001$

As a starting point, and for comparability with earlier meta-analyses, we ran separate univariate meta-regression models for each moderator. These are reported in Table 4, separated into sections for the registered and further exploratory moderators. Equivalence tests were also conducted at the univariate level and shown in that table.[7] They indicated that none of the pre-registered moderators explained a *substantial* amount of variance (i.e. no R^2 was greater than 5%), even though several moderators explained a statistically *significant* amount of variance. Specifically, among the registered moderators, the complexity of the task, the longevity of the team, the performance rater (particularly when dichotomized into objective vs. subjective ratings), the article focus, the year of data collection, and the countries' level of collectivism was associated with effect sizes for at least some domains. Among exploratory moderators, countries' level of power distance, the country of data collection, whether a team was a top-management team, and the team's function showed significant correlations with effect size, with country and function potentially showing a substantial association (in that their R^2 was not significantly below 5%). Figure 4 show the relationships between the *significant* moderators[8] and the meta-analytic estimates (see SM 2C for the remaining results, and for full tables). To summarize, it can be seen that – in line with

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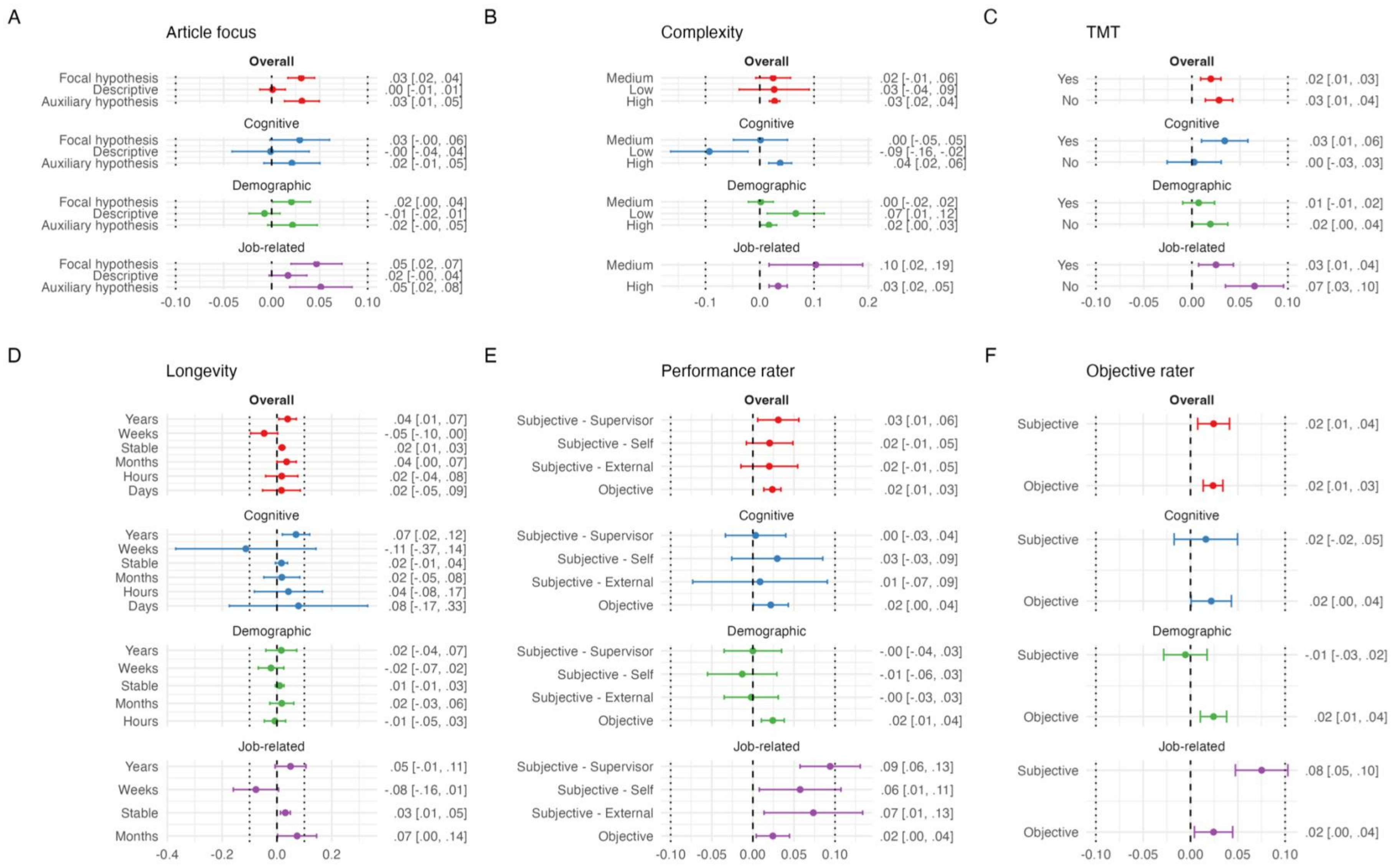
[7] To enable the estimation of BCa confidence intervals for R2 (which provide the most accurate coverage according to Viechtbauer, 2023), 5,000 bootstrap resamples had to be drawn for each moderator. Given that this involves the estimation of two multilevel meta-regression models each time, it is very computationally intensive (~50 CPU-hours per moderator), so that we could only do it either in the univariate or the multivariate case. Given that the incremental R2 is usually smaller than the raw R2, and that the incremental R2 depends on the presence of our specific set of moderators, we came to believe that reporting and testing the raw contribution of each moderator to explaining the variance in effect sizes would be more useful for readers interested in specific moderators.

[8] We omit country differences from the main manuscript, as these sub-samples are often dominated by a single study (or a closely related body of work) situated in a particular industry and should thus not be seen as indicating country-level differences. All details can be found in SM 2C.

the hypotheses – high rather than low task complexity was associated with a more positive relationship between diversity and performance for cognitive and job-related, but – contrary to expectations – not for demographic diversity. Articles that hypothesized a relationship between diversity and performance consistently found a stronger relationship than those were diversity only featured descriptively (e.g., as a covariate). Subjective performance ratings differed systematically from objective ratings, in that they yielded a more *positive* relationship between job-related diversity and performance, and a more *negative* relationship between demographic diversity and performance – but no differences between various subjective raters emerged. Overall, and in line with the R^2-values reported in Table 4, these differences were small, and may thus be of limited practical importance.


A
Article focus
Overall
Cognitive
Demographic
Job-related
Focal hypothesis
Descriptive
Auxiliary hypothesis
B
Complexity
Medium
Low
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Yes
No
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Longevity
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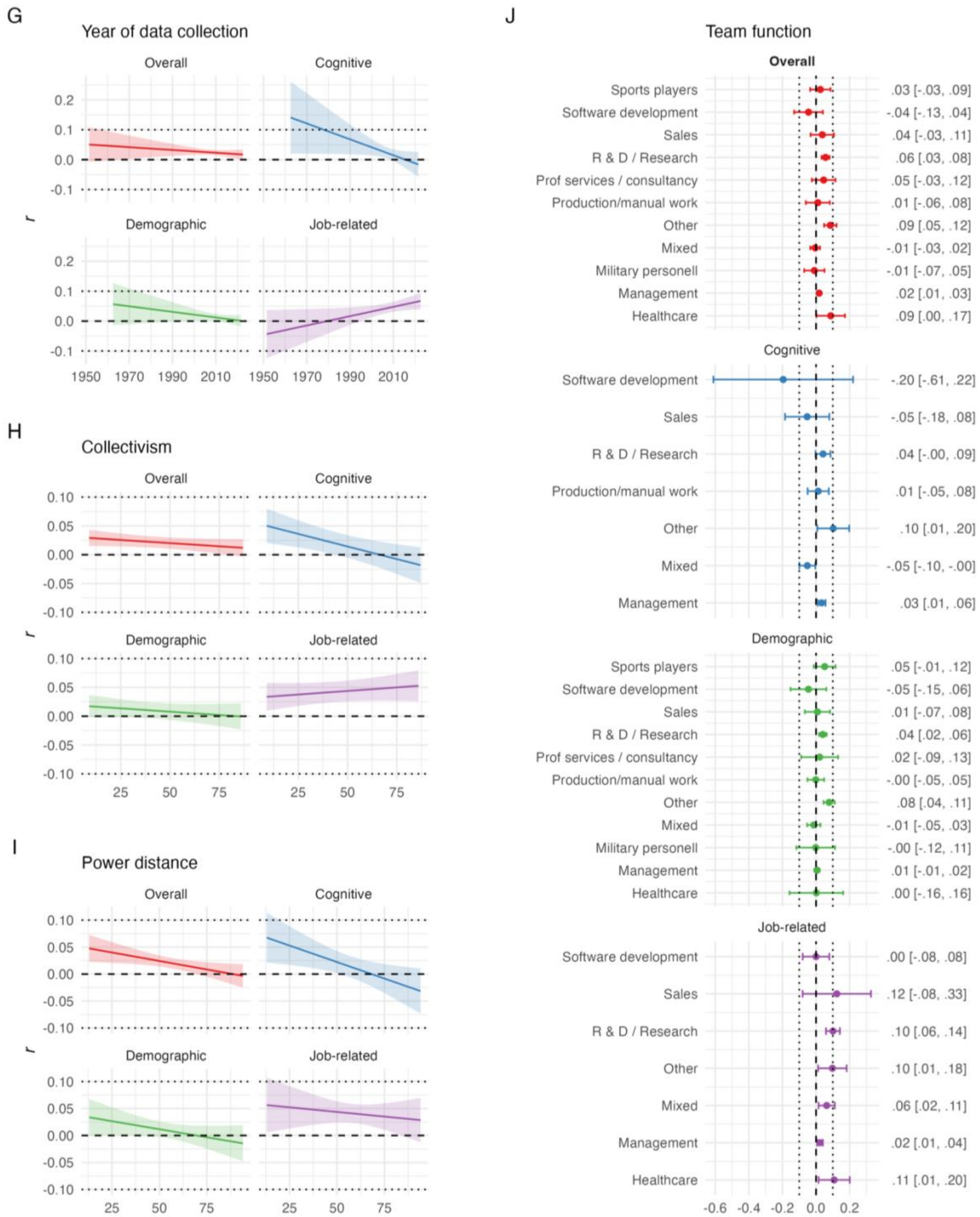

Figure 4. Estimated correlation between diversity and team performance depending on moderator values. Error bars show 95% confidence intervals; dotted lines indicate threshold for small effects ($|r| < .1$). Only correlations investigated in at least 5 samples are shown. The length of regression lines corresponds to the range of observed data.

To account for inter-relations between moderators, we estimated a multivariate meta-regression model including all pre-registered moderators. In this model, based on 100 imputations of missing data, only some moderators remained significant (reported in brackets in Table 4), in that they explained *unique* variance in the effect sizes. This was the case for article focus (overall), collectivism (for cognitive diversity), performance rater (for job-related diversity), and complexity (marginally, for cognitive diversity).

In a final step, we used *metaCART* to uncover potential interactions between moderators. We identified substantial variability in results between repeated runs. To select a suitable classification tree, we followed guidance by the developers of the method (E. Dusseldorp, personal communication) to estimate 50 trees and then select the mode of the number of resulting leaves. To further reduce distortions introduced by the effect size sampling, we repeated this across 10 datasets sampling different effect sizes from each sample. Across two modeling strategies (conventional and lookahead) and two imputation strategies (best and random), the modal result for the overall dataset were three leaves, where metaCART split twice on collectivism to single out a small subsample (7 studies) that were predominantly set in Turkey and showed larger effects. This is very unlikely to suggest that collectivism has different effects precisely at that level, and while there might be a moderator combination that sets these studies apart, we cannot identify one with any confidence. This finding was replicated to some extent in the dataset including only Job-related correlations, while the modal result for the other domains was that metaCART could not identify any moderation. Therefore, *metaCART* could not substantially add to our understanding of moderator interactions here. Full details can be found in SM 2C.

Publication bias

To explore potential publication bias, we began with funnel plots and Egger's test of funnel plot asymmetry to identify whether small studies systematically differed from larger studies. Figure 5 shows the results. For demographic and job-related diversity, there was no evidence that standard errors predicted effect sizes, so that it appears unlikely that substantial selection for *positive* effects and statistical significance took place there (note that this does not consider selection for significance operating in both directions). For cognitive diversity, Egger's regression test was significant with p = .016. However, the regression slope pointed in an uncommon direction (β = -0.60) indicating that studies with larger samples reported *larger* positive associations. This is unlikely to indicate publication bias against significant results but may rather suggest substantive differences between smaller and larger studies here.

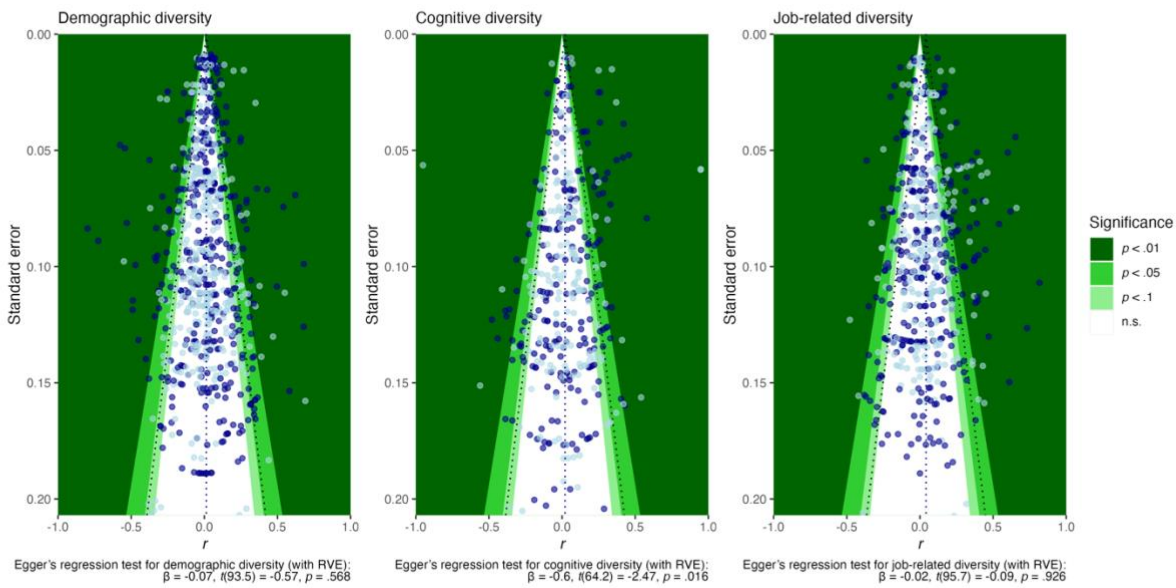

Figure 5. Funnel plots showing the observed effect sizes in relation to their standard error and statistical significance. The dotted lines in each plot show the meta-analytic effect size estimate with its 95% confidence interval at a given standard error. For legibility, the most extreme 1% of standard errors is not shown.

Selection models (extended from 3-PSM) were used to directly estimate whether significant (negative or positive) correlations had greater likelihoods of getting published than non-significant correlations. Across two different bootstrapping methods, there was no evidence of publication bias in favor of significant correlations in either direction. However, it appeared that for demographic diversity, *negative* significant relationships for demographic diversity were substantially less likely to get published than *positive* significant correlations, so that only 32% [13%, 73%] of the effect sizes expected in that category were reported.[9] For job-related and cognitive diversity, non-significant correlations appeared to be *more* likely to get published than non-significant correlations. Full results are available in SM 2D. Overall, this suggests that publication bias is unlikely to inflate the meta-analytic estimates or their heterogeneity, though the estimate for demographic diversity *might* be biased upwards. Here it needs to be noted that the results of interest in the original papers were rarely correlation coefficients per se, so that this result does not imply that there is no publication bias at the level of claims about diversity.

Exploratory analyses

Non-linear relationships

All statistical analyses presented here, and most research on the diversity-performance link to date, have focused on linear relationships. While there have been arguments to suggest that the relationship may be non-linear, this cannot be meta-analyzed based on reported summary statistics describing linear relationships, such as correlation coefficients (Gasparrini et al., 2012). Therefore, we can only offer a qualitative summary of the evidence for non-linear relationships.

Out of the 534 English-language reports included in this meta-analysis, 33 reported tests of non-linear relationships. Among the 26 that set out hypotheses, most found (partial) support for them (88 %). Where a specific functional form was hypothesized, this usually took the form of an inverted-U (∩) shape, where optimal performance is achieved at an

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[9] This estimate and confidence interval is based on the pre-registered effect-size-bootstrapping. Using the cluster-bootstrapping approach supported by preliminary simulation results (Pustejovsky & Joshi, 2023), we obtained an estimate of 38% [14%, 99.7%].

intermediate level of diversity, with weaker performance at both higher and lower levels of diversity (58%). Conversely, 27% of hypotheses stipulated a U-shape, where performance is higher in teams high or low in diversity. However, only one of the articles testing non-linear relationships correctly tested for the presence of a turning point (i.e. Hoisl et al., 2016), while most others relied solely on the significance of the quadratic term, which is also compatible with relationships that plateau or accelerate, without ever turning (Leif & Simonsohn, 2014). A comprehensive summary of the evidence can be found in SM 2G, while the implications will be discussed below.

Differences between team types

To further explore when diversity makes a difference, we investigated whether the relationship between diversity and performance is different for various team types. As can be seen in Panel C in Figure 4, the performance of top management teams was more positively correlated with cognitive diversity, and less positively correlated with job-related diversity compared to other teams, resulting in no significant difference overall. Relatedly, where performance was measured as firm-level financial performance (a subset of top team outcomes), the overall diversity-performance link was weaker than that for other outcomes (r = .011 [-.001, .023] vs r = .033 [.020, .045], p_{Wald} = .012), driven by differences in the association for job-related diversity.

For teams engaged in research and development activities, diversity was more closely associated with performance than for other teams (r = *.058* [.033, .083] vs r = .020 [.010, .029], p_{Wald} = .007), particularly when it came to job-related (but not cognitive) diversity. Relatedly, for outcomes explicitly related to creativity and innovation, the diversity-performance link was stronger than for other outcomes (r = .056 [.25, .88] vs r = .020 [.011, .029], p_{Wald} = .024), particularly for cognitive and job-related (but not demographic) diversity.

More broadly, the industry sector teams operated in was not a significant moderator of the diversity-performance links, though this estimate is rather uncertain and *not* equivalent to the smallest effect size of interest (Table 4). Therefore, further research is needed here. Team's function was a significant moderator, primarily driven by the difference between R&D teams and others already discussed above. Other differences are visible in Figure 4.

Use of and differences between diversity measures

Against our expectations, the broad operationalization of diversity as variety or separation did not affect the diversity-performance relationship. Therefore, we explored the use of measures further. Across the sample, the most used measure was the Blau index (also called Herfindahl-Hirschman), accounting for 35.9% of effect sizes. When measured as variation, diversity was also commonly measured with the Teachman entropy index (4.0%). For diversity as separation, the coefficient of variation was used most frequently (16.1%), followed by the standard deviation (13.5%). Additionally, 5.9% of effect sizes were based on binary splits, often indicating whether there was any diversity on a particular dimension. Any other measurement was used in less than 4% of cases (full details in SM 2E).

Unfortunately, the use of measures was strongly associated with the diversity sub-domain – educational level, tenure and age were the only sub-domains where variety and separation each accounted for at least 10% of the effect sizes. Therefore, we explored whether the conceptualization of diversity and/or the use of measures was associated with effect sizes within these sub-domains (noting that these tests will have much lower power than moderation tests on the full sample). There was only a *marginally* significant trend for tenure diversity, where the estimate for the association between variety and performance was larger than that for separation and performance, and no longer significantly smaller than the smallest effect size of interest (r = .092 [.024, .161] vs. r = 027 [.003, .051], p_{Wald} = .070).

Relationships between reported correlations and citation counts

With a wide range of model specifications, we found no evidence for a relationship between the size, direction, or significance of the correlation between diversity and performance, and the number of citations a report received when controlling for its publication type (e.g., journal article, dissertation), language and the publication year (see SM 2E). This may again be related to the limited relationship between the correlations and articles' focal results yet does not provide evidence for a suspicion that the reception of the evidence is systematically skewed.

Robustness checks

Exclusion of studies showing evidence of misreporting

Our dataset did not include any retracted papers (per the Retraction Watch Database as of 10/02/2024). However, the GRIM test flagged two papers that reported means that were inconsistent with the reported sample sizes and scale ranges, and the *statcheck* package flagged 21 papers with reporting mistakes (some of which were very minor, such as reporting $p < .004$ instead of $p = .004$). Therefore, we reran the main meta-analysis without the 63 effect sizes from these 23 papers. This only resulted in minor changes, with the estimated correlations changing by .003 or less. While this pushed the significance of the correlation between demographic diversity and performance over the threshold (from $p = .035$ to $p = .055$), we take it to suggest that our results and their interpretation are not substantially affected by possible misreporting, in as far as this can be detected with such simple methods.

Restriction to objective performance measures

While we already considered objective vs. subjective performance measures as a moderator, we registered to conduct analyses focused exclusively on objective measures as a robustness check. In the remaining dataset of 1,547 effect sizes, the main results were in line with what was to be expected from the moderation analyses: the expected correlation between demographic diversity and performance was larger than in the full dataset (.25 [.010, .037] *vs* .14 [.001, .026]), while the correlation between job-related diversity and performance was smaller for objective-measures only (.025 [0.005, 0.046] *vs* .042 [0.025, 0.058]). However, the point estimate for cognitive diversity was identical and none of the differences were statistically significant. Regarding moderators, the pattern of results was similar, though the difference between correlations reported for focal rather than descriptive tests occurred only for demographic diversity. Full details can be found in SM 2E.

Table 5. *Summary of results*

Hypothesis / Research Question	**Outcome**	**Summary**
RQ1a: Is the link between diversity and team performance insubstantial (i.e., $\lvert r \rvert < .1$)? Does this differ between the dimensions of diversity?	✓	Significantly positive, but insubstantial association between diversity and performance for all dimensions. Insubstantial differences between the three dimensions.
H1: Diversity has a substantial positive association with performance when the task is high in complexity.	✗	While greater task complexity predicts a stronger positive relationship between job-related and cognitive diversity and performance, the estimates remain below the $\lvert r \rvert < .1$. For demographic diversity, the correlation is not significantly associated with task complexity
H2: Diversity has a more positive association with team performance when the task requires a high level of interdependence.	✗	Interdependence did not significantly predict effect size.
H3a: Diversity has a more negative link to performance in tasks that focus on maximizing production of an output with a pre-defined strategy.	(✗)	Could only be tested in small sample, as criterion was often unclear, and rarely about production. In that sample, no significant difference between production and creative tasks.
H3b: Diversity has a more positive link to performance in tasks where performance depends on creative divergence rather than convergence.	(✗)	Could only be tested in rather small sample, as criterion was often unclear. Nevertheless, in that sample, there was no significant difference between divergence and convergence.
RQ2a: Is the relationship between team diversity and performance related to a country's level of collectivism versus individualism?	Mixed	Cognitive diversity has a more negative relationship with diversity in countries higher in collectivism, while there is no significant relationship with the other domains.
H4a: The relationship between diversity (particularly demographic diversity) and team performance has become more positive over time.	✗	The regression slop for year on effect size is not significant but negative for both the overall sample and for demographic diversity.
H4b: The relationship between diversity and team performance is positive and substantial (i.e. $r > .1$) in evidence from the past decade (2012-2022).	✗	Lack of support for H4a precludes this.
H7: Diversity has a more positive link to performance when the team is low in authority differentiation than when it is high in authority differentiation.	(✓)	*Reported too rarely in primary studies.* Used national power distance as an exploratory 'replacement' as greater cultural power distance is likely to correlate with greater authority differentiation. In line with the hypothesis, greater power distance was associated with a more negative diversity-performance relationship, overall and for demographic diversity
RQ3b: How does the link between diversity and performance differ depending on the longevity of a team?	✗	No systematic relationship between team's longevity and observed correlations. Significantly weaker correlations for short-lived teams for job-related diversity in univariate test likely due to differences in tasks.
RQ4a: How does the link between diversity and performance differ depending on whether performance is rated subjectively or measured objectively?	✓	In line with earlier findings, objective ratings associated with more *negative* correlations for job-related diversity and (less consistently) more *positive* correlations for demographic diversity, compared to subjective ratings.
H8: Diversity will have more positive associations with performance where it is measured as variety rather than separation.	✗	No significant relationship, but weak test as measurement choice was very closely linked to diversity sub-domain. In exploratory analysis, marginally significant difference found for tenure diversity in line with expectations.
H9: Studies where the link between diversity and performance is the focal hypothesis will report larger (H8a) and more positive (H8b) effect sizes than studies where this is an auxiliary or descriptive result.	(✓)	Expected difference found between studies testing diversity effects as a focal hypothesis and those reporting descriptive results. Against expectations, auxiliary hypotheses were closer to focal than descriptive results.
Omitted hypotheses related to diversity climate (H5), psychological safety (H6), and virtuality (RQ3a).	?	*Reported too rarely in primary studies*
Selected exploratory:		✗ Citation counts not related to effect size or significance under a wide range of model specifications ✓ Diversity-performance link stronger for creative and R&D tasks than others ? Diversity-performance link weaker when top-team diversity is correlated with firm financial performance

Discussion

Overall, our results show that diversity (across demographic, job-related and cognitive dimensions) is positively correlated with team performance, but with an insubstantial effect size: on average, diversity (on one trait) explains far less than 1% of the variance in team performance. However, the observed effects vary widely, with substantial negative and positive effects well within the 80% credibility interval. Therefore, the identification of factors that explain this variation is critical, so that we carried out moderation analyses. However, this was hampered by limited description of teams' contexts and tasks in the literature, so that some proposed moderators could not be tested.

Nevertheless, what we found broadly supports the contention that diverse cognitive resources may have value, while contrasting social identities may be less beneficial. Specifically, at the level of sub-domains, we found that diversity in degrees, functions and nationalities was significantly related to performance, likely because they all reflect possession of distinct bodies of knowledge. Conversely, diversity in age, race/ethnicity, educational levels, and personal values was not significantly related to team performance, likely because these categories are less related to cognitive resources and more related to social identities. However, the positive correlations for gender and tenure diversity do not quite fit this pattern. Here we would speculate that the positive relationship for gender diversity may primarily reflect a more efficient use of talent (by recruiting from under-represented groups), while tenure diversity may make it more likely for distinct perspectives to remain salient, and thus valuable – yet this requires further research and/or theorizing.

When it comes to the types of tasks teams engaged in, teams pursuing tasks high in complexity generally showed a more positive correlation between job-related and cognitive diversity and performance, possibly because these tasks could benefit from diverse perspectives and skill sets (Sulik et al., 2021). Similarly, diversity had a more positive relationship with team performance for teams engaged in research and development tasks and for teams that pursued outcomes related to creativity and innovation. However, none of these settings affected the relationship between demographic diversity and performance, and the average correlations remained insubstantial (< .1) throughout.

When it came to team processes and context, many reports lacked details, so that we could not test whether diversity climate, psychological safety or virtuality make a difference. We also had insufficient information on authority differentiation within teams, but it appears likely that this is associated with national power distance – in that teams in countries low in power distance are more likely to spread authority within the team. Across the dimensions, teams in countries low in national power distance showed a stronger relationship between diversity and performance, which we take to suggest that it is important to diffuse authority within a team, so that space is created for distinct perspectives to emerge. Across countries, greater collectivism predicted a more negative relationship between cognitive diversity and team performance, likely because a greater focus on team cohesion makes it more difficult for different perspectives to emerge.

While we did not find substantial evidence of publication bias, the substantial difference in reported correlations for descriptive rather than hypothesized relationships is important to note. This suggests that claims made – rather than correlations reported – may be selected for significance, so that a reading of the literature may create a somewhat misleading impression. Also, the indication that significant negative correlations between demographic diversity and team performance may be less likely to be published than others needs to be noted as it might affect the perception of the literature, though its potential impact on the meta-analytic results is limited due to the preponderance of correlations that lack significance.

Theoretical implications

While we found small average correlations, and thus small differences between levels of moderators, the pattern overall corresponds to the idea that an understanding of diversity effects must consider both the downsides of conflicting social identities and the upsides of richer cognitive resources. Situations that called for substantial cognitive resources (e.g., situations high in complexity, or R&D teams), and tasks that focused on creativity, particularly benefited from diversity – but, given the small overall effect size, it appears that the contradictory dynamics balance out on average.

Similarly, we confirm the finding by van Dijk et al. (2012) that objective performance measures show a different pattern of association with (demographic and job-related) diversity than subjective measures, so that diversity may have distinct effects on team performance and the perception of team performance. Unfortunately, we did not have data on diversity climates (i.e. beliefs about diversity) – yet the fact that we got near-identical estimates for the diversity-performance relationship for objective measures across domains, but distinct estimates for subjective measures may indicate that beliefs concerning the value of diversity may affect perceptions more strongly than results.

Need to focus on non-linear relationships

Theoretically, it appears highly implausible that diversity would have a linear relationship with performance, in that each incremental "unit" would have the same effect on performance. Yet, this is what most reports assume, generally without any justification. Compelling argument can be made for various functional forms. For instance, one may posit that increasing diversity from a low baseline primarily increases the breadth of cognitive resources, while increases from a higher baseline led to a situation where social identity concerns become dominant and undermine team dynamics (e.g., Luan et al. 2015). If so, moderately diverse teams could be expected to outperform both minimally and maximally diverse teams (Sulik et al., 2021). Conversely, however, one might argue that conflict is highest at an intermediate level of diversity, where teams can break down into a small number of subgroups – so that teams are better off either homogenous or maximally diverse, resulting in a U-shaped relationship (Dayan et al., 2017). This can be linked to *faultline* research which suggests that teams underperform if they can break down into a small number of subgroups that share multiple identities (Kirkman et al. 2013), which appears most likely at intermediate levels of diversity – yet this body of research is only weakly linked to the literature discussing diversity effects. Relatedly, some research, particularly concerning board gender diversity, has suggested that the benefits of diversity are only realized when there is a 'critical mass' of minority-group members. While this could be seen as indicating a step-change in the relationship, from zero to a positive slope, it is typically operationalized as a U-shaped relationship (e.g., Joecks et al., 2013) as well. Alternatively, at the most basic level, one might expect that the benefits of more diverse cognitive resources – like those of almost all good things – diminish at the margin, so that a linear relationship would flatten out.

As outlined above, the reports summarized here offer limited insights as to which of these accounts is most (widely) appropriate, or as to whether a linear approximation is good enough, in that very few reports consider non-linearity theoretically or empirically, and that there was evidence for selective reporting among those that did. Within the limited evidence, a ∩-shaped was tested and supported most frequently – though often the tests used were unable to distinguish this from a diminishing curve that flattens out rather than turns negative. Here further theoretical and empirical work is urgently needed.

Practical implications

The "business case for diversity" is widely articulated, and many efforts towards greater diversity are justified based on its claimed potential to increase organizational performance. The results here show that this may be too simplistic – diversity does not substantially improve (or hamper) team performance across the board. While it may be worth noting that the evidence suggests that diversity may be more likely to provide (minimal) benefits rather than harms on average, the picture is more complex.

For teams that perform tasks which directly benefit from a wide range of perspectives, such as those tasked with research, creativity, and innovation, it might make sense to aim for greater diversity *in order to boost performance* – even though the average associations remain small. Thus, it appears that diverse teams need the right context to flourish. Unfortunately, our data on team context was limited, yet it indicates that teams may benefit from shared authority and an appreciation of individuality, so that different perspectives can emerge effectively.

In other teams, expected increases in team performance do not provide a strong justification for increasing diversity. Evidently, there are many other important components of the (business) case for diversity, equity and inclusion that persist – including moral, legal and reputational reasons, as well as the need to find strong individual talent even if it does not come in the 'prototypical' guise. Raising expectations regarding universal performance increases, however, appears not to be intellectually honest and may potentially backfire when expected changes do not materialize, and the very foundation provided for diversity initiatives is weakened (Ely & Thomas, 2020).

Implications for research

Our review of a wide range of reports linking diversity and team performance leads us to make three recommendations to researchers in this field, most urgently when it comes to the measurement of diversity.

Improve diversity measurement

Across the literature reviewed here, the most frequent citation in the methods sections appeared to be to Harrison and Klein (2007) who highlighted that diversity may be conceptualized as variety, disparity or separation and that the measurement choice must reflect the chosen conceptualization. Nevertheless, that message was rarely heeded. Instead, their article was often simply cited as indicating that continuous and categorical measures need to be aggregated differently. Most strikingly, this led to the use of separation measures (particularly the coefficient of variation) when hypotheses appeared to be about variety. For instance, hypotheses about the *value* of tenure or age diversity seem to imply that a roughly even distribution of values over the possible range would be best (so that many different age groups are included), yet the most common measures used (i.e. the coefficient of variation and the standard deviation) would indicate that teams consisting of two homogenous sub-groups at the extreme ends are higher in diversity. Conversely, race/ethnicity was almost exclusively measured as variety, e.g., with the Blau index, even where hypotheses suggested concerns with subgroup formation that would be better reflected in measures of separation. In that regard, we are left to repeat the call by Bell et al. (2011) a decade on and urge researchers to choose measures appropriate to their hypotheses, and to justify these choices.

Furthermore, most measures of variety treated all categories equally – even though it appears clear that along most dimensions, some categories will be further apart (and thus have more distinct cognitive resources and social identities) than others, whether that is in teams composed of German, French, and Chinese workers, or teams composed of marketing, sales, and engineering specialists. Some studies developed more targeted measures of distance, such as Ingersoll et al. (2017) who operationalized nationality diversity by taking linguistic distance into account, yet this was usually done ad-hoc without strong validation. Relatedly, studies used very different numbers of categories in measures of functional (or educational) diversity, which is problematic in that a larger number of categories appears to make it more likely that the differences between categories become highly uneven. Here, more explicit justifications and (simulation) research into the impact of such choices is needed.

Finally, most dedicated diversity indices assume a non-linear relationship between the share of minority-group members and the diversity of the resulting group,[10] which is certainly defensible. However, none of the papers included here discussed that feature when choosing to measure diversity with such an index or as when choosing to simply use the percentage of minority group members. Given that (conceptually) results may radically diverge based on the choice of indices, and that readers' understanding of diversity (particularly in the two-category case) may often reflect something closer to percentages than to the indices, more explicit choices, reporting and robustness checks may be advisable.

Describe context clearly

The relationships between diversity and team performance vary widely, so that the identification of boundary conditions is a priority for research. Many studies are concerned with specific moderators – yet many moderators can only be meaningfully uncovered when aggregating findings across studies and settings. However, that requires a clear description of the context in which teams operated. In too many cases, it is unclear what the teams did, how they were managed, or even what sector they operated in. Relatedly, performance measures were often too generic to map onto specific tasks or theoretical expectations, particularly when they were based on subjective assessments. For instance, global ratings of team creativity are limited, in that they omit theoretically important distinctions such as that between convergence and divergence, and instead rely on subjective semantic understandings of broad terms.

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[10] This can be illustrated with the Blau index, which is most used. Here, in a group made of men and women, an increase in the share of women from 0% to 10% would have 9 times the effect on diversity than an increase from 40% to 50% (moving from 0 to .18 in the first case, and from .48 to .50 in the second).

Consider non-linear associations (correctly)

As discussed above, more research needs to consider non-linearity in the association of diversity with team performance. However, this needs to be done correctly. In addition to the small number of studies that reported tests of non-linear relationships, methodological shortcomings limit the interpretation of the evidence. Most importantly, almost all studies only test whether a quadratic term of their diversity measure is a significant predictor of performance, and then use the coefficient sign to deduce whether there is a U-shaped or inverted U-shaped relationship. However, this is insufficient as it may lead to inaccurate claims regarding a reversal of the relationship when there is in fact only a diminishing (e.g., logarithmic) association. Instead, studies investigating non-linearity should present plots *of the observed data* that allow readers to understand its range and shape, and specifically test whether the slopes on both sides of a proposed turning point are significant, thereby confirming that increasing diversity indeed initially predicts *increases* and later *decreases* in performance (Simonsohn, 2018; see Hoisl et al., 2016 for an example of a similar analysis here).

Strengths and limitations

The current work has some substantial strengths compared to earlier meta-analyses. It used a comprehensive reproducible search strategy that included a substantial range of grey literature (particularly dissertations). It is also the first English-language meta-analysis that substantially goes beyond the English-language literature, primarily by integrating the voluminous Chinese literature as well as some sources in a range of other languages. However, the search beyond the English literature relied on Google Scholar and author contributions – future research should consider using dedicated bibliographic databases in other scholarly languages to ensure broader coverage. Also, search terms could be more comprehensive in future work – particularly given rapidly improving tools to semi-automate the screening process. [11]

Methodologically, multi-level meta-analytic models with robust-variance estimation made it possible to use all information included in reports (rather than just a single effect size per sample), which particularly enabled the inclusion of measures presented as covariates in a paper, and thereby reduced the potential influence of reporting biases. The use of equivalence-testing helped to avoid an excessive focus on statistical significance, which can be achieved for practically insignificant findings given the ever-increasing samples available for comprehensive meta-analyses. However, at present the choice of the smallest effect size of interest may be seen as arbitrary, so that there is a need for researchers to start discussing what constitutes a substantially meaningful effect on (e.g.) team performance. The use of multiple imputation for missing data, and then meta-regression to move beyond the univariate testing of moderators, helped to calibrate the confidence in moderation analyses. However, it also became clear that univariate tests can enable clearer communication of results, as average effects within a category are more interpretable than marginal means (and subject to fewer subjective choices regarding the reference levels of other moderators).

Most fundamentally, the interpretation of our results – correlations between diversity and team performance – is limited by the correlational nature of the data. Most effect sizes are cross-sectional, while for some, performance is lagged by one period (e.g., measured in the subsequent year). This means that associations between diversity and performance may be confounded, and that there may even be reverse causation in some circumstances. Some

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[11] While contacting authors to request further papers, we received some good suggestions for keywords that may benefit future researchers who may wish to include opposites of diversity (homogeneity), additional performance terms ("outcomes", "effectiveness", "goal achievement", “decision-making” and "strategic choice"). Also, searching for “faultlines” may be helpful as much of that research controls for the ‘traditional’ diversity indicators

studies attempted to estimate causal relationships from observational data, for instance by using instrumental variable approaches (e.g., Ingersoll et al., 2017) yet they were rare, and their approaches varied too widely to aggregate these results separately. Reliable longitudinal evidence (e.g., from random-intercept cross-lagged panel models) was absent from our sample, and experimental evidence was rare and generally confined to fairly artificial lab settings (though the results there did not differ significantly from the observational effect sizes). Until more research with such designs is conducted, any interpretation of meta-analytic results has to keep their correlational nature in mind, which cannot provide direct support for causal claims. Nevertheless, we would argue that particularly the *absence* of substantial correlations is still informative, in that strong causal effects would seem to imply their presence.

Directions for future research

Regarding primary research, we already discussed the need to increase clarity on non-linear relationships and diversity measurements/conceptualizations. In addition, further longitudinal research would be valuable if it uses cross-lagged (random-intercept) models or growth models that allow to estimate within-team changes following changes in diversity. This would need to go along with the development of theoretical accounts of temporal dynamics, particularly regarding non-linear effects of time (see Srikanth et al., 2016). Finally, further research is needed into the moderators that we identified as theoretically meaningful, yet could not test given the extant evidence, specifically virtuality, beliefs about diversity, psychological safety, authority differentiation and different types of creative performance (e.g., convergence vs. divergence).

Regarding evidence synthesis, one promising avenue would be to meta-analyze non-linear relationships, given their theoretical and practical importance and the dearth of evidence. However, that would need to take the form of a mega-analysis (or individualparticipant data meta-analysis) where datasets underlying the various reports are retrieved, so that consistent non-linear (e.g., quadratic) models can be estimated and then aggregated.

Additionally, meta-SEM models could be employed to understand the mechanisms linking diversity to team-level outcomes (in line with Triana et al., 2021). However, in line with their results, it appears to us that the literature mostly focuses on mediators explaining potential negative pathways (i.e. team processes that could be hampered by identity conflicts) – with some exceptions particularly around information elaboration. So, there might be a need for more primary research into team processes before meta-SEM can deliver a comprehensive picture.

Finally, the mechanisms and conditions by which job-related diversity affects performance appear under-theorized, at least where it is seen as distinct from cognitive diversity. We included it here primarily due to its practical significance and focused on cognitive and demographic diversity, yet there is scope for further work that focuses on this dimension.

Conclusion

Diversity is at times taken to promise creative breakthroughs or threaten communicative breakdowns. Our results here show that the picture is more complex – when reduced to a single estimate, the average (linear) correlation between team diversities and team performances is too small to matter substantively. Instead, context matters. While it appears that diversity may benefit creative tasks, and that the diversity-performance link may be enhanced by a (team) culture that distributes power and values individuality, further research on this is needed – further research that measures diversity in line with a clear theoretical conceptualization, and that allows for non-linear relationships between diversity and performance. Additionally, interactions between multiple diversities need to be considered further. In the meantime, arguments other than those about performance may be more compelling when it comes to promoting action toward diversity.

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Note. References marked with an asterisk (*) are included in the meta-analysis.

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