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Purvanova, Radostina K.; Kenda, Renata

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The impact of virtuality on team effectiveness in organizational and non-organizational teams: A meta-analysis

Radostina K. Purvanova[1] | **Renata Kenda**[2]

[1]Department of Management and Organizational Leadership, College of Business and Public Administration, Drake University, Des Moines, IA, USA

[2]Department of Organization Studies, School of Social and Behavioral Sciences, Tilburg University, Tilburg, The Netherlands

Correspondence
Radostina K. Purvanova, Department of Management and Organizational Leadership, College of Business and Public Administration, Drake University, 2507 University Ave, Des Moines, IA 50311, USA.
Email: ina.purvanova@drake.edu

Abstract

We meta-analytically assess the virtuality-team effectiveness relationship using 73 samples of organizational teams (5738 teams) reporting on a wide range of productive (e.g. earnings), performance (e.g. customer ratings), social (e.g. cohesion), and team member (e.g. project satisfaction) outcomes. Our results suggest that in work organizations, virtuality is not a direct input—negative or positive—to team effectiveness. In contrast, using 109 samples of non-organizational teams (5620 teams), we show that virtuality is a significant negative input to team effectiveness. We also meta-analytically assess the issue of results generalizability from non-organizational to organizational settings, and find that overall, results from non-organizational studies largely fail to generalize to organizational virtual teams. Using moderator analysis, we explore a number of study features that may explain the poor results generalizability from non-organizational to organizational studies. We find that results from non-organizational studies using undergraduate students, short team duration, and laboratory settings drive the non-generalizability effect, whereas results from non-organizational studies using graduate students, longer team duration, and classroom settings produce results comparable to those of organizational

studies of virtual teams. Theoretical, methodological, and practical implications are discussed.

KEYWORDS
meta-analysis, team effectiveness, virtual teams, virtuality

1 | INTRODUCTION

Virtual teams are groups of employees who collaborate from dispersed locations via electronic means to accomplish common team objectives (Gilson et al., 2015). An ever-growing number of work organizations employ virtual teams (Einola & Alvesson, 2019; IWG, 2019), prompting some to describe them as "the new normal way of teaming" (Dennis et al., 2014, p. 18; see also Raghuram et al., 2019). Organizations cite gains in productivity, greater talent utilization, and savings in business travel, among others, as key reasons to adopt virtual teams (Deeb, 2020; Jimenez et al., 2017). Yet, the increased reliance on virtual teams is tempered by another rising trend—bringing teams back in (Roberts, 2014). Just prior to the 2020 health pandemic, companies such as Best Buy, Bank of America, IBM, Zappos, and Yahoo!, as well as government agencies such as the U.S. Department of Agriculture and U.S. Department of Education, were some of the work organizations to discourage virtual teams, issuing public statements that collaboration requires co-presence and face-to-face communication (Aratani, 2018; Lee, 2016; Simons, 2017; Wagner, 2018). Similarly, employers such as Google, Netflix, and DropBox have publicly stated that once the 2020 health pandemic subsides, workers may continue to work virtually some of the time, *but* would need to come in for in-person team interactions rather than collaborate with their teams virtually (Alexander et al., 2020; Vasel, 2020; Wakabayashi, 2020). It appears, then, that assumptions about virtual teams' effectiveness are quite varied in the world of work as organizations have both embraced, and rejected, virtual teams in recent years (Wilkie, 2019).

Whereas opinions about virtual teams diverge among organizational actors, extant meta-analytic reports on the relationship between virtuality and team effectiveness have been consistently negative (Carter et al., 2019). In keeping with the predominantly negative theoretical view on virtuality in the virtual teams literature (Gibbs & Boyraz, 2015; Nurmi & Hinds, 2016a; Raghuram et al., 2019), a number of meta-analyses have shown that important team outcomes, such as decision accuracy (Baltes et al., 2002; Dennis & Wixom, 2002), efficiency (Baltes et al., 2002; Dennis et al., 2001), knowledge sharing (Ortiz de Guinea et al., 2012), intrateam conflict or disagreement (Fjermestad, 2004), and team member task and project satisfaction (Baltes et al., 2002), are significantly worse in virtual teams relative to their traditional, face-to-face counterparts. However, these findings may fail to inform organizational leaders' understanding of the impact of virtuality on team effectiveness as studies of *organizational* virtual teams are severely underrepresented in extant meta-analyses. That is, 76% to 100% of primary studies included in prior virtuality-team effectiveness meta-analyses[1] involve short-lived student teams, not organizational virtual teams. Hence, though virtual teams are considered "the new normal way of teaming" in work organizations (Dennis et al., 2014, p. 18), and despite over 30 years of virtual teams research (Raghuram et al., 2019), the impact of virtuality on *organizational* teams remains poorly understood.

In response, our study's central objective is to throw light on the virtuality-team effectiveness relationship in *organizational* teams by providing a comprehensive meta-analytic test of the effects of virtuality on organizational teams. Though the number of studies reporting on organizational virtual teams has increased appreciably in recent years (Gibbs et al., 2017), only some of these studies have been included in prior meta-analyses (see footnote 1). By cumulating results from this largely untapped data source, we aim to more definitively establish how virtuality impacts team effectiveness in *organizational* teams. To this end, we draw on novel theoretical developments in the virtuality literature offered by the virtuality-as-paradox perspective (Cousins et al., 2007; Dubé & Robey, 2008; Gibbs, 2009; Purvanova & Kenda, 2018; Siebdrat et al., 2009). This newly developed theoretical perspective, which is heavily grounded in theories of practice (e.g. Sandberg & Tsoukas, 2011), has emerged from qualitative observations of *organizational* virtual teams. It deviates from the predominant negative view on virtuality in an important way: Rather than assuming superiority of in-person communication and spatio-temporal proximity, this perspective presents a balanced view on virtuality, inclusive of *both* virtuality's challenges, *and* its opportunities (Cousins et al., 2007; Dubé & Robey, 2008; Gibbs, 2009; Purvanova & Kenda, 2018; Siebdrat et al., 2009). The virtuality-as-paradox perspective serves as our theoretical background regarding virtuality's expected impact on the effectiveness of *organizational* teams.

An important second objective of our research is to provide an in-depth exploration of the issue of results generalizability from *non-organizational* to *organizational* virtual teams. Non-organizational teams—which, as mentioned, are over-represented in extant meta-analyses—are typically composed of students interacting under laboratory or other non-organizational settings for short periods of time. Scholars have long argued that results from non-organizational virtual teams likely do not generalize to naturally occurring organizational virtual teams (e.g. Gibbs et al., 2017; Hertel et al., 2005; Kirkman et al., 2012; Martins et al., 2004; McGrath & Tschan, 2007; Purvanova, 2014). Because this argument questions the very validity of accumulated empirical knowledge about virtuality's impact on team effectiveness, it is tremendously important, and yet, it has remained underexplored. We carefully examine the criticisms that have been levied against non-organizational virtual teams research while also paying attention to counter-arguments. For example, non-organizational studies have been criticized for relying on student participants and for studying virtuality's endpoints only. But, Mesmer-Magnus et al. (2011) argued that student teams provide valuable information about the effectiveness of virtual teams at-large, and Walther (2011) defended the practice of comparing "virtual" to "face-to-face" teams on the basis that the absence of a "gold standard" may lead to fallacious or misleading conclusions about virtual teams. We subject this important debate to empirical scrutiny by leaning on the study design and validity literature which discusses core drivers of results generalizability (Bracht & Glass, 1968; Campbell, 1957; Gibson, 1979; Hammond, 1998a, 1998b). Specifically, we code several theory-driven moderators to address issues raised by the study design and validity literature related to the concepts of population validity, ecological validity, and representative design.

To accomplish our objectives of (1) understanding the impact of virtuality on team effectiveness in *organizational* teams and (2) examining how moderators explain results generalizability (or lack thereof) from *non-organizational* to *organizational* virtual teams, we meta-analyze effect sizes from 73 independent samples of organizational virtual teams, and 109 independent samples of non-organizational virtual teams. Further, to systematically organize the various team effectiveness outcomes reported in the literature, we employ Hackman and

Wageman's (2005) framework of team effectiveness. We report meta-analytic estimates for productive team outcomes (e.g. earnings, accuracy, and process improvements), performance team outcomes (e.g. externally assessed team performance and team member assessed team performance), social team outcomes (e.g. cohesion and team trust), and individual team member outcomes (e.g. project/task satisfaction and relational quality).

2 | THEORETICAL BACKGROUND AND HYPOTHESES

To develop theoretical understanding of how virtuality influences team effectiveness in *organizational* teams, we integrate the virtuality-as-paradox perspective (Cousins et al., 2007; Dubé & Robey, 2008; Gibbs, 2009; Purvanova & Kenda, 2018; Siebdrat et al., 2009) with the multidimensional model of team effectiveness (Hackman & Wageman, 2005). The virtuality-as-paradox perspective represents an attractive theoretical foundation for two reasons: It describes both the dark and the bright faces of virtuality rather than focusing on one or the other, and it is grounded in practice which improves its ability to speak to virtuality's influence on *organizational* teams. In turn, Hackman and Wageman's (2005) model of team effectiveness provides a useful organizing framework for the myriad of team effectiveness outcomes researched in the literature. The model describes three facets of team effectiveness, productive outcomes, social outcomes, and team member outcomes, and has been employed by recent meta-analyses to effectively present quantitative summaries of findings on otherwise disparate team effectiveness outcomes (e.g. Breuer et al., 2016).

We begin with brief overviews of the virtuality-as-paradox perspective and the multidimensional model of team effectiveness. We next overlay the key idea advanced by the virtuality-as-paradox perspective—namely, that virtuality engenders *both* challenges *and* opportunities—onto the three team effectiveness outcomes (productive, social, and team member) to describe how virtuality likely influences these outcomes in organizational teams. We then contrast this framework with the negative view on virtuality that dominates non-organizational virtual teams research, and offer hypotheses about the role of moderators for results generalizability from non-organizational to organizational virtual teams.

2.1 | Theoretical overview

2.1.1 | Virtuality-as-paradox

According to the literature on virtuality, technology dependence and geographic dispersion are defining features of virtuality (Dulebohn & Hoch, 2017; Gibbs et al., 2017). The virtuality-as-paradox perspective builds on this view and imagines virtuality's defining dimensions as forces that engender paradoxical tensions: They simultaneously create challenges and opportunities for teams. For example, technology dependence creates interactions that are both impersonal and less biased, and geographic dispersion increases both isolation and autonomy.[2] Importantly, this perspective grew out of qualitative research on *organizational* teams (e.g. Cousins et al., 2007; Dubé & Robey, 2008), and, in the tradition of practice theory (Bourdieu, 1990; Sandberg & Tsoukas, 2011), it aims to capture how virtuality is constituted and experienced by teams. Because the central interest of our meta-analysis lies in

understanding the effects of virtuality on *organizational* virtual teams, the virtuality-as-paradox perspective is an appropriate theoretical backdrop.

2.1.2 | Team effectiveness

Lamenting that team effectiveness is often measured with quantitative outcomes which often "do not address other outcome dimensions [....] that are also consequential for any team's long-term organizational performance" (p. 272), Hackman and Wageman (2005) defined team effectiveness as a three-dimensional concept consisting of productive, social, and individual outcomes. Productive outcomes are the products, services, or decisions a team has produced, that meet or exceed standards for quality, quantity, and timeliness. In research, productive outcomes have been operationalized either objectively, using various productivity metrics (i.e. financial results, sales, profitability, mission accomplishment, efficiency, accuracy, and process improvement), or subjectively, using performance ratings (i.e. team-leader or team-member ratings of team performance, customer ratings of quality or team effectiveness; Mathieu et al., 2008). Social outcomes are another essential aspect of overall team effectiveness; they capture the strength of the interpersonal bonds among team members, and enhance team members' ability and desire to work interdependently in the future. In research, social outcomes have been operationalized as viability, commitment, cohesion, trust, and identification, among others (e.g. Balkundi & Harrison, 2006; Breuer et al., 2016; DeChurch et al., 2013; LePine et al., 2008; Wang et al., 2014). Finally, Hackman and Wageman (2005) argued that team members' personal (individual) outcomes cannot be ignored as part of the overall effectiveness of a team. Accordingly, effective teams are those whose members learn new skills, develop positive interpersonal relationships, and enjoy improved well-being as a result of the team experience. Examples of individual outcomes examined in research include job/task satisfaction, learning, intrinsic motivation, and health and well-being (e.g. Adamovic, 2018; Nurmi & Hinds, 2016a).

2.2 | Virtuality and team effectiveness

2.2.1 | In organizational teams

Consistent with the virtuality-as-paradox perspective, research on *organizational* teams has documented how the two core virtuality dimensions of technology dependence and geographic dispersion impact productive, social, and team member outcomes both negatively, and positively. In terms of productive outcomes, technology dependence often results in mistakes due to misunderstandings, confusion, and information overload (Presbitero, 2021), and geographic dispersion poses coordination challenges due to time zone differences (Prasad et al., 2017a) and lack of team member colocation (Charlier et al., 2016). These virtuality features hurt productive outcomes. However, technology allows senior leaders or experts from across the organization to easily consult teams, provide direction, or offer solutions (Baskerville & Nandhakumar, 2007). And, geographic dispersion allows virtual teams to stretch their work cycle across time zones which increases time-on-task beyond 8 h a day (Carmel et al., 2010). Geographic dispersion also allows virtual teams to include members with special skills and qualifications from different cities, countries, and even continents which increases

the level of expertise on the team (Boh et al., 2007a). These virtuality features help improve productive outcomes.

In terms of social outcomes, technology dependence creates difficulties in establishing emotional connections with teammates (Furst et al., 2004; Hacker et al., 2019) because it removes social and contextual meaning and nonverbal cues, and geographic dispersion eliminates spontaneous "watercooler" interactions (Bartel et al., 2012), introduces language and cultural barriers (Sarker & Sahay, 2004), and creates geographic and cultural faultlines within teams (Zakaria, 2017). These virtuality features hurt social outcomes. However, technology makes virtual communication less biased (Triana et al., 2012) and improves perceptions of equality (Purvanova et al., 2021), while geographic dispersion isolates team members from the gossip and politics going on at local offices and creates a psychologically safe team climate (Kirkman et al., 2013a). These virtuality features help improve social outcomes.

Finally, in terms of individual team member outcomes, technology causes virtual team members to feel overwhelmed (Barley et al., 2011) and disrupts their work–life balance (Adamovic, 2018); in turn, geographic dispersion is socially detaching and leaves team members feeling isolated (Orhan et al., 2016a). These virtuality features hurt individual team member outcomes. However, team members find technology-enabled virtual projects to be highly motivational (Nurmi & Hinds, 2016a). And, geographic dispersion enables members to perform work anytime and anywhere (Perry et al., 2016a) and also allows team members to build professional relationships with—and to learn from—dispersed experts (Halgin et al., 2015). These virtuality features help improve individual team member outcomes.

The analysis above shows that virtuality both worsens and improves productive, social, and individual team effectiveness outcomes in *organizational* virtual teams. Interpreted through the prism of the virtuality-as-paradox perspective that predicts simultaneous positive and negative effects of virtuality, the evidence suggests that virtuality's positive and negative features cancel each other out. That is, virtuality presents teams with challenges that may drag team effectiveness down, but also with opportunities which may raise team effectiveness up. Therefore, overall, the direct effect of virtuality on team effectiveness in *organizational* teams is likely neutral.

> **Hypothesis 1a.** Virtuality's direct effects on productive, social, and individual team effectiveness outcomes are neutral in studies of organizational virtual teams.

2.2.2 | In non-organizational teams

Scholars have noted that non-organizational studies of virtual teams tend to define virtuality in largely negative terms (Nurmi & Hinds, 2016a). Indeed, Raghuram et al.'s (2019) citation analysis of the virtual teams literature showed that non-organizational studies of virtual teams do depict virtuality as a negative input; our own review of the non-organizational studies in our meta-analytic database confirmed this observation. There is no shortage of virtuality theories that predict poorer outcomes in virtual teams for a variety of reasons, including (a) fewer or "leaner" communication channels used in virtual interactions (media richness theory, Daft & Lengel, 1984; communication theory, Shannon & Weaver, 1949); (b) paucity or complete lack of nonverbal and paraverbal cues (social presence theory; Short et al., 1976); (c) ambiguity in social roles and status (the lack of social context cues hypothesis; Sproull & Kiesler, 1986); (d) users' aversion to lean communication tools and preferences for richer tools (electronic propinquity theory; Korzenny, 1978); (e) beliefs that virtual communication is less trustworthy

than in-person communication (signaling theory; Donath, 2007); and (f) perceptions that technology-mediated communication is more effortful (efficiency framework; Nowak et al., 2005). Influenced by this theoretical body, the general expectation in much of the non-organizational virtual teams literature has traditionally been that virtuality impacts a wide array of team effectiveness outcomes negatively.

Indeed, prior meta-analyses have strongly supported this negative view, reporting negative effects of virtuality for a number of team effectiveness outcomes (e.g. lower decision accuracy, lower efficiency, less knowledge sharing, poorer perceptions of the team experience, and higher rates of intrateam disagreement; Baltes et al., 2002; Dennis & Wixom, 2002; Fjermestad, 2004; Lim et al., 2007; Ortiz de Guinea et al., 2012) with few exceptions (e.g. greater idea generation, Dennis et al., 2001; Fjermestad, 2004; greater participation equality, Rains, 2005). As these prior meta-analyses are based almost exclusively on studies of non-organizational teams, we expect to replicate their findings using an updated dataset of non-organizational virtual teams studies (the last meta-analysis on the effects of virtuality on team outcomes, Ortiz de Guinea et al., 2012, included studies published up to 2010).

> **Hypothesis 1b.** Virtuality's direct effects on productive, social, and individual team effectiveness outcomes are significantly negative in studies of non-organizational virtual teams.

2.2.3 | Contrasting organizational and non-organizational virtual teams

The theoretical and empirical distinctions we outlined above between organizational and non-organizational virtual teams research suggest that *sample type* may be a crucially important moderator of the virtuality-team effectiveness relationship. Thus, based on extant theory and research findings, we expect that non-organizational virtual teams research might not generalize to organizational virtual teams, yielding significantly more negative results than organizational virtual teams research.

> **Hypothesis 1c.** Sample type moderates the effects of virtuality on productive, social, and individual team effectiveness outcomes, such that non-organizational studies yield significantly more negative effect sizes than organizational studies.

However, an important second objective of our research is to better understand when results generalizability from non-organizational to organizational virtual teams might be poor. We look to the literature on study design and external validity (Bracht & Glass, 1968; Campbell, 1957; Gibson, 1979; Hammond, 1998a), which outlines three central threats to external validity: population validity (Bracht & Glass, 1968), ecological validity (Carlsmith et al., 1976; Gibson, 1979), and representative design (Brunswik, 1956; Hammond, 1998b). *Population validity* deals with generalizations to populations of persons (e.g. who could be expected to behave in the same ways as participants in a study did). To ensure greater population validity, a researcher must identify a representative sample of the population to which to generalize the study's results. *Ecological validity* deals with the realism of the study procedures and settings (e.g. do participants assign meaning to the situation and to their actions). To ensure greater ecological validity, a researcher must employ methodologies and procedures that generate a sense of realism and buy-in within participants. *Representative design* deals with generalizations to environments

(e.g. under what conditions can the same results be expected). To ensure a more representative design, a researcher must operationalize the independent variable in a way that is true to the underlying construct.

In the virtual teams literature, scholars have questioned the generalizability of results from non-organizational to organizational virtual teams for each of the three reasons described above. Specifically, Gibbs et al. (2017) criticized virtual teams research on the grounds that student virtual teams are fundamentally different from organizational virtual teams; this echoes the concept of *population validity*. McGrath and Tschan (2007) criticized virtual teams research on the grounds that one-shot, short-lived virtual teams are not equivalent to virtual teams working on meaningful tasks over time; this echoes the concept of *ecological validity*. Kirkman et al. (2012) criticized virtual teams research on the grounds that modeling virtuality as a "face-to-face" versus "virtual" dichotomy captures only the two extreme points of virtuality; this echoes the concept of *representative design*. However, concerns about results generalizability are not omnipresent in the virtual teams literature. Mesmer-Magnus et al. (2011) argued that student participants are not inherently inappropriate, and Walther (2011) argued that dichotomizing virtuality may not necessarily be a poor choice. To address the presently unexplored notion that not all non-organizational virtual teams research is created equal, we take a deeper look at the role of methodological moderators of the virtuality-team effectiveness relationship below.

2.3 | Results generalizability

2.3.1 | Population validity

Discussions of population validity often revolve around the issue of using student samples (e.g. Gordon et al., 1986; Peterson, 2001). However, scholars suggest that the use of students per se is not a poor research practice (Highhouse & Gillespie, 2009). Rather, a greater threat to population validity is using samples whose characteristics interact with the construct under investigation in a way that confounds the results (Campbell & Gingrich, 1986; Druckman & Kam, 2011). For example, if participants lack relevant knowledge which individuals in the targeted population possess, then study participants may act in ways that are dissimilar to how members of the targeted population act, preventing valid inferences from such participants (Highhouse & Gillespie, 2009). Whereas it is reasonable to assume that undergraduate students may act in dissimilar ways relative to the employed adults who work in virtual teams (e.g. Gibbs et al., 2017), graduate students may be a more representative population to sample. Graduate students are employed adults who likely have experience working on teams, perhaps even virtual teams. Hence, consistent with the logic suggested by the population validity concept, we distinguish between non-organizational studies of virtual teams that employ undergraduate students, and studies that employ graduate students. We expect that results from studies of undergraduate students would differ from organizational virtual teams studies (which are based on employed adults), whereas results from studies of graduate students would not.

> **Hypothesis 2a.** Participant type moderates the effects of virtuality on productive, social, and individual team effectiveness outcomes, such that non-organizational studies using undergraduate students yield significantly more negative results than organizational studies using employees, whereas non-organizational studies using graduate students yield comparable results to organizational studies using employees.

2.3.2 | Ecological validity

Ecological validity refers to the realism (or lack thereof) of non-field research (Hammond, 1998a). Realism is considered to be high when participants assign meaning to the situation they are in and to the behavior they are carrying out (Berkowitz & Donnerstein, 1982; Dobbins et al., 1988; Druckman & Kam, 2011). It is reasonable to assume that short-lived, one-shot non-organizational virtual teams are not equivalent to organizational virtual teams that work together on meaningful projects over time. As McGrath and Tschan (2007) argued, it might be hard for participants in a team of strangers working on a low stakes assignment for about 60 min or less to commit to their team, to the project, or to the study. In contrast, realism is greater and participants' behavioral and attitudinal commitment is higher when participants (1) engage with their team longer and (2) are more invested in the task. Hence, consistent with the logic suggested by the ecological validity concept, we distinguish between non-organizational studies that employ short-lived teams and studies that create longer team engagements. We expect that results from studies of short team assignments would differ from studies of organizational virtual teams (where average team tenure is reported to be 1.5 years; (Purvanova, 2014), whereas results from studies of longer team assignments would not.

We also distinguish between non-organizational studies conducted in the lab versus the classroom because these settings create differing levels of meaningfulness and participant investment in the task. Specifically, laboratory studies typically engage teams in tasks of fictitious nature and no real consequences, whereas classroom studies typically incentivize teams through creating meaningful class projects and rewarding team members with a grade. We therefore expect that results from lab studies of virtual teams would differ from studies of organizational virtual teams (where team members work on real life, high-stakes projects), whereas results from classroom studies would not.

> **Hypothesis 2b.** Team duration moderates the effects of virtuality on productive, social, and individual team effectiveness outcomes, such that non-organizational studies of short team duration yield significantly more negative results than organizational studies of long duration, whereas non-organizational studies of medium team duration yield comparable results to organizational studies of long duration.

> **Hypothesis 2c.** Study setting moderates the effects of virtuality on productive, social, and individual team effectiveness outcomes, such that non-organizational studies conducted in the lab yield significantly more negative results than organizational studies, whereas non-organizational studies conducted in the classroom yield comparable results to organizational studies.

2.3.3 | Representative design

Representative design deals with the operationalization of the independent variable and the degree to which it corresponds to the underlying construct (Brunswik, 1956; Hammond, 1998b). It is reasonable to argue that representing virtuality as a "face-to-face" versus "virtual" dichotomy (or a "low virtuality" versus "high virtuality" dichotomy) does not capture the entirety of the virtuality construct because sampling from the construct's extremes only leaves teams of intermediate degrees of virtuality out (e.g. Kirkman et al., 2012). For example,

teams that rely on a mix of in-person and technology-mediated communication, as well as teams comprised of some colocated and some dispersed team members, are not represented (or modeled) when researchers operationalize the independent variable—virtuality—by its extremes. In contrast, when a continuous measure of virtuality is utilized, researchers are more likely to sample teams of differing levels of virtuality. For example, researchers may create different levels of technology dependence in an experimental setting (e.g. create teams that interact fully or mostly face-to-face, fully or mostly virtually, and through a mix of face-to-face and virtual tools), and/or researchers may create different levels of geographic dispersion in an experimental setting (e.g. create teams with fully dispersed team members, with fully colocated team members, and with a mix of dispersed and colocated team members). In these instances, researchers come closer to representing the virtuality construct. Hence, consistent with the logic suggested by the representative design concept, we distinguish between non-organizational studies that operationalize virtuality dichotomously and studies that use continuous operationalizations. We expect that results from non-organizational studies that operationalize virtuality as a dichotomous construct would differ from studies of organizational virtual teams (where the full range of virtuality is observed), whereas results from non-organizational studies that operationalize virtuality as a continuous construct would not.

> **Hypothesis 2d.** Virtuality measurement moderates the effects of virtuality on productive, social, and individual team effectiveness outcomes, such that non-organizational studies that dichotomize virtuality yield significantly more negative results than organizational studies utilizing continuous virtuality measurement, whereas non-organizational studies than represent virtuality as a continuous construct yield comparable results to organizational studies utilizing continuous virtuality measurement.

3 | METHOD

3.1 | Literature search and inclusion criteria

We employed a multi-pronged strategy to locate primary studies on virtual teams available as of December 2019. First, we searched library databases (e.g. *EBSCO*, *JSTOR, PsycINFO*, *Web of Science*, and *Digital Dissertations*) as well as free-access search engines (e.g. *GoogleScholar* and *ResearchGate*) for journal articles, dissertations, conference papers, book chapters, and working papers on virtual teams, using the key word "team" with the qualifiers "virtual," "geographically dispersed," "dispersed," "distributed," "global," and "computer-mediated." Second, we reviewed the reference lists of 19 qualitative (e.g. Gibson et al., 2014; Gilson et al., 2015) and 11 quantitative (e.g. Baltes et al., 2002; Benbasat & Lim, 1993) reviews of the virtual teams literature.[3] Third, we searched the websites of journals that publish virtual teams research (e.g. *Journal of Applied Psychology*, *Organization Science*, and *Human-Computer Interaction*) for in-press and online first articles. Through these efforts, we identified 1096 citations to examine for inclusion.

Studies had to satisfy four criteria to be included in the analyses. First, studies had to report at least one bivariate correlation (or statistical information needed to compute an effect size) between virtuality and an aspect of team effectiveness (e.g. productive outcomes, social outcomes, and/or individual team member outcomes). Second, studies had to employ an

established operationalization of virtuality, such as "face-to-face"/"virtual" (or "low virtuality"/"high virtuality"), geographic dispersion (e.g. distance among team members), or technology dependence (e.g. degree of reliance on communication technology). Studies employing unusual virtuality measures, such as using avatars, were excluded. Third, because our interest is in understanding outcomes in the context of virtual teams, studies had to include virtual teams and their members, not telecommuters or free-lance virtual workers. Fourth, because effect sizes drawn from different levels of analysis should not be merged in meta-analyses (Ostroff & Harrison, 1999), we required that studies of team outcomes (e.g. earnings, cohesion, etc.) report team-level relationships between virtuality and these outcomes, as well as that studies of individual team member outcomes (e.g. project satisfaction, relationship quality) report individual-level relationships. Studies which reported on team effectiveness outcomes at the individual level, as well as studies which reported on individual outcomes at the team level, were excluded.[4] The final database consisted of 175 studies with 182 unique samples that included a total of 11,358 teams (for team-level studies) and 9856 team members (for individual-level studies).

3.2 | Coding of studies

The first author and two research assistants coded all studies following rigorous training and using standardized procedures. The following information was extracted from each study (% agreement shown in parentheses): sample size (number of teams and number of team members; 100%), participants type (100%), description of the task (100%), team duration (93%), study setting (98%), virtuality measurement (100%), virtuality operationalization (98%), outcome operationalization (94%), scale reliabilities (98%), level-of-analysis for each effect size (individual or team; 91%), and reported effect size (e.g. correlation coefficient, *t*-test, chi-square; 94%). All discrepancies in the coding of study information were resolved by the first author. Following the coding of study information, we created composite variables using Mosier's (1943) eq. 8 to ensure sample independence in cases where (a) relationships between an outcome and two or more virtuality indices were reported and (b) relationships between virtuality and two or more conceptually similar outcomes (e.g. project satisfaction and task satisfaction) were reported. Effect sizes were coded such that higher values represent better outcomes at higher levels of virtuality. Any effect sizes reported in the opposite direction were reverse-coded. Hence, a positive *r* indicates better outcomes at higher levels of virtuality, whereas a negative *r* indicates worse outcomes at higher levels of virtuality.

3.3 | Coding of variables

3.3.1 | Team effectiveness outcomes

We conceptualized *team effectiveness* as four higher-order outcomes: productive outcomes, performance outcomes, social outcomes, and individual team member outcomes (Hackman & Wageman, 2005). We differentiated between 'productive' and 'performance' outcomes because productive outcomes (e.g. earnings) capture results, whereas performance outcomes (e.g. customer satisfaction ratings) capture performance (Mathieu et al., 2008). To reveal a more detailed picture of the impact of virtuality on discrete team effectiveness outcomes, we further

grouped identical and conceptually similar measures into lower-order outcomes consistent with prior meta-analyses (e.g. Christian et al., 2017; Jones et al., 2016; Leslie et al., 2014). Specifically, within *productive outcomes*, measures were grouped into three specific outcome categories: *earnings, accuracy*, and *process improvements*; within *performance outcomes*, measures were grouped into two specific outcomes: *other-rated team performance* and *team member-rated team performance*; within *social outcomes*, measures were grouped into two specific outcomes: *cohesion* and *team trust*; finally, within *individual team member outcomes*, measures were grouped into two specific outcomes: *project/task satisfaction* and *relational quality*. See Appendix A for details on how team effectiveness outcomes were classified.

3.3.2 | Moderators

We coded five moderators: participant type, team duration, study setting, sample type, and virtuality measurement. For participant type, we indicated "1" for studies of undergraduate students, "2" for studies of graduate students, and "3" for studies of employees. A small number of studies used a mix of undergraduate and graduate students, or a mix of undergraduate students and university employees; we employed a conservative approach and coded these studies "2." Team duration was coded based on information about how long the teams in the study were in existence; studies were coded as short ("1"), medium ("2"), and long ("3"). Studies coded "short" focused on short-lived teams that interacted together from 25 min to 3 h (the mean and the mode in this category was 60 min). Studies coded "medium" included teams that interacted together from 10 days to 4 months (the mean in this category was 25 days and the mode was 30 days). Studies coded "long" included teams that interacted together from 6 months to 4 years (the mean in this category was 2.25 years and the mode was 2 years). Fourteen organizational samples (of 73) did not report information on the longevity of the teams they studied; we imputed the average value observed across organizational studies (2.25 years) and coded these studies "3" as well.

Study setting was coded "1" for lab studies, "2" for classroom studies, and "3" for organizational studies following Gibbs et al. (2017). Specifically, lab studies are those where ad hoc teams work on tasks as part of a research study on a voluntary basis, for the possibility to win a prize, or for extra credit (as opposed to a grade). Classroom studies are those where teams work on projects as part of regular, graded class activities. Organizational studies are those where teams complete tasks/projects as part of their work responsibilities. Thus, studies coded "1" and "2" constitute our non-organizational studies for our sample type moderator, and studies coded "3" constitute the organizational studies. Finally, virtuality measurement was coded "1" for studies that employed a dichotomous measure of virtuality (i.e. "face-to-face" versus "virtual," or "low virtuality" versus "high virtuality") and "2" for studies that employed a continuous measure (i.e. reported on teams of various degrees of virtuality).

Appendix B shows the distribution of these sample characteristics within each of the four team effectiveness outcomes we meta-analyzed. To be maximally informative, the appendix crosses study setting (lab, classroom, and organization) with participant type (undergraduate students, graduate students, and employees), team duration (short, medium, and long), and virtuality measurement (dichotomous and continuous), and additionally provides information about year of publications. The appendix shows that for each outcome, there is a fairly large number of samples in the moderator categories, making our moderator analyses viable. Finally, please see Appendix S1 for a detailed listing of each study included in the meta-analysis, along with the effect size(s) each study contributed and a description of each study's characteristics.

3.4 | Meta-analytic procedures

We used Schmidt and Le's (2004) meta-analysis program to calculate sample size weighted effect sizes corrected for criterion reliability (r_c), and the associated corrected sampling error variances ($\mathrm{Var}_{[e']}$). Where reliability was not reported, we used the average reliability across studies; we assumed perfect reliability for productive outcomes (e.g. earnings). To perform the meta-analysis itself, we used Wilson's meta-analysis macros for SPSS (Lipsey & Wilson, 2001; Wilson, 2005). We applied restricted maximum likelihood random-effects procedures on the corrected effect sizes (r_c) and corrected sampling error variances ($\mathrm{Var}_{[e']}$). We chose random- over fixed-effects procedures to account for heterogeneity in effect sizes beyond that produced by sampling error. Random-effects procedures achieve this by adding a random-effects variance component to the sampling error variance component in the calculation of the standard error variance (Raudenbush & Bryk, 2002). Each effect size is then weighed by the inverse of this variance. Wilson's meta-analysis macros produce a Z test that establishes whether the meta-analytic effect size (ρ) differs from zero (Lipsey & Wilson, 2001). For hypotheses testing purposes, we report the Z test, and for completeness, we also report the 95% confidence intervals around ρ's. To assess homogeneity of effect sizes, we examine the Q_w statistic.

To examine the issue of results generalizability from non-organizational to organizational studies, as well as to investigate the moderating role of our moderators on the virtuality-team outcomes link, we cued in Wilson's meta-ANOVA macro that compares mean effect sizes across categories. To test for significance, we examined the Q_B statistic, which is the meta-ANOVA analogue to F; it is distributed as a chi-square, and evaluated on $df = \mathrm{k}$ where k is the number of categories. Similar to F, a significant Q_B indicates that the effect size in at least one category (e.g. non-organizational studies) differs significantly from the effect size in another category (e.g. organizational studies).

4 | RESULTS

Table 1 presents results of meta-analyses that speak to Hypotheses 1a–c, for each of the four broad team effectiveness outcomes (i.e. productive, performance, social, and individual), as well as each specific outcome (i.e. earnings, accuracy, process improvements, other-rated team performance, team member-rated team performance, cohesion, team trust, project/task satisfaction, and relational quality). The table reports the total number of independent samples included in each meta-analysis (k), the total number of teams (for team-level outcomes) or individuals (for individual-level outcomes; N), the meta-analytic effect size (ρ) where positive values indicate beneficial effects and negative values indicate detrimental effects of virtuality on outcomes, the standard deviation of ρ (SD_ρ), the 95% confidence interval around ρ (95% CI_ρ), the test of significance of the meta-analytic effect size from zero (Z), the test of homogeneity of effect sizes (Q_w), and the test of significance of the meta-analytic contrasts (Q_B).

H1a stated that in *organizational* teams, virtuality's influence on team outcomes is neutral. We obtained strong support for H1a (see the lines marked "Organizational" in Table 1), as results show non-significant (i.e. neutral) effect sizes in organizational samples. Specifically, virtuality did not relate significantly to any of the four broad team effectiveness outcomes: $\rho_{\mathrm{PRODUCTIVE}} = 0.07$ ($\mathrm{SE}_{(\rho)} = .066$, CI = [−.06; .20], $Z = 1.10$, $p = .271$), $\rho_{\mathrm{PERFORMANCE}} = 0.01$ ($\mathrm{SE}_{(\rho)} = .028$, CI = [−.04; .07], $Z = 0.36$, $p = .718$), $\rho_{\mathrm{SOCIAL}} = -0.03$ ($\mathrm{SE}_{(\rho)} = .025$, CI = [−.08; .02], $Z = -1.30$, $p = .195$), and $\rho_{\mathrm{INDIVIDUAL}} = -0.04$ ($\mathrm{SE}_{(\rho)} = .024$, CI = [−.08; .01], $Z = -1.46$,

TABLE 1 Random effects meta-analyses of virtuality and team effectiveness outcomes in organizational and non-organizational teams

Team Effectiveness[a]	*k*	*N*	ρ	$SE_{(\rho)}$	95% $CI_{(\rho)}$	*Z*	Q_w	CONTRASTS (Q_B)
Productive								
Outcomes[a]	56	5044	−.12**	.039	[−.20; −.05]	−3.14**	351.61**	
Organizational	9	2288	.07	.066	[−.06; .20]	1.10	57.89**	
Non-Org.	47	2756	−.16**	.042	[−.25; −.08]	−3.93**	198.77**	
Org v Non-Org								7.41**
Earnings	16	1303	−.13	.090	[−.31; .04]	−1.47	123.25**	
Organizational	4	851	.13	.133	[−.13; .39]	0.98	33.42**	
Non-Org.	12	452	−.24*	.096	[−.43; −.05]	−2.47*	42.27**	
Org v Non-Org								5.54*
Accuracy	38	3010	−.12**	.045	[−.21; −.03]	−2.58**	201.54**	
Organizational	3	724	.05	.037	[−.02; .13]	1.47	1.29	
Non-Org.	35	2286	−.13**	.049	[−.23; −.04]	−2.70**	175.17**	
Org v Non-Org								1.65
Process								
Improvements	11	2019	−.03	.043	[−.12; .05]	−0.79	24.94**	
Organizational	5	1437	.04	.059	[−.07; .16]	0.70	10.82*	
Non-Org.	6	582	−.11*	.047	[−.20; −.02]	−2.36*	6.11	
Org vs Non-Org								8.00**
Performance								
Outcomes[a]	87	7425	−.04	.025	[−.09; .01]	−1.69	250.95**	
Organizational	40	3989	.01	.028	[−.04; .07]	0.36	70.58**	
Non-Org.	47	3436	−.09*	.039	[−.17; −.01]	−2.21*	161.84**	
Org v Non-Org								3.68
Other-Rated	58	5780	−.02	.028	[−.08; .03]	−0.75	159.28**	
Organizational	30	3503	−.01	.027	[−.06; .04]	−0.42	46.08*	
Non-Org.	28	2277	−.02	.056	[−.13; .09]	−0.27	111.86**	
Org v Non-Org								0.05
Team Member-Rated	44	3456	−.09*	.037	[−.16; −.01]	−2.34*	141.07**	
Organizational	15	866	−.03	.068	[−.16; .10]	−0.42	39.75**	
Non-Org.	29	2599	−.12*	.046	[−.21; −.02]	−2.50*	101.26**	
Org v Non-Org								0.98
Social								
Outcomes[a]	63	3909	−.15**	.030	[−.21; −.09]	−4.88**	156.84**	
Organizational	24	1735	−.03	.025	[−.08; .02]	−1.30	22.59	
Non-Org.	39	2174	−.20**	.043	[−.29; −.12]	−4.70**	115.67**	
Org v Non-Org								5.50*
Cohesion	49	3077	−.14**	.034	[−.21; −.07]	−4.10**	124.06**	
Organizational	18	1544	−.03	.027	[−.08; .02]	−1.15	12.28	
Non-Org.	31	1533	−.19**	.052	[−.29; −.09]	−3.65**	96.07**	
Org v Non-Org								3.03
Team Trust	26	1363	−.13*	.052	[−.23; −.03]	−2.53*	64.14**	
Organizational	10	379	−.04	.064	[−.17; .08]	−0.64	10.58	
Non-Org.	16	984	−.19**	.071	[−.32; −.05]	−2.63**	51.98**	
Org v Non-Org								1.86

TABLE 1 (Continued)

Team Effectiveness[a]	*k*	*N*	ρ	$SE_{(\rho)}$	95% $CI_{(\rho)}$	*Z*	Q_w	CONTRASTS (Q_B)
Team Member								
Outcomes[a]	49	9856	−.11**	.027	[−.16; −.06]	−4.14**	277.71**	
Organizational	21	4648	−.04	.024	[−.08; .01]	−1.46	41.79**	
Non-Org.	28	5208	−.17**	.043	[−.26; −.09]	−4.10**	206.39**	
Org v Non-Org								6.28*
Project/Task								
Satisfaction	31	6153	−.11**	.036	[−.19; −.04]	−3.17**	194.14**	
Organizational	11	2282	−.02	.033	[−.08; .05]	−0.58	17.62	
Non-Org.	20	3871	−.17**	.051	[−.27; −.07]	−3.31**	158.37**	
Org v Non-Org								3.83*
Relational								
Quality	24	4919	−.13**	.036	[−.20; −.06]	−3.64**	112.04**	
Organizational	12	2559	−.07	.036	[−.14; .00]	−1.86	28.76**	
Non-Org.	12	2360	−.20**	.062	[−.32; −.08]	−3.28**	71.22**	
Org v Non-Org								2.95

Abbreviations: *k*, number of effect sizes (independent samples); *N*, total number of teams (for the first three outcomes) or total number of team members (for the fourth outcome); *Org v Non-Org*, organizational versus non-organizational studies contrasts; Q_B, meta-ANOVA analogue to *F*, distributed as chi-square and evaluated on $df = 1$; Q_W, chi-square test of homogeneity of effect sizes; SE_ρ, standard error of the meta-analytic effect size; *Z*, test of significance of the effect size from zero; 95% $CI_{(\rho)}$, 95% confidence interval around the meta-analytic effect size; ρ, meta-analytic effect size: positive values indicate beneficial effects of virtuality, negative values indicate detrimental effects of virtuality.
[a]*k*'s do not sum up to total *k*'s because some samples reported on more than one outcome.
*$p \leq .05$.
**$p \leq .01$.

$p = .146$). Similarly, there were no significant relationships between virtuality and any of the nine specific outcomes: $\rho_{\text{EARNINGS}} = 0.13$ ($p = .329$), $\rho_{\text{ACCURACY}} = 0.05$ ($p = .145$), $\rho_{\text{PROCESS IMPROVEMENTS}} = 0.04$ ($p = .483$), $\rho_{\text{OTHER-RATED PERFORMANCE}} = -0.01$, ($p = .676$), $\rho_{\text{TEAM MEMBER-RATED PERFORMANCE}} = -0.03$ ($p = .678$), $\rho_{\text{COHESION}} = -0.03$ ($p = .250$), $\rho_{\text{TRUST}} = -0.04$ ($p = .523$), $\rho_{\text{SATISFACTION}} = -0.02$ ($p = .565$), and $\rho_{\text{RELATIONAL QUALITY}} = -0.07$ ($p = .063$). These results—which are new to the literature—suggest that virtuality is not a direct determinant of team effectiveness, whether negative or positive, within *organizational* teams, consistent with the virtuality-as-paradox perspective.

H1b stated that in non-organizational teams, virtuality's influence on team outcomes is negative. We obtained strong support for H1b (see the lines marked "Non-Organizational" in Table 1). Specifically, virtuality was significantly negatively linked to each of the four broad team effectiveness outcomes: $\rho_{\text{PRODUCTIVE}} = -0.16$ ($SE_{(\rho)} = .042$, $CI = [-.25; -.08]$, $Z = -3.93$, $p < .001$), $\rho_{\text{PERFORMANCE}} = -0.09$ ($SE_{(\rho)} = .039$, $CI = [-.17; -.01]$, $Z = -2.21$, $p = .027$), $\rho_{\text{SOCIAL}} = -0.20$ ($SE_{(\rho)} = .043$, $CI = [-.29; -.12]$, $Z = -4.70$, $p < .001$), and $\rho_{\text{INDIVIDUAL}} = -0.17$ ($SE_{(\rho)} = .043$, $CI = [-.26; -.09]$, $Z = -4.10$, $p < .001$). Similarly, there were significant negative relationships between virtuality and eight of the nine specific outcomes: $\rho_{\text{EARNINGS}} = -0.24$ ($p = .014$), $\rho_{\text{ACCURACY}} = -0.13$ ($p = .007$), $\rho_{\text{PROCESS IMPROVEMENTS}} = -0.11$ ($p = .019$), $\rho_{\text{TEAM MEMBER-RATED PERFORMANCE}} = -0.12$ ($p = .013$), $\rho_{\text{COHESION}} = -0.19$ ($p < .001$), $\rho_{\text{TRUST}} = -0.19$ ($p = .009$), $\rho_{\text{SATISFACTION}} = -0.17$ ($p < .001$), and $\rho_{\text{RELATIONAL QUALITY}} = -0.20$ ($p < .001$). The only exception to this pattern of significant negative results occurred in the case of other-rated team performance: $\rho_{\text{OTHER-RATED PERFORMANCE}} = -0.02$, ($p = .787$). These results are highly consistent with previously reported meta-analytic estimates, likely because prior meta-analyses have been largely or exclusively based on non-organizational studies.

H1c stated that sample type moderates the virtuality-team effectiveness relationship such that the direct effect of virtuality is significantly more negative in non-organizational studies than in organizational studies. Results of meta-analytic contrast tests comparing, for each outcome, the effect size from organizational studies to the effect size from non-organizational studies are shown in the "Org v Non-Org" lines of Table 1. H1c was supported as the contrast tests produced significant Q_B's in the case of productive outcomes ($Q_{B(\text{PRODUCTIVE})} = 7.41$, $p = .007$), social outcomes ($Q_{B(\text{SOCIAL})} = 5.50$, $p = .019$), and individual team member outcomes ($Q_{B(\text{INDIVIDUAL})} = 6.28$, $p = .012$) and approached significance in the case of performance outcomes ($Q_{B(\text{PERFORMANCE})} = 3.68$, $p = .055$).[5] These results show that *non-organizational* samples produce significantly more negative results than *organizational* samples for most broad team effectiveness outcomes; however, this is not always the case for specific outcomes (see footnote 5). Hence, consistent with H1c, results generalizability from non-organizational to organizational studies is poor when the four core dimensions of team effectiveness are considered—productive, performance, social, and individual outcomes.

In addition to results that speak to H1a–c, Table 1 also presents results on the overall effect of virtuality on team effectiveness outcomes across our entire dataset (i.e. organizational and non-organizational samples combined; see the top line for each outcome). The significantly negative effect size estimates observed for three of the four broad outcomes (productive, social, and individual) and for six of the nine specific outcomes (accuracy, team member -rated performance, cohesion, team trust, project/task satisfaction, and relational quality) suggest that merging together results from organizational and non-organizational studies provides a misleadingly negative view on the direct effects of virtuality on team outcomes.

Hypotheses 2a–d focus on the effects of four moderators that may explain the poor results generalizability across organizational and non-organizational virtual teams studies: participant type (H2a), team duration (H2b), study setting (H2c), and virtuality measurement (H2d). To begin, Hypothesis 2a stated that non-organizational studies of undergraduate students yield significantly more negative results than organizational studies of employees, whereas non-organizational studies of graduate students yield comparable results to studies of employees. Table 2 reports results for each of the four broad team effectiveness outcomes that support H2a. Specifically, contrast tests showed that studies of undergraduate student participants yielded more negative results than studies of employees for all outcomes: $Q_{B(\text{PRODUCTIVE})} = 7.82$, $p < .001$ ($\rho_{\text{UNDERGRADS}} = -0.17$ vs. $\rho_{\text{EMPLOYEES}} = 0.07$), $Q_{B(\text{PERFORMANCE})} = 3.64$, $p = .054$ ($\rho_{\text{UNDERGRADS}} = -0.09$ vs. $\rho_{\text{EMPLOYEES}} = 0.01$), $Q_{B(\text{SOCIAL})} = 4.95$, $p = .026$ ($\rho_{\text{UNDERGRADS}} = -0.21$ vs. $\rho_{\text{EMPLOYEES}} = -0.03$), and $Q_{B(\text{INDIVIDUAL})} = 9.95$, $p < .001$ ($\rho_{\text{UNDERGRADS}} = -0.20$ vs. $\rho_{\text{EMPLOYEES}} = -0.04$). In contrast, studies of graduate student participants yielded comparable results to studies of employees for three of the four broad outcomes (social outcomes was the exception). These results give credence to concerns related to the issue of population validity, or using student participants in virtual teams research, but with an important caveat: Graduate students appear viable participants.

Hypothesis 2b stated that non-organizational studies of short team duration yield significantly more negative results than organizational studies of long duration, whereas non-organizational studies of medium team duration yield comparable results to studies of long duration. Table 3 reports results for each of the four broad team effectiveness outcomes that strongly support H2b. Specifically, contrast tests showed that studies of short-team duration yielded more negative results than studies of longer team duration for all outcomes: $Q_{B(\text{PRODUCTIVE})} = 9.56$, $p < .001$ ($\rho_{\text{SHORT}} = -0.21$ vs. $\rho_{\text{LONG}} = 0.07$), $Q_{B(\text{PERFORMANCE})} = 11.29$, $p < .001$ ($\rho_{\text{SHORT}} = -0.19$ vs. $\rho_{\text{LONG}} = 0.02$), $Q_{B(\text{SOCIAL})} = 9.71$, $p < .001$ ($\rho_{\text{SHORT}} = -0.26$ vs. $\rho_{\text{LONG}} = -0.03$), and

TABLE 2 Moderators of the virtuality-team outcomes relationship: Participant type

Team effectiveness	*k*	*N*	ρ	$SE_{(\rho)}$	95% $CI_{(\rho)}$	*Z*	CONTRASTS (Q_B)
Productive Outcomes							
Undergrads	38	2237	−.17**	.047	[−.26; −.08]	−3.62**	
Grad students	9	519	−.13	.093	[−.31; .05]	−1.42	
Employees	9	2288	.07	.066	[−.06; .20]	1.10	
Empl-UGs							7.82*
Empl-Gs							3.08
Performance Outcomes							
Undergrads	36	1915	−.09	.053	[−.20; .01]	−1.80	
Grad students	11	1521	−.06	.055	[−.17; .05]	−1.14	
Employees	40	3989	.01	.028	[−.04; .07]	0.36	
Empl-UGs							3.64*
Empl-Gs							1.53
Social Outcomes							
Undergrads	31	1637	−.21**	.051	[−.31; −.11]	−4.05**	
Grad students	8	537	−.20**	.077	[−.35; −.05]	−2.58**	
Employees	24	1735	−.03	.025	[−.08; .02]	−1.30	
Empl-UGs							4.95*
Empl-Gs							7.21**
Team Member Outcomes							
Undergrads	24	4549	−.20**	.042	[−.28; −.12]	−4.74**	
Grad students	4	659	−.01	.180	[−.36; .34]	−0.06	
Employees	21	4648	−.04	.024	[−.08; .01]	−1.46	
Empl-UGs							9.95**
Empl-Gs							0.07

Abbreviations: Empl-Gs, employees versus graduate students studies contrasts; Empl-UGs, employees versus undergraduate students studies contrasts; *k*, number of effect sizes (independent samples); *N*, total number of teams (for the first three outcomes) or total number of team members (for the fourth outcome); Q_B, meta-ANOVA analogue to *F*, distributed as chi-square and evaluated on $df = 1$; SE_ρ, standard error of the meta-analytic effect size; *Z*, test of significance of the effect size from zero; 95% $CI_{(\rho)}$ = 95% confidence interval around the meta-analytic effect size; ρ, meta-analytic effect size: positive values indicate beneficial effects of virtuality, negative values indicate detrimental effects of virtuality.

*$p \leq .05$.

**$p \leq .01$.

$Q_{B(INDIVIDUAL)} = 6.75$, $p < .001$ ($\rho_{SHORT} = -0.20$ vs. $\rho_{LONG} = -0.04$). In contrast, studies of medium team duration yielded comparable results to studies of long team duration for all outcomes. These results give credence to concerns related to one aspect of ecological validity—length of engagement with the team—as they show that studying teams of short duration (typically, 60 min) produces biased results and that studying teams of medium duration (typically, 30 days) alleviates this concern.

Hypothesis 2c stated that lab studies yield significantly more negative results than organizational studies, whereas classroom studies yield comparable results to organizational studies.

TABLE 3 Moderators of the virtuality-team outcomes relationship: Team duration

Team effectiveness	*k*	*N*	ρ	$SE_{(\rho)}$	95% $CI_{(\rho)}$	*Z*	CONTRASTS (Q_B)
Productive Outcomes							
Short	36	2309	−.21**	.048	[−.30; −.11]	−4.32**	
Medium	11	447	−.00	.059	[−.12; .11]	−0.07	
Long	9	2288	.07	.066	[−.06; .20]	1.10	
Long-Short							9.56**
Long-Medium							0.69
Performance Outcomes							
Short	21	1376	−.19**	.061	[−.31; −.07]	−3.10**	
Medium	27	2307	−.01	.046	[−.10; .08]	−0.28	
Long	39	3742	.02	.028	[−.04; .07]	0.62	
Long-Short							11.29**
Long-Medium							0.45
Social Outcomes							
Short	18	1302	−.26**	.058	[−.37; −.15]	−4.51**	
Medium	21	872	−.14*	.064	[−.27; −.02]	−2.25*	
Long	24	1735	−.03	.025	[−.08; .02]	−1.30	
Long-Short							9.71**
Long-Medium							1.42
Team Member Outcomes							
Short	18	2690	−.20**	.062	[−.32; −.08]	−3.20**	
Medium	10	2518	−.13*	.057	[−.24; −.02]	−2.35*	
Long	21	4648	−.04	.024	[−.08; .01]	−1.46	
Long-Short							6.75**
Long-Medium							2.93

Abbreviations: *k*, number of effect sizes (independent samples); *N*, total number of teams (for the first three outcomes) or total number of team members (for the fourth outcome); Q_B, meta-ANOVA analogue to *F*, distributed as chi-square and evaluated on $df = 1$; SE_ρ, standard error of the meta-analytic effect size; *Z*, test of significance of the effect size from zero; 95% $CI_{(\rho)}$, 95% confidence interval around the meta-analytic effect size; ρ, meta-analytic effect size: positive values indicate beneficial effects of virtuality, negative values indicate detrimental effects of virtuality.

*$p \leq .05$.

**$p \leq .01$.

Table 4 reports results for each of the four broad team effectiveness outcomes that largely support H2c. Specifically, contrast tests showed that lab studies yielded more negative results than organizational studies for all outcomes: $Q_{B(\text{PRODUCTIVE})} = 8.29$, $p = .004$ ($\rho_{\text{LAB}} = -0.18$ vs. $\rho_{\text{ORG}} = 0.07$), $Q_{B(\text{PERFORMANCE})} = 9.31$, $p = .002$ ($\rho_{\text{LAB}} = -0.16$ vs. $\rho_{\text{ORG}} = 0.01$), $Q_{B(\text{SOCIAL})} = 8.17$, $p = .004$ ($\rho_{\text{LAB}} = -0.25$ vs. $\rho_{\text{ORG}} = -0.03$), and $Q_{B(\text{INDIVIDUAL})} = 4.76$, $p = .029$ ($\rho_{\text{LAB}} = -0.17$ vs. $\rho_{\text{ORG}} = -0.04$). In contrast, classroom studies yielded comparable results to organizational studies for three of the four broad outcomes (individual outcomes was the exception). These results give credence to concerns related to another aspect of ecological validity—meaningfulness of context—as they demonstrate that artificial activities in the lab produce biased results, but that

TABLE 4 Moderators of the virtuality-team outcomes relationship: Study setting

Team effectiveness	*k*	*N*	ρ	$SE_{(\rho)}$	95% $CI_{(\rho)}$	*Z*	CONTRASTS (Q_B)
Productive Outcomes							
Lab	37	2351	−.18**	.047	[−.28; −.09]	−3.90**	
Classroom	10	405	−.08	.083	[−.24; .09]	−0.91	
Organization	9	2288	.07	.066	[−.06; .20]	1.10	
Org-Lab							8.29**
Org-Classroom							2.02
Performance Outcomes							
Lab	26	1457	−.16*	.056	[−.27; −.05]	−2.82*	
Classroom	21	1979	−.01	.053	[−.11; .10]	−0.11	
Organization	40	3989	.01	.028	[−.04; .07]	0.36	
Org-Lab							9.31*
Org-Classroom							0.15
Social Outcomes							
Lab	24	1448	−.25**	.058	[−.37; −.14]	−4.37**	
Classroom	15	726	−.12*	.059	[−.23; −.00]	−2.04*	
Organization	24	1735	−.03	.025	[−.08; .02]	−1.30	
Org-Lab							8.17*
Org-Classroom							0.99
Team Member Outcomes							
Lab	16	2602	−.17*	.064	[−.29; −.04]	−2.62*	
Classroom	12	2606	−.18*	.058	[−.29; −.07]	−3.10*	
Organization	21	4648	−.04	.024	[−.08; .01]	−1.46	
Org-Lab							4.76*
Org-Classroom							5.40*

Abbreviations: *k*, number of effect sizes (independent samples); *N*, total number of teams (for the first three outcomes) or total number of team members (for the fourth outcome); Org, organization; Q_B, meta-ANOVA analogue to *F*, distributed as chi-square and evaluated on $df = 1$; SE_ρ, standard error of the meta-analytic effect size; *Z*, test of significance of the effect size from zero; 95% $CI_{(\rho)}$, 95% confidence interval around the meta-analytic effect size; ρ, meta-analytic effect size: positive values indicate beneficial effects of virtuality, negative values indicate detrimental effects of virtuality.

*$p \leq .05$.

**$p \leq .01$.

studying teams engaged in higher stakes assignments, such as classroom teams working on projects for a grade, alleviates this concern.

Finally, Hypothesis 2d stated that non-organizational studies representing virtuality as a dichotomy yield significantly more negative results than organizational studies that represent the full virtuality spectrum, whereas non-organizational studies representing virtuality as a continuum yield comparable results to organizational full-virtuality-range studies. Table 5 reports results for each of the four broad team effectiveness outcomes that provide general support for H2d. Specifically, contrast tests showed that non-organizational studies using dichotomous virtuality measures yielded more negative results than organizational studies using continuous

measures for three of the four broad outcomes: $Q_{B(\text{PRODUCTIVE})} = 4.29$, $p = .038$ ($\rho_{\text{DICHOTOMOUS NON-ORG}} = -0.15$ vs. $\rho_{\text{CONTINUOUS ORG}} = 0.03$), $Q_{B(\text{SOCIAL})} = 9.31$, $p = .002$ ($\rho_{\text{DICHOTOMOUS NON-ORG}} = -0.25$ vs. $\rho_{\text{CONTINUOUS ORG}} = -0.04$), and $Q_{B(\text{INDIVIDUAL})} = 8.55$, $p = .004$ ($\rho_{\text{DICHOTOMOUS NON-ORG}} = -0.19$ vs. $\rho_{\text{CONTINUOUS ORG}} = -0.03$); the contrast test for the performance outcome was not significant: $Q_{B(\text{PERFORMANCE})} = 2.67$, $p = .102$ ($\rho_{\text{DICHOTOMOUS NON-ORG}} = -0.08$ vs. $\rho_{\text{CONTINUOUS ORG}} = 0.02$). In contrast, non-organizational studies representing virtuality as a continuum yielded comparable results to organizational full-virtuality-range studies for three of the four broad outcomes (the test for

TABLE 5 Moderators of the virtuality-team outcomes relationship: Virtuality dichotomization

Team effectiveness	*k*	*N*	ρ	$SE_{(\rho)}$	95% $CI_{(\rho)}$	*Z*	CONTRASTS (Q_B)
Productive Outcomes							
Dich. Non-Org	40	2016	−.15**	.035	[−.22; −.09]	−4.43**	
Cont. Non-Org	7	740	−.18	.109	[−.39; .04]	−1.64	
Cont. Org	5	1629	.03	.079	[−.13; .18]	0.32	
Cont. Org–Dich. Non-Org							4.29*
Cont. Org–Cont. Non-Org							1.42
Performance Outcomes							
Dich. Non-Org	38	1845	−.08	.047	[−.18; .01]	−1.81	
Cont. Non-Org	9	1594	−.08	.043	[−.17; .00]	0.06	
Cont. Org	38	3911	.02	.043	[−.07; .10]	0.65	
Cont. Org–Dich. Non-Org							2.67
Cont. Org–Cont. Non-Org							3.44
Social Outcomes							
Dich. Non-Org	33	1708	−.25**	.039	[−.33; −.18]	−6.49**	
Cont. Non-Org	6	466	.05	.051	[−.05; .15]	0.97	
Cont. Org	22	1614	−.04	.029	[−.09; .01]	−1.44	
Cont. Org–Dich. Non-Org							9.31**
Cont. Org–Cont. Non-Org							2.37
Team Member Outcomes							
Dich. Non-Org	27	4933	−.19**	.035	[−.26; −.12]	−5.48**	
Cont. Non-Org	1	275	–	–	–	–	
Cont. Org	17	3810	−.03	.042	[−.11; .05]	−0.71	
Cont. Org–Dich. Non-Org							8.55**
Cont. Org–Cont. Non-Org							–

Abbreviations: Cont Non-Org, continuous non-organizational; Cont. Org, continuous organizational; Dich. Non-Org, dichotomized non-organizational; *k*, number of effect sizes (independent samples); *N*, total number of teams (for the first three outcomes) or total number of team members (for the fourth outcome); Q_B, meta-ANOVA analogue to *F*, distributed as chi-square and evaluated on $df = 1$; SE_ρ, standard error of the meta-analytic effect size; *Z*, test of significance of the effect size from zero; 95% $CI_{(\rho)}$, 95% confidence interval around the meta-analytic effect size; ρ, meta-analytic effect size: positive values indicate beneficial effects of virtuality, negative values indicate detrimental effects of virtuality.

*$p \leq .05$.

**$p \leq .01$.

individual outcomes could not be estimated as only one non-organizational study here used a continuous measure of virtuality). Importantly, as shown in Appendix B, among the non-organizational studies, lab studies were especially likely to employ dichotomous virtuality measurement. To avoid this confound, we repeated the analyses in Table 5 using only the classroom non-organizational studies. Results (available upon request) were unchanged. Overall, these results give credence to concerns related to representative design, or modeling only the extreme ends of the virtuality spectrum, though these concerns appear more relevant to some team effectiveness outcomes (productive, social, and individual outcomes) than to others (performance outcomes).

4.1 | Supplemental analyses

The literature defines virtuality as a multi-dimensional construct consisting of technology dependence and geographic dispersion; however, the relationship between these virtuality components remains an open question. To provide a preliminary estimate of how technology dependence and geographic dispersion correlate, we identified studies in our dataset where researchers measured both technology dependence and geographic dispersion; there were 12 such studies (13 independent samples), all involved *organizational* virtual teams. Technology dependence is measured either by degree of reliance on communication technology, or by its opposite—degree of reliance on face-to-face interaction [reverse-coded]. Geographic dispersion is measured either by physical distance between team members (e.g. in miles), or by the percent of team members located at the same versus at a different location. A bare-bones meta-analysis of the correlations between technology dependence and geographic dispersion in the 13 samples that measured both virtuality dimensions estimated $r_{\text{SAMPLE-SIZE WEIGHTED}} = 0.29$ ($\text{CI}_{95\%}$ [.26; .31]). This estimate shows that technology dependence and geographic dispersion are indeed positively correlated, suggesting that teams that rely on technology to communicate tend to be more geographically dispersed, and vice versa.

We also performed a series of additional analyses to explore moderators of the virtuality-team effectiveness relationship in *organizational* teams. First, in keeping with the supplemental analyses above, we explored the moderating role of virtuality operationalization (technology dependence versus geographic dispersion). As shown in Table 6, results revealed no differences across the technology dependence and geographic dispersion categories for three of the four broad team effectiveness outcomes: $Q_{B(\text{PRODUCTIVE})} = 0.27$, $p = .603$ ($\rho_{\text{TECH}} = 0.02$ vs. $\rho_{\text{GEO}} = 0.09$), $Q_{B(\text{SOCIAL})} = 1.05$, $p = .304$ ($\rho_{\text{TECH}} = 0.04$ vs. $\rho_{\text{GEO}} = -0.06$), and $Q_{B(\text{INDIVIDUAL})} = 0.27$, $p = .810$, ($\rho_{\text{TECH}} = -0.04$ vs. $\rho_{\text{GEO}} = -0.01$). However, effect sizes differed for performance outcomes: $Q_{B(\text{PERFORMANCE})} = 4.07$, $p = .044$ ($\rho_{\text{TECH}} = 0.08$ vs. $\rho_{\text{GEO}} = -0.04$). These results suggest that overall, technology dependence and geographic dispersion measures can be used interchangeably in organizational research, consistent with the accepted bi-dimensional definition of virtuality.

Second, because teams in different industries and companies may be subjected to differing expectations, provided with different resources, and generally subjected to different experiences (e.g. Carter et al., 2019), we explored the moderating role of two company characteristics: industry and company type. For industry, we coded studies into three categories: IT/telecomm, service, product; for company type, we coded studies into two categories: multinational and domestic. As reported in Table 6, neither of these two company characteristics moderated the virtuality-team effectiveness link for any of the four team effectiveness outcomes. Third, consistent with the literature on team composition (Mathieu et al., 2014; Stewart & Carter, 2018) which has identified an array of team member attributes that may affect team effectiveness

TABLE 6 Supplemental analyses: Moderators of the virtuality-team effectiveness relationship in organizational teams

Team effectiveness	*k*	*N*	ρ	$SE_{(\rho)}$	95% $CI_{(\rho)}$	*Z*	Q_B
Productive Outcomes							
Virtuality Operationalization							
Tech dependence	3	292	.02	.126	[−.23; .26]	0.13	
Geo dispersion	6	1996	.09	.081	[−.06; .25]	1.17	0.27
Company Characteristics							
Industry							
IT/Telecomm	2	88	.15	.194	[−.23; .53]	0.75	
Service	3	629	.02	.142	[−.26; .30]	0.15	
Product	3	935	.09	.139	[−.18; .36]	0.64	0.28
Company Type							
Multinational	3	306	.09	.136	[−.18; .35]	0.63	
Domestic	3	1230	.09	.119	[−.14; .33]	0.79	0.00
Team Characteristics							
Occupation							
IT/Engineering	3	127	.21	.129	[−.04; .46]	1.61	
R&D	1	–	–	–	–	–	
Consult/Mgmt/Sales	3	1347	.14	.095	[−.05; .32]	1.42	0.83
National Diversity							
Homogeneous	2	1129	.20	.142	[−.08; .48]	1.38	
Heterogeneous	2	88	.15	.179	[−.20; .50]	0.83	0.04
Gender Diversity[a]							
% Male	3	354	.01	.009	[−.01; .03]	0.79	0.63
Performance Outcomes							
Virtuality Operationalization							
Tech dependence	10	835	.08	.051	[−.02; .18]	1.65	
Geo dispersion	19	2564	−.04	.034	[−.11; .03]	−1.17	4.07*
Company Characteristics							
Industry							
IT/Telecomm	15	1165	−.02	.054	[−.13; .08]	−0.39	
Service	4	584	.07	.109	[−.14; .29]	0.67	
Product	9	928	.04	.066	[−.09; .17]	0.62	0.88
Company Type							
Multinational	24	1962	−.02	.028	[−.07; .04]	−0.64	
Domestic	6	1498	.03	.039	[−.05; .10]	0.65	0.82
Team Characteristics							
Occupation							
IT/Engineering	11	724	−.01	.063	[−.13; .11]	−0.14	

TABLE 6 (Continued)

Team effectiveness	*k*	*N*	ρ	$SE_{(\rho)}$	95% $CI_{(\rho)}$	*Z*	Q_B
R&D	7	490	−.00	.075	[−.15; .15]	−0.02	
Consult/Mgmt/Sales	8	1834	.03	.061	[−.09; .15]	0.54	0.26
National Diversity							
Homogeneous	8	1723	−.05	.063	[−.17; .08]	−0.72	
Heterogeneous	10	758	.00	.065	[−.13; .13]	0.03	0.27
Gender Diversity[a]							
% Male	20	1141	−.00	.003	[−.01; .00]	−0.99	0.97
Social Outcomes							
Virtuality Operationalization							
Tech dependence	4	140	.04	.097	[−.15; .24]	0.45	
Geo dispersion	9	1078	−.06	.042	[−.15; .02]	−1.53	1.05
Company Characteristics							
Industry							
IT/Telecomm	7	312	−.01	.067	[−.14; .12]	−0.14	
Service	2	115	.02	.110	[−.20; .23]	0.16	
Product	7	978	−.08	.051	[−.18; .03]	−1.50	0.99
Company type							
Multinational	13	600	−.09	.048	[−.18; .01]	−1.81	
Domestic	4	178	−.06	.086	[−.22; .11]	−0.66	0.10
Team characteristics							
Occupation							
IT/Engineering	8	355	−.06	.058	[−.17; .06]	−0.92	
R&D	6	1008	−.03	.032	[−.09; .04]	−0.79	
Consult/Mgmt/Sales	3	170	−.01	.085	[−.18; .15]	−0.17	0.22
National Diversity							
Homogeneous	3	150	−.01	.087	[−.18; .16]	−0.11	
Heterogeneous	8	336	−.07	.060	[−.19; .05]	−1.13	0.30
Gender Diversity[a]							
% Male	13	561	−.00	.003	[−.01; .00]	−1.35	1.81
Individual Team Member Outcomes							
Virtuality Operationalization							
Tech dependence	12	2216	−.04	.036	[−.11; .03]	−1.22	
Geo dispersion	5	835	−.01	.062	[−.13; .12]	−0.10	0.27
Company Characteristics							
Industry							
IT/Telecomm	2	227	−.04	.069	[−.17; .10]	−0.54	
Service	1	–	–	–	–	–	
Product	1	–	–	–	–	–	–

(Continues)

TABLE 6 (Continued)

Team effectiveness	*k*	*N*	ρ	$SE_{(\rho)}$	95% $CI_{(\rho)}$	*Z*	Q_B
Company Type							
Multinational	9	2247	−.00	.042	[−.08; .09]	0.95	
Domestic	4	604	−.05	.069	[−.19; .08]	0.44	0.40
Team characteristics							
Occupation							
IT/Engineering	4	430	−.08	.051	[−.18; .02]	0.09	
R&D	2	396	−.06	.056	[−.17; .06]	0.28	
Consult/Mgmt/Sales	1	–	–	–	–	–	0.78
National Diversity							
Homogeneous	1	–	–	–	–	–	
Heterogeneous	6	1118	.05	.032	[−.01; .11]	1.51	–
Gender Diversity[a]							
% Male	16	3498	.00	.002	[−.00; .01]	0.62	0.38

Abbreviations: *k*, number of effect sizes (independent samples); *N*, total number of teams (for the first three outcomes) or total number of team members (for the fourth outcome); SE_ρ, standard error of the meta-analytic effect size; *Z*, test of significance of the effect size from zero; Q_B, meta-ANOVA analogue to *F*, distributed as chi-square and evaluated on *df* = k where k is the number of categories; 95% $CI_{(\rho)}$, 95% confidence interval around the meta-analytic effect size; ρ, meta-analytic effect size: positive values indicate beneficial effects of virtuality, negative values indicate detrimental effects of virtuality.

[a]Because gender diversity is a continuous moderator (coded as percent men within teams), we performed meta-analytic regressions—that is, we regressed gender diversity on the effect sizes. Thus, for these analyses, we report the following: *k* number of effect sizes (independent samples); *N* = total number of teams (for the first three outcomes) or total number of team members (for the fourth outcome); *b* = unstandardized meta-regression coefficient; $SE_{(\beta)}$ = standard error of *b; Z* = test of significance of the effect size from zero; Q_R = meta-regression analogue to *F*, distributed as a chi-square, and evaluated on *df* = k where *k* is the number of regression coefficients.

*$p \leq .05$.

**$p \leq .01$.

metrics, we explored the moderating role of three team characteristics: occupation of team members, national diversity within teams, and gender diversity within teams. For occupation, we coded studies into three categories: IT/engineering, R&D, consulting/management/sales; for national diversity, we coded studies into two categories: homogeneous teams (i.e. all team members from the same nationality) and heterogeneous (team members from different nationalities); for gender diversity, we coded percent males (i.e. continuous moderator). As Table 6 shows, neither of these three team characteristics moderated the virtuality-team effectiveness link for any of the four team effectiveness outcomes in organizational teams.

These results suggest that the neutral relationship between virtuality and team effectiveness within *organizational* teams reported in our main analyses (Table 1) is robust to the effects of moderators. We note that information related to moderators was not consistently reported in primary studies, causing small sample sizes in some categories. We also note that the Q_w statistic (which tests for homogeneity of effect sizes; see Table 1), indicated that effect sizes within *organizational* teams were more homogeneous relative to effect sizes within *non-organizational* teams, suggesting that moderators may not play a large role within *organizational* teams. Altogether, our supplemental analyses, coupled with our main results, provide evidence on the differing nature of the relationship between virtuality and team effectiveness in *organizational*

versus *non-organizational* teams: neutral in the former, and subject to the effects of moderators in the latter, as expected.

5 | DISCUSSION

We had two overarching goals in this research – to shine light on the relationship between virtuality and team outcomes in *organizational* virtual teams, and to understand whether results from studies of *non-organizational* virtual teams generalize to *organizational* teams, and if so—under what circumstances. We found that in *organizational* teams, virtuality does not exert a direct relationship—positive or negative—on any of the four broad and nine specific team effectiveness outcomes we examined: (1) productive outcomes, including earnings, accuracy, and process improvements, (2) performance outcomes, including externally-rated team performance and team member-rated team performance, (3) social outcomes, including cohesion and team trust, and (4) individual team member outcomes, including project/task satisfaction and relational quality. We also found that results from *non-organizational* teams were significantly more negative than results from *organizational* teams. However, we showed that not all non-organizational studies fail to produce generalizable results; specifically, when non-organizational studies employ graduate student participants, create a longer team engagement, stimulate greater participant investment, and model virtuality more fully, they do produce results that generalize to organizational virtual teams. Finally, in supplemental analyses, we showed that the neutral relationship between virtuality and team effectiveness in *organizational* teams holds across a number of moderators. We discuss implications of these findings for theory, research, and practice below.

5.1 | Theoretical implications

The virtual teams literature has been largely guided by theories focused on deficiencies in virtuality's two core dimensions: technology dependence and geographic dispersion (Nurmi & Hinds, 2016a; Raghuram et al., 2019). Prominent virtuality theories often assume that technology dependence reduces the quality of information available to virtual team members (Daft & Lengel, 1984; Sproull & Kiesler, 1986), as well as that distance makes interactions less kind, less personal, and less collaborative (Kiesler & Cummings, 2002; Short et al., 1976). In fact, the expectation that virtuality should exert a negative impact on team outcomes is so strong that researchers often question the validity of their own findings if they do not conform to the predominant pessimistic view. For instance, when Mortensen and Hinds (2001a) did not observe a negative effect of virtuality on conflict, they asked: "The question remains, why did we not find what many scholars and theories have predicted?" (p. 229). Even in recent times, when virtual teams are not a novelty anymore, scholars continue to wonder at positive results. For example, Klitmøller and Lauring (2016) interpreted their finding that virtual team members are open to language diversity as "highly interesting because it indicates that temporal and spatial distance ... might not have only negative influences" (p. 282), and Schinoff et al. (2020) characterized research findings that virtual team members do develop positive interpersonal relationships as "curious" (p. 1396).

We believe that our first finding—that virtuality is not a direct determinant, negative or positive, of team effectiveness outcomes in *organizational* teams—is theoretically important

because it strengthens efforts to shutter the monopoly of the negative view on virtuality (e.g. Gibbs et al., 2015; Nurmi & Hinds, 2016a). In developing our arguments, we drew on the virtuality-as-paradox perspective (Cousins et al., 2007; Dubé & Robey, 2008; Gibbs, 2009; Purvanova & Kenda, 2018) which argues for a holistic, balanced view on virtuality, where scholars consider virtuality as a simultaneously negative and positive force whose opposing effects cancel each other out. Our results are consistent with the central prediction on this perspective of overall neutral effects, and we contend that balanced theoretical frameworks, such as virtuality-as-paradox, represent the future of virtuality theory. At the same time, we see a need for further development of this new line of theoretical work. Our research helps the literature take the first step towards re-theorizing virtuality as we offer a comprehensive empirical estimate of the relationship between virtuality and a broad set of team effectiveness outcomes in *organizational* teams. Future theory and research should engage in the next step, and theorize and empirically examine *how* teams navigate opposites (Gibbs et al., 2008; Purvanova & Kenda, 2018) to achieve balanced outcomes.

First, beyond positing a cancel-out mechanism to explain virtuality's overall neutral effects on team outcomes, the virtuality-as-paradox perspective should explain *how* teams deal with the dark and the bright sides of virtuality simultaneously. Paradox theory (Lewis, 2000; Smith & Lewis, 2011) describes a *both/and* response to paradox, where actors transcend contradiction by reconceptualizing opposites as complementary rather than competing. The virtuality-as-paradox perspective could unpack this idea and use it to explicate *how* virtual teams deal with the deindividuating, impersonal nature of virtual communication while capitalizing on its potential to remove bias and improve equality within teams. Similarly, *how* virtual teams deal with the isolating and detaching properties of spatial dispersion while capitalizing on its potential to increase autonomy should also be unpacked theoretically. Thus, theorizing *how* teams respond to paradox is an avenue for future growth of the virtuality-as-paradox perspective.

Second, building on the point above, such future theoretical efforts may benefit from the adoption of qualitative methodologies. In their seminal paper on paradox theory, Smith and Lewis (2011) argued that research questions related to direct and moderating effects (as in the present research) are best explored by quantitative methodologies, such as studying mean tendencies or comparing alternatives. In contrast, questions related to tensions and approaches to navigating tensions are best explored by qualitative methodologies, such as systemic, discursive, and contextual analysis. Coincidentally, qualitative methodologies are also superbly positioned for theory building (Feldman & Orlikowski, 2011; Shepherd & Suddaby, 2017), which is important as our results of no direct (negative or positive) effects of virtuality on team effectiveness suggest the need to re-think how we theorize the role of virtuality in teams. Our literature searches revealed a plethora of qualitative studies of virtual teams. Future theory-building attempts may begin by performing a qualitative meta-synthesis (Sandelowski et al., 2007)—a methodology designed to systematize qualitative data akin to the systematizing of quantitative data in meta-analysis (for an example, see Beigi & Shirmohammadi, 2017)—which may provide a useful foundation for theory building.

Third, the virtuality-as-paradox perspective may benefit from adopting a temporal lens and explicating the role *time* plays in dealing with paradoxical tensions. Extant theories suggest that time helps virtual teams overcome virtuality's challenges. For example, media naturalness theory (Kock, 2002) discusses a compensatory process which facilitates adaptation to lean media over time, and channel expansion theory (Carlson, 1995; Carlson & Zmud, 1999), along with adaptive structuration theory (DeSanctis & Poole, 1994), discuss that the use and reuse of technology leads to acquisition of knowledge and skills relevant to dealing with technology. The

virtuality-as-paradox perspective may build on the adaptation mechanisms suggested by these theories, and go above by explicating what teams actively do to adapt and to synergize virtuality's challenges and opportunities over time. In other words, the virtuality-as-paradox perspective should theorize the likely *both/and* responses to paradox by explaining what long-term strategies virtual teams develop (Zhang et al., 2015). The need to adopt a temporal lens is also consistent with long-standing concerns in the virtual teams literature that we lack knowledge regarding "when things happen" in virtual teams (Mitchell & James, 2001: 530; see also Hertel et al., 2005; Kirkman & Mathieu, 2005; Martins et al., 2004; McGrath & Tschan, 2007). Hence, another issue that future work on the virtuality-as-paradox perspective can theorize is how much time is required for virtual teams to see the benefits of synergizing competing demands.

It is important to point out that we do not discount the contributions of the vast number of theories that have informed the virtual teams literature over the decades. Though we agree with others that prior theories have been largely one-sided in their descriptions of virtuality (e.g. Gibbs & Boyraz, 2015; Nurmi & Hinds, 2016a; Raghuram et al., 2019), they have identified important challenges that virtuality poses. However, we do suggest that the role of virtuality's negative aspects may have been overplayed to the detriment of more balanced approaches. Our findings of neutral relationships between virtuality and team outcomes in *organizational* teams strongly suggest that it is time for virtuality theory to embrace a balanced view of virtuality. To paraphrase Gibbs et al. (2008: 207), virtuality theory should not ask *whether* virtual teams are successful (as most extant theories do), but rather, *how*—by what means and through which practices—they are able to be successful.

6 | METHODOLOGICAL IMPLICATIONS

In contrast to our finding that virtuality is not a direct determinant of team outcomes in *organizational* teams, we found significant negative effects of virtuality on team outcomes in *non-organizational* teams. These negative results are fully in line with prior meta-analyses, likely due to the fact that the vast majority of samples included in prior meta-analyses have been of *non-organizational* virtual teams. Hence, it is important to realize that the primacy of the negative view on virtuality has been maintained over the decades by findings from *non-organizational* virtual teams studies. Furthermore, we found that results from non-organizational studies are not only significantly negative, but that they are significantly more negative than findings from organizational studies of virtual teams. This represents evidence for poor results generalizability from non-organizational to organizational settings. However, we are not calling for the discontinuation of non-organizational virtual teams research because our moderator analyses showed that researchers can design studies in ways that increase results generalizability from non-organizational to organizational settings.

Specifically, we explored four moderators (i.e. participant type, team duration, study setting, and virtuality measurement) that have been discussed as likely causes of poor results generalizability in the virtual teams literature, but have not been systematically studied. Our results strongly supported the moderating role of these study design factors. We found that when studies use graduate students, create longer team engagements, more appropriately incentivize participants, and represent virtuality as a continuum, results from non-organizational studies do translate to organizational virtual teams. In contrast, when researchers enroll undergraduate students in studies with low realism where virtuality is dichotomized, results are likely to be

negatively biased. These findings have important implications for future virtual teams research because they demonstrate that when access to organizational virtual teams is not possible, designing a generalizable study in non-organizational settings is possible. These findings, however, should not be interpreted as an attack on *experimental* research. We say this because oftentimes, experiments are presumed to have low external validity, and therefore, findings showing poor generalizability (such as ours) are interpreted as validation of the limitations of experimental research. Yet, most of the studies in our dataset that generalized to organizational virtual teams were experiments (i.e. they manipulated the independent variable—virtuality—which is the key defining characteristics of experiments; Podsakoff & Podsakoff, 2019). Specifically, among the 22 studies of graduate students, 15 were experiments (68%); among the 45 medium-length studies, 34 were experiments (76%); among the 40 classroom studies, 31 were experiments (78%). Thus, experimental methodologies have a place in virtual teams research.

In supplemental analyses limited to *organizational* virtual teams only, we explored whether the two core operationalizations of virtuality, technology dependence and geographic dispersion, produce equivalent results, and found that these operationalizations can be used interchangeably. This is an important validation of the apparent truce that has been reached in the virtuality literature between those arguing that technology dependence is the true virtuality definition (Kirkman & Mathieu, 2005) and those arguing that geographic dispersion is (Foster et al., 2015). That is, accepting technology dependence and geographic dispersion as the two defining virtuality characteristics appears to have been justified (Dulebohn & Hoch, 2017; Gibbs et al., 2017). We also explored the moderating role of two company characteristics (industry and company type) and three team characteristics (occupation, national diversity, and gender diversity of team members), and found that the virtuality-team effectiveness relationship remains intact across these moderators in *organizational* teams. Pending additional research on this issue, these supplemental results suggest that researchers can assume that sampling virtual teams with different company or team characteristics would likely yield consistent results across *organizational* virtual teams.

Finally, though investigations of results generalizability from non-field to field settings often assume that field studies are a golden standard, this is just an assumption (Dipboye & Flanagan, 1979): non-field research suffers from threats to external validity, but field research suffers from threats to internal validity. For example, it is difficult to isolate the effects of virtuality from other potential influences on team effectiveness, to assure adequate participant engagement, and so on. This is another reason we believe that conducting virtual teams research in non-organizational settings (which allows researchers to retain some control), but employing study design features, such as graduate students, longer team lifespans, more engaging team projects (which create realistic contexts), and fuller-spectrum virtuality measurements, may represent the best of both worlds for future researchers.

6.1 | Applied implications

Our study represents the most comprehensive meta-analysis of the relationship between virtuality and a broad array of team outcomes (i.e. productive, performance, social, and individual) within *organizational* teams. Hence, our findings have a number of practical implications for virtual teams in work organizations. First, the finding that virtuality does not affect team outcomes either positively or negatively in organizational virtual teams suggests that supporters and opponents of virtual teams in work organizations are equally incorrect. For example, companies that have embraced virtual teams have argued that they are a means to increase

productive outcomes, improve efficiency, realize cost savings, assist with talent utilization, and deliver innovation (DeRosa, 2017; Hannah, 2019). Our results stop short of demonstrating a significant positive effect of virtuality on productive and performance outcomes. Work organizations that have rejected virtual teams (e.g. Best Buy, Bank of America, IBM, Wells Fargo, Zappos, Yahoo!, the U.S. Department of Agriculture, the U.S. Department of Education; Aratani, 2018; Lee, 2016; Simons, 2017; Wagner, 2018) have argued that they suffer from poor social processes, collaboration, and team climate (Spector, 2017; Swisher, 2013), and are detrimental to individual team member outcomes, such as motivation (Schulze & Krumm, 2017) and attachment (Kirkman et al., 2002; Sobel-Lojeski, 2015). Our results do not validate such concerns as they show no significant negative impact of virtuality on social processes or on individual team member outcomes.

Overall, our results suggest that organizations must be clear on what they can expect to gain from virtual teams. For example, virtuality might not increase productive and performance outcomes, but it still allows organizations to utilize dispersed talent or serve global clients. We advise organizations that if going virtual fits with strategic objectives or is necessitated by the external environment, they should adopt virtual teams. Indeed, the current COVID-19 health pandemic has led to an unprecedented increase in work-from-home – and, by extension, in virtual teams – around the globe. The reactions of business leaders reported in the popular press to the sudden mass-scale adoption of virtual work and virtual teams speaks volumes in support of our results. That is, business leaders have publicly expressed amazement that both productive and social outcomes have remained high despite the virtualization of work and specifically—of teams, much like scholars have marveled at unexpected positive results for virtual teams in academic research. For example, in the beginning months of the Covid-19 pandemic, Facebook CEO Mark Zuckerberg (2020) stated:

> For our existing employees, our guiding principle is figuring out what will enable us to serve our community best and unlock the most innovation. My own thinking on this has changed over the past few months. I've always felt that our teams worked better when they could physically be together. We even designed our offices so there are places where people can walk freely and have those serendipitous conversations that can lead to really good ideas. But I think that technology can help us overcome this, and the lockdown has shown us what's possible with the tools we have today.

Similarly, though Apple's CEO, Tim Cook, was apprehensive about virtuality's impact on team productivity and creativity in the initial phases of the COVID-19 work-from-home period, he expressed a newfound realization that virtuality does not have to be detrimental to teamwork in a *Forbes* interview: "[Cook] commented on the abilities of his team to create and build new Apple Watches and iPads while working remotely, and said that 'the company likely won't return to the way we were because we've found that there are some things that actually work really well virtually'" (Kelly, 2020).

We hope that our results of neutral relationships between virtuality and a host of team outcomes serve to end the debate on whether virtuality is bad or good for teams, and get scholars and practitioners alike to start focusing on when to deploy virtual teams and on how to ensure that they are maximally effective. Because the concept of paradox suggests the presence of both challenges and opportunities, thinking of virtuality as paradoxical might help organizations manage their approach to virtual teams.

6.2 | Limitations, strengths, and future research

Our meta-analysis is not without limitations. First, regarding our planned moderator analyses, our sample sizes in most moderator categories were quite large, but in our supplemental moderator analyses, some categories had a smaller number of primary studies. This may have decreased our ability to detect significant moderators of the virtuality-team effectiveness link in *organizational* teams. It is also possible that we did not detect significant moderators because there was not much variability in the *organizational* samples data as evidenced by low Q_w values that test for homogeneity of effect sizes (in Table 1). To more clearly understand whether the lack of significant moderators was due to low power (i.e. moderators are present but cannot be detected with small sample sizes) or to low homogeneity of effect sizes (i.e. moderators are not present), we call on researchers to describe their samples more fully so that future meta-analysts could verify our results.

Second, we corrected all outcomes for measurement unreliability (except for objective outcomes, such as earnings, where we assumed reliability to be 1). We did not use interrater reliability to correct team-level outcomes (e.g. social outcomes) even though rater-specific error variance is usually larger than item-specific error (De Jong et al., 2016). We used scale reliability rather than interrater reliability to correct effect sizes because, unfortunately, interrater reliability metrics are either not reported, or are differently calculated across studies (e.g. ICC, r_{wg} and etc.). Correcting for scale reliability, rather than interrater reliability, results in more conservative estimates because, as mentioned, scale reliability is not a very large source of error and thus, it does not result in large meta-analytic corrections. Hence, our results likely underestimate the true virtuality-team effectiveness relationship.

Third, we did not include some measures of team effectiveness, such as creativity, communication, knowledge sharing, and others, because fewer studies reported on such outcomes, especially in *organizational* research. However, relative to prior virtuality meta-analyses, our study included the broadest number of team effectiveness outcomes. Still, as data on additional team effectiveness metrics accumulates, future meta-analyses should expand the scope of outcomes examined. Additionally, we did not include virtual team members' job performance as an outcome because our work focused on team effectiveness factors. However, exploring the job performance of virtual employees (whether virtual team members or telecommuters) is a very relevant question due to the advent of virtual work prompted by the 2020 health pandemic. We call on future researchers to update Gajendran and Harrison's (2007) findings on the individual job performance of virtual workers.

Fourth, scholars have discussed productive, performance, social and individual outcomes as separate dimensions of team effectiveness (e.g. Hackman & Wageman, 2005), or as interconnected elements in the team input-process-output model (IPO; Ilgen et al., 2005). We treated outcomes as dimensions of team effectiveness because our primary interest was in understanding virtuality's direct impact on team effectiveness. However, applying an IPO framework to examine how virtuality affects relationships between processes (such as social outcomes) and outputs (such as productivity and performance), is a worthwhile endeavor for future research.

Fifth, we hailed researchers' efforts to study teams of longer duration and we also suggested that researchers should refrain from studying short duration teams on the grounds that results from short-length studies do not generalize to organizational virtual teams. This interpretation is well supported by the logic of ecological validity, as discussed in our hypotheses development section. However, all organizational virtual teams studies in our dataset were based on teams that had worked together for a significant amount of time (the average in our dataset was

2.25 years), which means that available field research is mute on the issue of how virtuality impacts teams at the beginning of their tenure. Hence, it is quite possible that results of short-length *non-organizational* studies may generalize to recently formed *organizational* virtual teams—a possibility our study could not address. We call for a significant uptick of research on organizational virtual teams in their nascency.

Sixth, our meta-analysis focused on estimating the direct relationship between virtuality and team effectiveness outcomes. However, even though virtuality is defined as a team input variable (Gilson et al., 2015, p. 1316), some prior meta-analyses have treated virtuality as a moderator of the relationship between other predictors and team effectiveness: that is, team design characteristics and team effectiveness (Carter et al., 2019), team trust and team effectiveness (Breuer et al., 2016; De Jong et al., 2016), and communication and team effectiveness (Marlow et al., 2018). This represents another important avenue for virtuality research, and we urge future meta-analysts to estimate virtuality's moderating effects for other relationships. As well, future research could explore non-linear relationships between virtuality and outcomes as it remains unclear whether hybrid teams (i.e. teams occupying the middle ranges of the virtuality spectrum) suffer or benefit from team hybridity (e.g. Fiol & O'Connor, 2005; Purvanova et al., 2021).

Finally, using citation analyses, Raghuram et al. (2019) identified different clusters of virtuality research, including a "virtual teams" cluster and a "computer-mediated work" cluster. Applying Raghuram et al.'s (2019) descriptions of these clusters, lab studies in our dataset likely belong to the "computer-mediated work" cluster, whereas organizational studies belong to the "virtual teams" cluster. Hence, the poor results generalizability from lab to organizational settings may be due to sampling studies from two different clusters of virtuality research. Whereas this may be a viable explanation for our results, our moderator analyses suggest that there is more to this story than just sampling from two clusters of virtuality research. For example, results of longer-duration lab studies do generalize, and so do results of graduate students lab studies. Thus, it is possible that different clusters of virtuality research do exist, but that study design features provide bridges across clusters.

The strengths of our meta-analysis may offset some of the limitations above. To begin, we meta-analyzed data from 73 *organizational* samples and 109 *non-organizational* samples (57 lab and 52 classroom samples). Whereas prior meta-analyses have included mostly lab samples, organizational and classroom samples have remained a largely untapped source of data. By meta-analyzing this new research, we provide the most comprehensive quantitative accounting of the effects of virtuality to-date. Second, we estimated results for productive, performance, social, and individual team effectiveness outcomes, providing a previously unavailable full-range view of how virtuality influences different aspects of team effectiveness. Third, our results from *non-organizational* teams are consistent with prior meta-analyses (i.e. finding a significant negative association between virtuality and team effectiveness), and our results in *organizational* teams are consistent with a recent meta-analysis of 19 organizational samples (i.e. finding a neutral association; Carter et al., 2019). The validity of our findings is further bolstered by large fail-safe *k*'s (i.e. we would have needed to include anywhere from 66 to 146 additional studies, depending on the specific outcome, to invalidate our results).

Fourth, meta-analyses typically perform a large number of tests, which may elevate family-wise error rates (Polanin & Pigott, 2015). However, our procedures are consistent with recommendations to decrease this potential issue (see Pigott & Polanin, 2020): We report exact *p* values (in Results), provide confidence intervals (in all tables), utilize a small number of planned comparisons (four planned moderators), and conduct moderator analyses on broad

versus specific outcomes (e.g. productive outcomes versus earnings, accuracy, and process improvements separately). Fifth, our planned moderator analyses helped reveal that *non-organizational* virtual teams research is often unfairly criticized (e.g. while studies using undergraduate students may be problematic, studies using graduate students are not; etc.). Thus, an important contribution of our meta-analysis is to temper criticism of *non-organizational* virtual teams research by asking critics to be more nuanced.

Sixth, meta-analyses are well-positioned to establish whether or not two variables (such as virtuality and team effectiveness) are related. Our approach to establishing a neutral relationship between virtuality and team effectiveness in *organizational* teams is consistent with Edwards and Berry's (2010) recommendations to specify null hypotheses based on conceptual reasoning, and to then increase theoretical precision by comparing null to non-null hypotheses. Accordingly, we leaned on the virtuality-as-paradox perspective for conceptual guidance, and we then compared effects derived from *organizational* samples to effects from *non-organizational* samples. Additionally, we conducted planned and supplemental moderator analyses to further document the robustness of our main results. Our approach gives confidence that the link between virtuality and team effectiveness in *organizational* teams is neutral, whereas it is negative in *non-organizational* teams, as expected.

In terms of future research, a reviewer noted that our dataset is based on studies conducted prior to the COVID-19 health pandemic, when only highly conscientious and high-performing employees were chosen for virtual team assignments (Boh et al., 2007a; Hertel et al., 2006). Typical virtual team members were also tech savvy and tech adaptable (Charlier et al., 2016) and received special training prior to commencing their virtual experience (Wright, 2015). In contrast, due to the sudden onset of the global health crisis, employees are asked to team virtually regardless of whether or not they possess the characteristics of successful virtual team members (outlined fully in Schulze & Krumm, 2017). Moreover, virtual team leaders—who are instrumental to team success especially at higher levels of virtuality (Gilson et al., 2015)—are no longer specifically screened or trained for virtual leadership. Future research could explore on how virtuality impacts team success when workers and leaders who may not necessarily be a match for virtual work are nevertheless asked to work and to team virtually. This is an exciting conjecture for future virtuality research to evaluate, and, as our reviewer noted, may make the virtuality-as-paradox perspective we adopted even more relevant in the post-COVID workplace.

6.3 | Conclusion

Our core motivation in this research was to clarify how virtuality influences team effectiveness in work organizations—an issue which after decades of research and a number of prior qualitative and quantitative reviews still remains unclear. Using 73 independent samples of *organizational* virtual teams, we found the relationship between virtuality and team effectiveness to be neutral, in contrast to the predominant negative view of virtuality, and in support of a more balanced view. Our secondary motivation was to estimate the generalizability of findings from *non-organizational* to *organizational* virtual teams. Using data from 109 independent samples of *non-organizational* virtual teams, we first showed that virtuality exerts a significant negative effect on team outcomes in non-organizational settings. Moreover, we showed that results from non-organizational settings often fail to generalize to organizational virtual teams. In moderator analyses, we helped identify study features which increase results generalizability, helping to

assure researchers that avenues for virtual teams research outside of work organizations are available. From an applied perspective, our meta-analysis sheds light on the current state of uncertainty in the world of practice regarding the value of virtual teams. Because virtuality is not a significant determinant—negative or positive—of team effectiveness, asking teams to work under virtual conditions should be mostly a matter of strategic organizational objectives, not of idiosyncratic opinions about the assumed negative effects of virtuality, or, conversely, of an overly enthusiastic embrace of technology-enabled, dispersed forms of work.

CONFLICT OF INTEREST

The authors declare that there is no conflict of interest that could be perceived as prejudicing the impartiality of the research reported.

DATA AVAILABILITY STATEMENT

The data that support the findings of this study are available from the corresponding author upon reasonable request.

ORCID

Radostina K. Purvanova https://orcid.org/0000-0002-6161-4300
Renata Kenda https://orcid.org/0000-0003-0606-1739

ENDNOTES

[1] We are aware of the following prior meta-analyses exploring the direct effects of virtuality on various team effectiveness outcomes (in order of publication date): McLeod (1992), Benbasat and Lim (1993), Walther et al. (1994), Dennis et al. (2001), Baltes et al. (2002), Dennis and Wixom (2002), Fjermestad (2004), Rains (2005), Lim et al. (2007), Mesmer-Magnus et al. (2011), and Ortiz de Guinea et al. (2012). Only two of these meta-analyses included some studies of organizational virtual teams: 10% in Mesmer-Magnus et al. (k = 8 of 79), and 24% in Ortiz de Guinea et al. (k = 16 of 66). Studies of *organizational* virtual teams constituted 0–2% of the datasets of the other virtual teams meta-analyses listed above. Additionally, Carter et al.'s (2019) meta-analysis on the effects of team design characteristics on team performance, which included 428 independent samples, treated virtuality as a team design characteristic and reported estimates on the virtuality-team performance relationship in 19 *organizational* virtual teams samples.

[2] For an illustration of various virtuality paradoxes that have been described in the literature, see Dubé and Robey (2008), Gibbs (2009), or Purvanova and Kenda (2018).

[3] The 19 qualitative studies we reviewed are (in order of publication date): Bell and Kozlowski (2002), Martins et al. (2004), Powell et al. (2004), Hertel et al. (2005), Webster and Staples (2006), Connaughton and Shuffler (2007), Gibbs et al. (2008), Wilson et al. (2008), Dixon and Panteli (2010), Martins and Schilpzand (2011), Walther (2011), Kirkman et al. (2012), Gibson et al. (2014), Foster et al. (2015), Gibbs and Boyraz (2015), Gilson et al. (2015), Gibbs et al. (2017), Schaubroeck and Yu (2017), and Raghuram et al. (2019). The 11 quantitative studies we reviewed are the prior virtuality-team effectiveness meta-analyses listed in footnote 1.

[4] To understand why we excluded studies along this fourth criterion, consider the following two examples: Example 1: Xue et al. (2005) assigned 64 students to eight face-to-face and eight virtual teams; upon project completion, team members reported on team cohesion (among others). Rather than aggregating reports of team cohesion to the team level ($N = 16$), researchers analyzed cohesion as an individual-level outcome ($N = 64$). This inappropriately inflates the sample size from 16 to 64. Therefore, including this study in our meta-analysis for cohesion would unfairly overweigh this study relative to other studies of team cohesion that appropriately reported results at the team level. Specifically, the cohesion effect size from this study would be weighed by a sample size of 64—rather than a sample size of 16—hence artificially and unfairly increasing this study's contribution to the calculation of the overall effect size. Example 2: Staples and Zhao (2006) assigned 380 students to 40 face-to-face and 39 virtual teams; upon project completion, team members reported on their

satisfaction with the project experience (among others). Rather than analyzing satisfaction results at the individual level ($N = 320$), researchers analyzed satisfaction as a team-level outcome ($N = 79$). This inappropriately deflates the sample size from 320 to 79. Therefore, including this study in our meta-analysis for individual team member outcomes would unfairly under-weigh this study relative to other studies of team member satisfaction that appropriately reported results at the individual-level. Specifically, the satisfaction effect size from this study would be weighed by a sample size of 79—rather than a sample size of 320—hence artificially and unfairly diminishing this study's contribution to the calculation of the overall effect size.

[5] The Q_B's for three of the nine specific outcomes were significant: $Q_{B(\text{EARNINGS})} = 5.54$ ($p = .019$), $Q_{B(\text{PROCESS IMPROVEMENTS})} = 8.00$ ($p = .005$), and $Q_{B(\text{SATISFACTION})} = 3.83$ ($p = .050$). The contrast tests were not significant for the remaining six specific outcomes: $Q_{B(\text{ACCURACY})} = 1.65$ ($p = .199$), $Q_{B(\text{OTHER-RATED PERFORMANCE})} = 0.05$ ($p = .821$), $Q_{B(\text{TEAM MEMBER-RATED PERFORMANCE})} = 0.98$ ($p = .323$), $Q_{B(\text{COHESION})} = 3.03$ ($p = .082$), $Q_{B(\text{TRUST})} = 1.86$ ($p = .172$), and $Q_{B(\text{RELATIONAL QUALITY})} = 2.95$ ($p = .086$). We do not show results for the specific team effectiveness outcomes in the remaining tables to conserve space. These results—which are highly consistent with results for the four broad outcomes—are available upon request from the first author.

[6] References for all studies included in the meta-analysis are found in the next section.

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SUPPORTING INFORMATION

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APPENDIX A

Operationalizations of Team Effectiveness Outcomes in Primary Studies

Team effectiveness	Organizational samples	Non-organizational samples
Productive outcomes		
Earnings	Net earnings Reaching financial targets Total sales	Negotiation pay-off Joint profit score Total projected sales
Accuracy	% defects (r.s.) Sum of errors (r.s.)	Correct solution Deviation from correct answer/expert solution (r.s.) Number of errors (r.s.)
Gains and improvements	% goal accomplishment Speed of product development Speed of client problem resolution On-time completion rate	% improvement of team relative to members Gains over best member Gains over average team solution
Performance outcomes		
Other-rated	Assessments by external clients, internal clients, external leaders, or team manager.	Team project assessed by experts, course instructor, or teaching assistants.
Team member-rated	Team member assessment of product or of process.	Team member assessment of product or of process.
Social outcomes		
Cohesion	Ratings of team cohesion, team unity, team identification, or team viability.	Ratings of team cohesion, team unity, team identification, or team viability.
Trust	Ratings of trust within team, psychological safety, or ability to be vulnerable with others.	Ratings of trust within team, psychological safety, or ability to be vulnerable with others.
Team member outcomes		
Project/task satisfaction	Satisfaction with team process or with team outcomes.	Satisfaction with team process or with team outcomes.
Relational quality	Connectedness to teammates Ratings of relationship quality Liking of teammates	Connectedness to teammates Ratings of relationship quality Liking of teammates

APPENDIX B

Characteristics of Samples Included in Meta-Analysis

	Productive outcomes			Performance outcomes			Social outcomes			Team member outcomes		
	Non-Org.		Org.	Non-Org.		Org.	Non-Org.		Org.	Non-Org.		Org.
Sample Characteristics	**Lab**	**Class**	**Org.**	**Lab**	**Class**	**Org.**	**Lab**	**Class**	**Org.**	**Lab**	**Class**	**Org.**
Participant type												
Undergraduate students	31	3	0	22	14	0	20	11	0	14	10	0
Graduate students or mix	6	7	0	4	7	0	4	4	0	2	2	0
Employees	0	0	9	0	0	40	0	0	24	0	0	21
Team duration												
Short (M = 60 min)	33	3	0	19	2	0	15	3	0	16	2	0
Medium (M = 25 days)	4	7	0	7	19	1	9	12	0	0	10	0
Long (M = 2.25 years)	0	0	9	0	0	39	0	0	24	0	0	21
Virtuality measurement												
Dichotomous measurement	32	8	4	24	14	2	23	10	2	16	11	4
Continuous measurement	5	2	5	2	7	38	1	5	22	0	1	17
Year of publication												
2000 or earlier	13	4	0	6	7	0	6	4	0	6	3	0
2001–2009	16	6	4	15	8	12	13	7	9	10	6	8
2010 or later	8	0	5	5	6	28	5	4	15	0	3	13

Note: Number of samples (*k*'s) within each of the four team effectiveness metrics featuring certain study characteristics.