

Seelos, Christian; Mair, Johanna; Traeger, Charlotte

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The future of grand challenges research: Retiring a hopeful concept and endorsing research principles

Christian Seelos¹ | Johanna Mair^{2,3}  | Charlotte Traeger^{4,5}

¹Stanford University Center on Philanthropy and Civil Society, Stanford, California, USA

²Strategy and Leadership, Hertie School, Berlin, Germany

³Stanford University Center on Philanthropy and Civil Society, Stanford, California, USA

⁴Hertie School, Berlin, Germany

⁵Center for Public Management (KPM), University of Bern, Bern, Switzerland

Correspondence

Johanna Mair, Strategy and Leadership, Hertie School, Friedrichstrasse 180, 10117 Berlin, Germany.

Email: mair@hertie-school.org

Abstract

Editorial notes in leading management journals have urged scholars to address Grand Challenges (GC) as an opportunity for producing knowledge that matters for society. This review explores whether current conceptualizations of GC support a productive path for management and organizational scholarship by guiding empirical inquiry, facilitating cumulative theory development, and informing practice. We systematically examine scholarly articles, calls for papers, and editorial notes published in management journals for consistency in how researchers use and define the concept of GC and the scope of associated phenomena and attributes. We find three prominent conceptual architectures in use: discursive, family resemblance, and phenomenon driven. The variety and incoherence of current uses of the GC concept and the lack of efforts to improve its analytical competence lead us to suggest its retirement. Instead, we propose building on the enthusiasm around GC research and using GC as a term to define research principles that collectively help align research efforts and improve theoretical development and practice. The principles we propose capture a genuine origin story for management research on GC.

‘There is no longer room for irrelevant things. We have no longer got the resources. Irrelevance is out’. — Dieter Rams¹

INTRODUCTION

We evaluate research by management scholars on Grand Challenges (GC). Calls for special issues (Academy of Management Journal, 2014; Howard-Grenville et al.,

2017a; Kunisch et al., 2021; Voegtlin et al., 2019) promoted research on GC as an opportunity to (re)direct attention towards phenomena of societal concern. This plea extends earlier proposals to ground organizational research in the observation of phenomena relevant to society at large (Walsh et al., 2003) and to instil a sense of responsibility in management research and education (Howard-Grenville et al., 2019; Tsui, 2021). We welcome the (renewed) attention to challenges of public concern but wonder whether our research efforts result in progressive theory development able to explain complex phenomena and inform practice.

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¹Quote at the permanent Dieter Rams exhibition at the Museum of Modern Art, San Francisco.

Articles published in highly ranked management journals between 2011 and 2021 have referred to GC as a concept that either is clearly defined (Ferraro et al., 2015) or lacks any set of coherent attributes (Brammer et al., 2019). Some authors have used the term to describe the empirical context of studies (Berrone et al., 2016; Grodal & O'Mahony, 2017; Reinecke & Ansari, 2016) or to denote a specific type of managerial challenge (Etzion et al., 2017; George et al., 2016; Howard-Grenville et al., 2017b). Others have used the concept to refer to an assumed gap between theory and practice (Banks et al., 2016; North, 2019), while still others have referred to research on GC as a broad discourse (Kaldewey, 2018; Howard-Grenville, 2021).

Scholars also differ in their assessment of the epistemic adequacy of current theories and methods in use. Garud and Gehmann (2016) and Touboulic and McCarthy (2020) argued that GC research requires an epistemic shift from the dominant positivistic paradigm in management research. In contrast, Eisenhardt et al.'s (2016) found that existing research paradigms and methods are 'particularly able to address these substantial [GC] problems' (p. 1115). However, side-lining epistemic reflection on what studying complex phenomena entails risks ignoring decades of critical voices and proposals for improving management research. Munir (2020), for example, was concerned that current theorizing in organizational research often relies on 'uncritical, sanitized and dangerously misleading simplification of messy, complex social phenomena' (p. 1). Davis (2015) voiced concerns about limited reflection on what and for whom management research is, and insufficient cumulative effort to build on or replicate prior findings.

Even a superficial engagement with the first decade of management scholarship on GC leaves readers puzzled as to how to go about GC research or what it seeks to explain. Because improving conceptual clarity is necessary for improving theory development (Suddaby, 2014), our focus in this review is on GC as a concept. We systematically and critically interrogate the current usage of the concept and its ability to support theory development and practical usefulness. Our objective is to assess the promise and hope surrounding the concept to reimagine future research on GC.

In the next section, we contextualize research on GC in management. We revisit the origin story of management GC research, motives to adopt GC research in other disciplines, and related earlier efforts in system science that failed to accumulate knowledge helpful and hopeful for tackling societal challenges. We then introduce our analytical approach to assess the literature. After describing our methods and sampling, we present our findings in three steps: first, we illustrate the broad range of empirical referents, the phenomena that authors associated with

GC; second, we expose incommensurable ambiguities in conceptual attributes of GC; and third, we discuss three conceptual architectures that differ in the constellation of terms, referred phenomena, and analytically important attributes. We reflect on the implications of current conceptualizations of GC for theory development and practice, and we offer ideas for strengthening the GC concept. For the future of GC research, we propose retiring the use of GC as a concept and adopting instead a progressive GC research agenda based on research principles.

CONCEPTUAL FOUNDATIONS

Contextualizing GC research

To provide context and motivate our review focus, we briefly reflect on GC's origin narratives in management research, revisit motives for engaging in GC research in other disciplines and distil lessons from research on system perspectives.

Origin narratives that affect consistency and coherence

Examining origin narratives helps trace the evolution in thinking about GC and understand how the (in)consistency between initial motivations, assumptions about concepts, and empirical foci affects progress in management research on GC. Hilbert's (1902) formulation of 23 mathematical problems by which he challenged colleagues at the Second International Congress of Mathematicians is frequently cited as the origin story of GC (Brammer et al., 2019; Colquitt & George, 2011; George, 2016; Howard-Grenville, 2021). Before finding their way into management research, Hilbert's problems inspired the Bill and Melinda Gates Foundation: 'On 26 January 2003, at the World Economic Forum in Davos, Switzerland, Bill Gates announced a \$200-million medical research initiative – the Grand Challenges in Global Health – based on a century-old model, the GC formulated by the mathematician David Hilbert' (Varmus et al., 2003, p. 398). Interestingly, Hilbert did not use the term GC. Also, he was less concerned about society's challenges than about the fate of mathematics as a scientific discipline, holding that the ability to formulate an abundance of problems is a sign that a discipline is alive. Considering the bewildering set of social and environmental problems and other phenomena of societal concern that management research now associates with GC, formulating an abundance of problems seems to be the only legacy of this origin story. We see holding onto this origin story as

problematic because studying random problems might impede the consistency, coherence, and conceptual clarity that are fundamental to theory development (Gerring, 1999; Suddaby, 2014). Identifying an adequate origin story and articulating how it matters for GC research is important to prevent language games, for when such games are purely based on the use of the term GC they risk obstructing progress in management and organization research (Astley & Zammuto, 1992; Clegg, 2002).

Motives for adopting GC in other scientific disciplines

In comparison with other disciplines, management research has adopted the term GC relatively late. Understanding scientific disciplines' implied motives for adopting the term GC provides a useful context for reflecting on management research on GC. Based on a non-systematic analysis of how the term GC is used in other disciplines, we identified three types of adoption motives. Firstly, authors strategically used GC as a theme to define a discipline's research priorities to build momentum around topics considered to generate the highest scientific, social, or commercial payoffs. Examples are Haux (1997) in medical informatics or Simons et al. (1991) in computing research. Secondly, authors symbolically adopted the term GC to rejuvenate scholarship and the reputation of scientific disciplines by associating research efforts with a larger noble idea. Examples can be found in chemistry (Lippard, 2000) or in arguments by Gilbert Omenn, President of the American Association for the Advancement of Science (Omenn, 2006). Lastly, authors adopted GC language opportunistically and instrumentally to access resources and funding. Examples include Fogarty (1994) in geographic information systems or Reddy (1995) in Artificial Intelligence. Mertens and Barbian (2015) saw adoption of the term GC as a silver bullet, serving a variety of scholarly and political objectives, for the field of Business and Information Systems Engineering. Clearly, the use of GC in scientific disciplines serves multiple purposes that are not necessarily aligned with ambitions for theory development or generating practical knowledge around challenges of societal concern.

System scholarship

GC-centric research is not the first attempt to direct scholarly attention towards solving society's grand problems. In the early 20th century, disciplines ranging from biology to psychology and sociology advocated for the adoption of system perspectives as a major reorientation of scien-

tific thought (Lilienfeld, 1978; von Bertalanffy, 1969). Ashby (1957) projected the hope that system perspectives provided society 'the essential methods by which to attack the ills – psychological, social, economic – which at present are defeating us by their intrinsic complexity' (p. 6). The promise of system scholarship and its prominent successes in technical engineering offered tremendous inspiration to policymakers. In 1966, Senator Gaylord Nelson chaired US Senate hearings on a bill to 'mobilize and utilize scientific manpower of the Nation to employ systems analysis and systems engineering' (Scientific Manpower Utilization Act, 1966). Nelson introduced his vision of 'A Space Age Trajectory to The Great Society' based on the logic that system scientists who can put a man on the moon should be able to also tackle societal challenges on earth. Within a few years, system analysis contracts had created a multi-billion-dollar industry that was 'exploited by a myriad of vested business and professional interests and fostered by government officials eager to be identified with and to take advantage of advanced concepts of management science' (Hoos, 1972, p. 5). Unfortunately, research on system perspectives evolved in disciplinary siloes that never agreed on a shared and consistent use of the term. Some research efforts employed the term system as an epistemic reference to cognition (e.g., system thinking, system perspectives) or theorizing about the world (e.g., complexity theory and various modelling approaches). For others, the term system captured ontological assumptions about the existence of systems in the real world. This fragmentation of perspectives left researchers and practitioners confused about the validity and practical utility of system research (Castellani & Hafferty, 2009; Jackson, 2000; Lilienfeld, 1978; Rousseau et al., 2016; Troncale, 1985).

The trajectory of system scholarship highlights the importance of adequately defining central concepts, reminding us to be explicit about the boundaries of knowledge claims, as their applicability has important consequences for policy and practice. Commenting on the policy adoption of system perspectives during the 1960s, Von Bertalanffy (1969) warned that the term system risked becoming a 'fashionable catchword'. He called system practitioners the "'new utopians" of our time ... at work creating a "New World", brave or otherwise' (p. 3). Merely adopting the term GC did little to make Gates's work more effective as a new utopian of our time. At the tenth anniversary of the launch of Gates's GC in Health effort, observers criticized the initiative for not having achieved any of its ambitious goals and for having significantly scaled back its funding (Doughton, 2014). Gates told a gathering of nearly a thousand people that 'I was pretty naive about how long that process would take' (Doughton, 2014).

Contextualizing GC research in management as to the origin story in use, to motives for adopting the term in

other scientific disciplines, and to lessons from scholarship on systems alerted us to the importance of countering tendencies to adopt the term GC for political reasons and the need to strive for concept clarity and practical usefulness. Accordingly, we examine the GC management research with an eye to how the use and characteristics of GC facilitate theoretical development and improve the practical relevance of knowledge claims.

Analysing GC research

In their influential editorial note, Colquitt and George (2011) used the term GC in reference to topics that appeal to reviewers and readers of the *Academy of Management Journal*, noting that addressing GC might positively influence the review process of submitted articles. However, they failed to provide concrete empirical referents or conceptual attributes associated with GC. Instead, they left it to the community of management scholars to develop GC research into a perspective of confronting topics ‘in a bold and unconventional way’ (p. 432) based on GC as an inspirational term, or to develop GC as a scientific concept aimed at theory development. Since then, management researchers have used the term GC as a concept by designating concrete phenomena and proposing concrete attributes (Gerring, 1999). However, we found no consensus about the role of the GC concept for developing new or advancing existing theory.

To systematically assess the literature, we use an analytical scaffolding that foregrounds the link among concepts, theory development, and the ability to inform policy and practice. We pay particular attention to how a term such as GC relates to phenomena and conceptual attributes.

Analytical scaffolding to assess conceptual architectures

The scaffolding we propose for evaluating GC research integrates criteria that scholars have identified as principal determinants of the quality and usefulness of scientific concepts (Collier & Mahon, 1993; Gerring, 1999; Goertz, 2020; Sartori, 1984; Suddaby, 2010). Central to this scaffolding is Gerring’s (1999) tripartite structure of concepts, which specifies relations between (a) a central term such as GC, (b) the phenomena this term refers to, and (c) the set of attributes that define and distinguish these focal phenomena from others. We evaluate the competence of this tripartite structure in terms of its ability to support theory development and to inform practice.

Concepts and theoretical claims about referred phenomena are the bread and butter of a progressive sci-

ence within a community of scholars (Ermakoff, 2015). Attributes or characteristics of phenomena are analytically important elements of theory development. Theory provides ‘conceptual order on the empirical complexity of the phenomenal world [and therefore] knowledge accumulation simply cannot occur without a conceptual framework’ (Suddaby, 2014, pp. 407–408). Weakly defined and ambiguous concepts impede conceptual goodness and hamper the translation of science into practice (Suddaby, 2014).

As an example of this scaffolding, consider the concept Quality of Life (QoL) that, like GC, confers an intuitive meaning. Goertz (2020) defined QoL in terms of eight attributes that include health, personal security, and work–life balance. These attributes are not merely descriptive; rather, they constitute causal mechanisms and processes that generate aspects of QoL. Variations in health are not just associated with but generate variations of QoL. The attributes are linked by an AND operator: QoL depends on having at least average levels of health, personal security, and work–life balance. QoL and its attributes can be measured, and we can establish variance in QoL to test and refine theories and to update the concept’s attributes by integrating additional attributes or changing operators from AND to OR. This conceptualization enables us to address practical questions such as: Does an intervention increase QoL?

Clarifying the concept GC will require an ongoing evaluation about descriptive attributes that help differentiate GC phenomena from others and identifying attributes that reflect the causal mechanisms that play analytically important roles in GC phenomena. For example, a GC attribute such as complexity, proposed by Ferraro et al. (2015), may serve neither function. Complexity is itself a concept that is difficult to operationalize. It may not be analytically useful to differentiate between social phenomena that have ‘sufficient’ levels of complexity to be members of the concept GC and phenomena that are insufficiently complex. We may also struggle to causally link variations in complexity with variations in other GC dimensions. Defining a broad concept such as GC requires ongoing effort because the useful set of attributes needs to be discovered and refined over time. Conceptual clarity is not a prerequisite for theory development; concept- and theory-development go hand in hand (Gerring, 1999; Goertz, 2020). The analytical question guiding our review, therefore, is: Are there signs in the literature that indicate if researchers are invested in improving the clarity and theoretical competence of GC as a concept?

Figure 1 summarizes our analytical scaffolding. We refer to a conceptual architecture as a constellation of terms, empirical referents, and attributes, and the implications of this constellation for theory development and for practice.

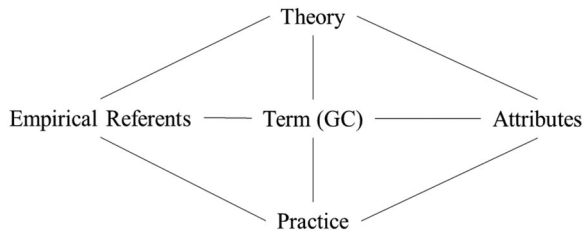


FIGURE 1 Conceptual architecture of GC and its constituent elements

METHODS

This review evaluates the progress of GC research published in management journals over the course of 10 years. To assess progress, we move beyond the mechanical elements often found in meta-analysis-based reviews (Aguinis et al., 2011) or the integrative approach applied to reviews of literatures built over several decades and/or spanning research areas (Cronin & George, 2020). Instead, we rely on reflexivity and the willingness to challenge conventions (Alvesson & Sandberg, 2020) to specify indicators of and hurdles to progress of a research effort. The specific purpose of this review is an assessment of theory development and the competence of GC research to accumulate knowledge that is effective for addressing societal challenges.

Sample selection

At the core of our review is management research on the concepts ‘grand challenge’ or ‘grand societal challenge’, often used interchangeably (Flink & Kaldewey, 2018). We combined different sources of data in the form of articles, editorial letters, and calls for research on GC and followed a three-step process. In a first step, in March 2020, we conducted a search in ten leading management journals, namely *Academy of Management Journal (AMJ)*, *Academy of Management Review (AMR)*, *Academy of Management Annals (AMA)*, *Academy of Management Perspectives (AMP)*, *Academy of Management Discoveries (AMD)*, *Organization Studies (OST)*, *Organizational Science (OS)*, *Management Science (MS)*, *Journal of Management (JOM)*, and *Journal of Management Studies (JMS)*. According to the Academic Journal Guide of 2018 by chartered ABS and the ABDC 2019 ranking, these journals are the highest ranked (ABS ranking: *AMR* 4*, *AMJ* 4*, *AMA* 4, *AMP* 3, *OS* 4*, *OST* 4, *MS* 4*, *JOM* 4*, *JMS* 4; ABDC ranking: *AMJ* A*, *AMR* A*, *AMA* A*, *AMP* A, *AMD* A, *MS* A*, *JOM* A*, *JMS* A*). This search identified 95 articles. For this and all subsequent searches, we read title, abstract and keywords,

and parts of the main text to identify the article’s fit with the objective of our review.

In a second step, in April 2020, we extended this search by including EBSCO Business Source Premier. We used the search boundaries of ‘Boolean operator’ and examined articles published by the Academy of Management, Elsevier, Emerald Publishing, John Wiley & Sons, Sage Publications, Springer Nature, Taylor & Francis, and Wiley-Blackwell, using the filters ‘Academic journals’, ‘Peer reviewed’, ‘English language’, and ‘2011–2019’. This search resulted in a total of 127 articles. We also searched for calls for articles in special issues related to GC in management journals, identifying nine calls issued in the *AMJ*, *AMD*, *OST*, *JMS*, *Journal of Product Innovation Management*, *International Entrepreneurship and Management Journal*, *Business Ethics: A European Review*, *Australian Journal of Management*, and *International Journal of Management Review*. We analysed the content of these calls for how editors framed GC research and what examples and definitions of GC they provided.

In a third step, in January 2021, we conducted an additional EBSCO search that focused on the time-period of 2020–2021, using the same search boundaries as in our April 2020 search. This search resulted in 61 articles. We deliberately opted for a stepwise and time-layered approach to collect and review articles on GC for the following reasons: first, we wanted to examine the relationship between articles, editorial notes, and calls for special issues. Second, we wanted to capture the emerging direction of GC research in its early phase of development. Third, we wanted to account for our own potential biases as reviewers by engaging in three – temporally separate – rounds of analysis and reflection. We excluded articles from non-management journals to focus on the use of the GC concept in management research. In addition, we excluded articles that mentioned GC only in the abstract, references, or keywords (i.e., not in the body). We excluded conference proceedings and working papers that were not peer reviewed. Based on this stepwise process, we identified a total of 107 articles, including 15 editorial notes, as well as nine calls for papers. Figure 2 summarizes our sample selection process.

Sample description

Of the articles from 2011 to 2021, 47% are conceptual and 53% are empirical studies. Interestingly, the relationship between conceptual and empirical work on GC changed in 2020–2021. In that period, 65% of articles were conceptual and 35% were empirical; we also observed a relative decrease in the number of editorial notes. Our sample includes articles published in 38 different journals, with

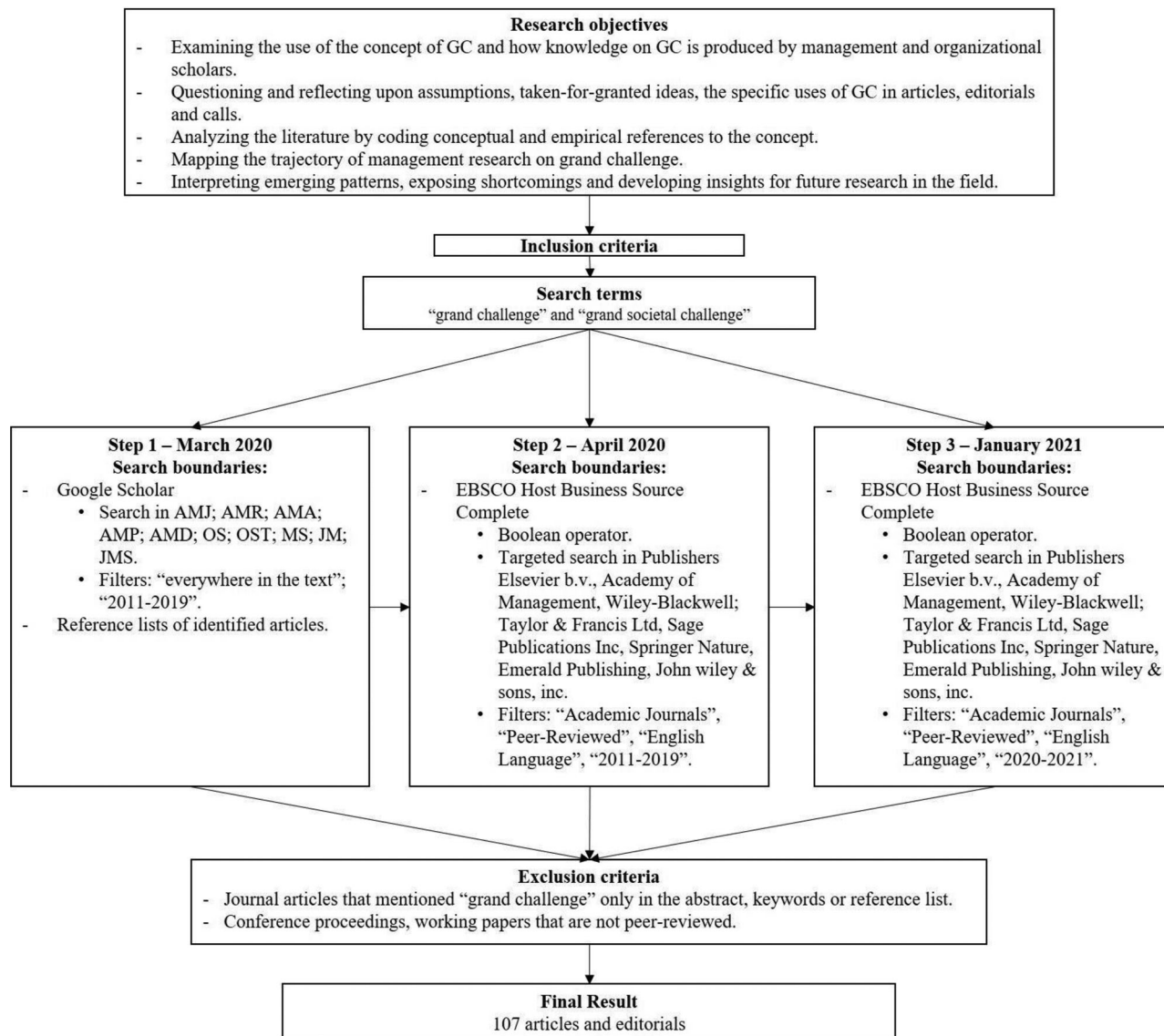


FIGURE 2 Overview of sample selection process. Figure adapted from Ceipek et al. (2019)

68% of the articles and editorials from eight journals, namely *AMJ*, *Australian Journal of Management*, *AMD*, *Journal of Business Ethics*, *AMP*, *Critical Perspectives on International Business*, *JMS*, and *Research Policy*. In the period of 2011–2019 92% of articles were published in journals ranked as 3 and 4 (according to ABS ranking). This percentage dropped to 55% in the period 2020–2021. In the period of 2020–2021 we also noticed an increase in articles published in specialized journals (e.g., *Industrial Marketing Management*, *Sustainability Accounting, Management and Policy Journal*, *International Journal of Operations & Production Management*, and *Human Resource Management Review*).

To assess momentum and evolution of GC research, we mapped the articles and editorials on a time axis. Overall, the number of publications referring to GC increased

rapidly over time, with a sharp uptick after 2015 (+12%). Five out of nine calls for GC papers were issued in 2017 and 2018, and these triggered a significant increase in the numbers of published articles. We noticed a wide variety of empirical referents used as instances of GC. To identify patterns, we coded empirical referents by applying a three-step process. First, we collected all referents of GC that authors referred to. This effort identified 235 examples in a total of 107 papers and editorials and nine calls for papers. Next, we synthesized these 235 examples into 78 first-order codes. In a final step, we aggregated related codes into 32 distinct themes. For instance, the empirical referents ‘Gender equality’, ‘Gender pay gap’, ‘Gender biases’, ‘Combatting gender inequality’, and ‘Managerial theory and research implications of gender research’ were coded with the first-order code ‘gender inequality’ and then

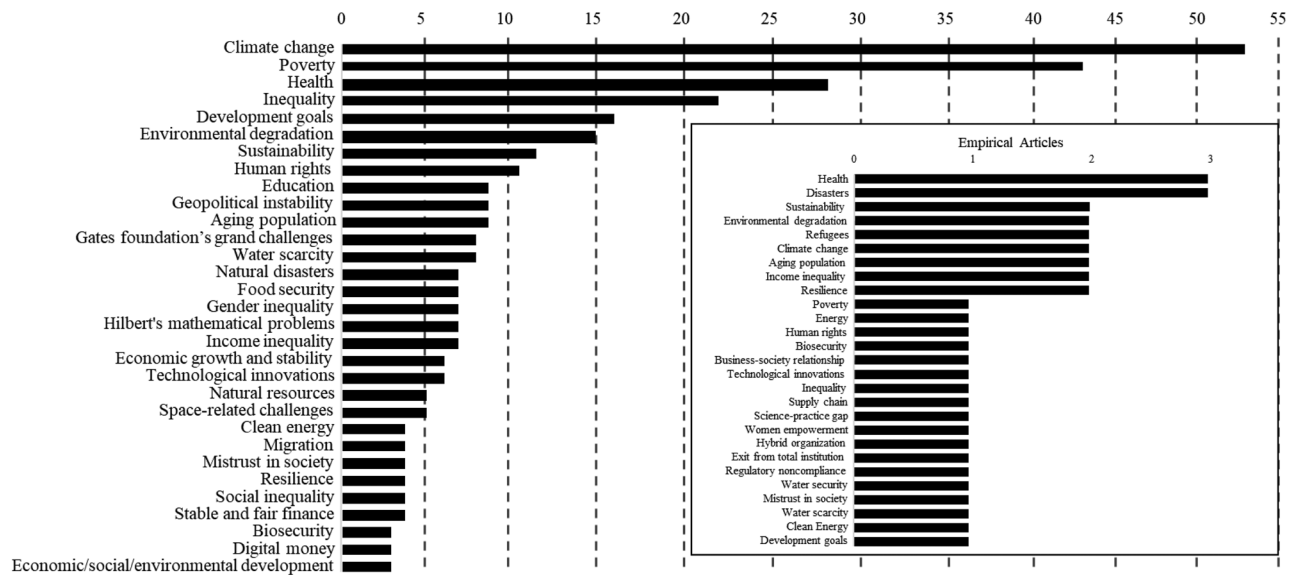


FIGURE 3 Frequency of empirical referents that management scholars consider as the most central for GC research in conceptual articles (large figure) and empirical articles (insert)

aggregated to the theme 'inequality'. Similarly, the empirical referents 'Geopolitical instability', 'War or conflict', 'Shifts in the geopolitical context', 'Endemic violent conflict', 'Increasing sociopolitical uncertainties', 'Resolving political conflicts', and 'Terrorism' were coded 'geopolitical instability' and then aggregated to the theme 'security'. GC themes comprise phenomena that range from technological innovations and digitalization to space-related challenges to migration and refugees.

To visualize patterns of empirical referents used in GC research, we counted the frequency of the appearance in conceptual articles as well as in empirical studies. Figure 3 exhibits the most frequent examples of empirical referents.

Management scholars framed climate change, poverty, health, inequality, and development goals as the most frequent illustrative examples for GC in conceptual papers. In empirical papers, the range of focal phenomena was far wider. Whereas climate change was mentioned in more than 50 conceptual articles, the most frequent referents in empirical papers, health and disasters, were only mentioned three times, and most empirical articles only addressed one referent.

REVIEW INSIGHTS

In the next three sections we build on this descriptive analysis. Employing the analytical scaffolding introduced in Figure 1, we assess GC's empirical referents, the range of empirical phenomena associated with GC, and the variance of conceptual attributes underpinning empirical and

conceptual research on GC. Finally, we reveal ambiguity in research perspectives that is visible in the different conceptual architectures in use. In each section we reflect on the implications of findings for theory development and practice and propose avenues for progress.

Empirical referents of the GC concept

Empirical referents bring concepts to life. They are observable and/or measurable instances of phenomena (Walker & Avant, 2005). Wilson (1990) suggested that one of the best ways to start with the analysis of a concept is to pick an uncontested empirical referent as an 'instance of the concept, something of which we would say "Well, if that isn't an example of so-and-so, then nothing is"' (p. 28). Unfortunately, we were unable to identify an empirical referent as a bona fide example of GC. Climate change, poverty, and health were the phenomena that researchers most frequently associated with the term. Whether these phenomena are GC or something else cannot be adjudicated because the literature does not provide consistent criteria for referring to a phenomenon as an instance of GC. We also have no basis for deciding which amongst all health issues are GC, or in which context health issues are GC or just normal aspects of social life. Consequently, we also have no basis for specifying the population of phenomena to which our knowledge claims can be validly generalized. We also noticed a weak intersection of phenomena that editorial notes and special issue calls indicated as central empirical referents of GC and those phenomena central to empirical papers (see Figure 3). A potential explanation for

this discrepancy is the difficulty for management scholars to study GC empirically, due to a lack of skills, lack of collaborations with researchers from other disciplines (e.g., natural sciences), or the complexity of phenomena and the long-term commitments required to adequately study them.

Another reason for this divergence might be that scholars merely frame their ongoing empirical research in terms of GC. We noticed several articles in the management literature that mentioned the term GC without providing any definition or reason for its adoption. Well-intended calls for special issues on GC and editorials in influential journals may trigger an opportunistic or even instrumental adoption of the concept. Scholars may perceive a higher probability of publication when framing their work as a contribution to GC research (Carl, 2020). Indeed, we recently participated in a PhD workshop where a senior scholar encouraged PhD students to frame their work in terms of GC to improve the odds of getting published. We admit that two authors of this paper are also guilty of this political manoeuvre by using the term GC opportunistically to be eligible for submission to a special issue on GC in a leading management journal. Hirsch and Levin (1999) identified such political moves as unintended consequences of efforts to institutionalize broad concepts.

Reframing ongoing research in terms of GC without any clear criteria for inclusion or exclusion expands the list of empirical referents. Some welcome this development and argue against a list of empirical referents that would define GC, expressing hope that boundary conditions of our findings will emerge (Howard-Grenville, 2021). We are less optimistic. In our view, this empirical proliferation threatens any meaningful differentiation of GC from the population of all societal challenges. This development is unlikely to generate a coherent set of attributes shared by several phenomena to provide clarity to the concept of GC in support of theory development (Gerring, 1999; Suddaby, 2010).

Apart from implications for theory development, choices about empirical referents have an important ethical dimension. Responsible GC research will require transparency about the criteria for selecting the focal phenomena as well as attention to phenomena we dismiss, ignore, or remain unaware of. For example, the phenomenon of modern slavery barely serves as an empirical GC referent despite important research on this challenge (Caruana et al., 2021). According to Oxfam, in 2020 more than half a million people were close to starvation worldwide (Oxfam, 2021), but in our corpus starvation is not mentioned or studied by management scholars as a GC. On which basis do we decide whether starvation should be considered in GC management research? Nomination processes may be criticized on grounds of representing

power dynamics and neo-colonialist tendencies where Western voices determine what is 'grand' and what is not. We found that GC research is mostly performed by Western scholars, even though many empirical GC referents matter for developing countries. More than 60% of articles were co-authored exclusively by researchers affiliated with an organization from an English-speaking country. Only four articles had co-authors from a non-OECD country.

Attributes of the GC concept

In this section, we focus on the breadth and consistency of GC attributes that researchers associate with GC, highlighting those attributes about which scholars had the most divergent and inconsistent assumptions. We use the current state of agreement about GC attributes as a rough indicator of concept clarity, which is an essential requirement for theory development (Suddaby, 2010). An emerging consensus about attributes and their permissible states determines whether criteria for inclusion of empirical referents will become more restrictive (potentially enabling a more focused research effort) or more expansive (potentially limiting our effectiveness as an epistemic community). Table 1 shows contrasting conceptual attributes in current GC research. The key insight from this table is the observation that selected GC attributes persist at a very broad range of permitted states. This variance threatens conceptual clarity and coherence and reduces its analytical competence.

Timescale of phenomena

Some articles referred to GC phenomena as short-term events. Examples include crisis-situations such as the current Covid-19 pandemic (Bacq et al., 2020), the refugee crisis of 2015, or natural disasters. Others focused on enduring problems that have evolved over long timescales. Inequality (Di Lorenzo & Scarlata, 2019), poverty, pollution, or climate change signify such enduring phenomena. Eisenhardt et al.'s (2016) claim illustrates the empirical consequences of permitting both attributes as defining the concept of GC: 'Grand challenges may be discrete with a clear endpoint, like landing a rover on Mars or developing a Zika vaccine, or broad and open-ended, such as curing cancer or eliminating poverty' (p. 1113). Linking these contrasting attributes with an OR operator clearly limits the usefulness of GC's conceptual architecture because phenomena with such contrasting attributes are unlikely to share sufficient commonalities to support theory development and refinement of the GC concept.

TABLE 1 Contrasting conceptual attributes (bold) in management literature on GC. References illustrate contrasting uses

Bacq et al., 2020	Event	Timescale of phenomena	Enduring	Di Lorenzo & Scarlata, 2019
Groen, 2019	Local	Spatial scale of phenomena	Global	Howard-Grenville, 2021
Buckley et al., 2017	Ambition	Types of phenomena	Problem	Ferraro et al., 2015
Markman et al., 2019	Individuals	Locus of solutions	Collectives	George et al., 2016
Mair et al., 2016	Micro	Level of analysis	Macro	Vakili & McGahan, 2016

Spatial scale of phenomena

Some authors assume that ‘local’ or ‘regional’ are attributes of GC phenomena. For example, Groen (2019) called the phenomenon of zero economic growth a GC that is particularly relevant for Japan and the Asia-Pacific region. Another example is the unequal distribution of access to water that creates local forms of water stress or water scarcity (Etzion et al., 2017; Touboulic & McCarthy, 2020). Other authors refer to empirical referents of GC that are global in nature, such as global warming and climate change or global development goals. For example, Howard-Grenville (2021) cited George et al. (2016) in her definition of GC as global problems. Unfortunately, this perspective illustrates a current tendency to socially construct the GC concept: ‘By this definition, the Covid-19 pandemic certainly registers as a grand challenge’ (Howard-Grenville, 2021, p. 254). Framing all global problems as GC makes the terms redundant. Using Gering’s (1999) criteria for evaluating conceptual goodness, we worry that the attributes of GC in current use lack coherence (crisis vs. enduring problems) and differentiation (global problems versus GC) to facilitate cumulative and comparative research.

Types of phenomena

Researchers frame GC phenomena as fundamentally different types or classes of phenomena. Some authors assume that GC are social problems or issues (Carl, 2020), and others argue that GC are the challenges or barriers (George et al., 2016) that make it difficult to solve problems. This distinction relates to the question of whether we intend to use GC as dependent variable or explanandum (e.g., to explain the nature of phenomena and to identify possibilities for intervention), or as an independent variable or explanans (e.g., to characterize the barriers to interventions that explain the persistence of GC). It is easy to see how this ambiguity of perspective could create inconsistencies and disagreements.

Some authors use GC to indicate ambitious efforts towards desirable social objectives. Landing a rover on Mars or sequencing the human genome are illustrative examples used in the GC literature. We also identified

uses of GC that combined ambitions and social problems. Buckley et al. (2017) referred to GC as ambitious objectives for solving important problems; an example would be the United Nations’ sustainable development goals. Other researchers refer to efforts towards problematizing future situations that are likely to occur but do not (yet) constitute social problems, such as concerns about how to prepare for an aging workforce. This variance in types of phenomena and associated attributes threatens the concept’s clarity. The many attributes associated with disparate phenomena hinders efforts to define how attributes relate to each other – a requirement for theorizing causal mechanisms that explain phenomena and their dispositions for being changed by practical interventions. Defining an analytically competent overlap in these various uses of GC still awaits dedicated effort towards resolving inconsistencies and agreeing on a core set of attributes.

Locus of solutions

Some authors have emphasized the efficacy of individuals (Markman et al., 2019; Schad & Smith, 2019) or single organizations (Mair et al., 2016) to address GC, while others insist that GC require collective action and collaboration across multiple stakeholders. For example, George et al. (2016) stated that ‘GCs, by their very nature, require coordinated and sustained effort from multiple and diverse stakeholders toward a clearly articulated problem or goal’ (p. 1881). These two perspectives are difficult to reconcile. George et al. (2016) proposed that multi-stakeholder collaboration is a defining attribute of GC. Consistently employing this definition would create an analytically useful lens towards those GC phenomena whose characteristics require collective effort. This perspective on collaboration might also help focus theoretical inquiries into the attributes of GC that explain why coordinated efforts rather than single-actor efforts are required to address GC.

However, this focus on collaboration may eventually be challenged by the legitimate claims that all efforts on any societal challenge require some form of collaboration. This claim may threaten the value of collaboration as a distinguishing attribute of GC. It might be more useful for theory development to treat collaboration as an empirical

question such as: Do certain GC require particular forms of collaboration, and do collaborative efforts on GC differ from collaborative efforts around non-GC phenomena? Collaboration then becomes an important attribute of GC that not all GC phenomena share. Theory development would thus provide contextualized insights into the nature of GC and inform decisions about which interventions are more likely to make a difference for which type of GC.

We noticed another problematic assumption about the nature of GC solutions. George (2016) stated that ‘the tackling of GCs could be fundamentally characterized as a managerial (organizational) and scientific problem’ (p. 2016). Increasing conceptual clarity requires relating these attributes to a specific set of phenomena for which this claim is true and excluding those phenomena for which primarily managerial and scientific efforts are unlikely to be effective. Examples may include challenges where power dynamics and social norms of elites resist efforts to change. The lack of effort to clarifying assumptions about key attributes means that GC research cannot clearly state what it seeks to explain.

Level of analysis

Some authors argue that GC – even when they constitute a macro-level phenomenon – can be effectively studied at a micro level. For example, Mair et al. (2016) studied inequality as a GC in India at the level of small rural villages, arguing that GC can be studied at the micro level of individuals or small groups when both the micro- and macro-level instantiations of a GC share similar features. This perspective implies that at least some GC have scale-independent characteristics. A contrasting perspective is offered by Ferraro et al. (2015), who claimed that GC are defined as problems that affect many people. This claim could encourage a tendency towards dismissing the validity of micro-level analyses. Three quarters of the articles in our corpus have taken a meso- (e.g., larger social groupings or regions) or macro-level (e.g., nations or the environment) perspective on GC. This reflection also highlights an opportunity to decouple analytical from definitional perspectives. Micro perspectives on global or ‘many people’ problems may help avoid the complexities involved in studying GC phenomena at a macro level and help to contextualize findings appropriately. Ferraro et al.’s (2015) focus on many people also highlights an opportunity for clarification by defining more precisely how many people it takes for a social problem to become a GC. As such, is a social problem in China automatically a GC while a social problem in the Faroe Islands might not qualify?

A related discussion has recently emerged that assumes variation in the degree of ‘grandness’ of challenges. Colquitt and George (2011) claimed that ‘the grandest

of these challenges are reflected in the United Nations Millennium Development Goals’ [and that] ‘the “grandness” of unresolved problems will vary from literature to literature over time’ (p. 432). Without specifying criteria for operationalizing or assessing ‘grandness’, we are afraid this distinction adds little value to theoretical or empirical research. Rather, we worry about creating a bias towards nominating ‘grander’ challenges that affect more people and employing macro levels of analysis as preferred empirical settings for our research.

The absence of any systematic discussion about the validity and usefulness of attributes to increase conceptual clarity (see Brammer et al., 2019 for an exception) implies that GC management research is still on an expansive pathway and that we tend to develop unrelated findings about phenomena that differ greatly from each other. Making progress towards GC theory development will require a discussion about the permitted variation of attributes across GC phenomena; whether their occurrence is always, sometimes, or rarely the case; or how many attributes a phenomenon must share for membership in GC. Another sign of progress would be the replacement of descriptive attributes such as the claims that GC are complex (Ferraro et al., 2015) or that they are short-term or enduring events with attributes that have a causal relation to the phenomena they are associated with. For example, developing complexity into a causal attribute would require knowledge development around how specific types of complexity are causally related to the emergence or robustness of GC or their liability to be changed through different types of interventions. Unfortunately, the increase in indiscriminately nominating phenomena as instances of GC will lead to a cumulation of mostly descriptive attributes that do not facilitate theoretical claims across phenomena because it becomes increasingly unclear what we are trying to explain. For this scenario, building on Goertz (2020), we predict an accumulation of OR operators that link the attributes within a conceptual architecture (e.g., GC describes phenomena that are short-term events such as the recent Covid-19 pandemic OR that constitute long-term processes such as climate change) to the detriment of AND operators that link attributes and support theorizing (e.g., attributes A, B, and C are necessary or sufficient to generate characteristics of GC). Unfortunately, in its current state attributes do not convey a core and essential meaning to the term GC.

Conceptual architectures in use in GC research

By looking for similarities in the ways authors framed GC as a concept, we noticed three approaches for

constructing conceptual architectures in the GC management literature: (a) a discursive architecture where socially constructed attributes define the conceptual membership of phenomena, (b) a family resemblance architecture grounded in similarity relations to other concepts, and (c) a phenomenon-driven architecture where the concept serves as an umbrella term for various GC phenomena. The three have distinct characteristics, but scholars employ them in partly overlapping ways.

Discursive architecture

The discursive conceptual architecture results from nominating GC attributes that are socially constructed narratives rather than empirically derived characteristics. Ferraro et al. (2015) are amongst the most frequently cited authors illustrative but also generative of the discursive GC architecture. They claimed that GC are constructed with variable ontologies that share attributes of affecting large populations, adversely affecting human welfare, and seeming intractable and resisting easy fixes.

The authors nominated complexity, uncertainty, and evaluative as defining attributes of GC but did not provide any empirical or theoretical justification for this selection. Unfortunately, neither the authors nor researchers who adopted this architecture made any claims about which minimal constellation of attributes constitutes a GC phenomenon. We have no basis for refuting the claim that any phenomenon with the attributes of being complex and affecting human welfare is a GC. This ambiguity fuels a tendency by researchers to socially construct a GC membership for their favourite phenomena by claiming a fit with the proposed attributes. Jarzabkowski et al. (2019) exemplify this tendency by claiming that the insurance protection gap is an example of a GC, basing this claim on a perceived match with some of the attributes proposed by Ferraro et al. (2015). We have no objective criteria by which we could scrutinize whether the insurance protection gap is a GC or some other phenomenon. The proposed GC attributes in this discursive architecture seem too generic to specify a concrete set of phenomena that is different from those specified by other related concepts such as social problems or wicked problems. As such, this contextual architecture provides little guidance for developing unique theory.

Current research employing this GC architecture has not yet developed measures that could capture variation in attributes such as uncertainty or complexity. Further development of this discursive architecture may depend on a consensus about attributes that generate a sharper empirical lens to refer to a differentiated set of phenomena. These attributes also need to be analytically relevant

to enable valid knowledge claims about phenomena (Gerring, 1999; Suddaby, 2010). However, we suspect that creating such a consensus may be difficult because even ambiguous attributes seem to be sticky if introduced at an early stage in a rapidly evolving field. This suspicion is supported by the fact that more than half of those papers which define the term GC by adopting a definition in a prior publication cited either Ferraro et al. (2015) or George et al. (2016), making these two articles by far the most cited sources of GC definitions. Many articles cite both definitions, even though the GC definitions of these two articles differ significantly. We are worried by the lack of critical scrutiny of these proposed attributes, and we do not see any conceptual development in terms of refinement, operationalization, or contextualization of this discursive GC architecture.

Family resemblance architecture

A different conceptual architecture based on a 'family resemblance' of the GC concept is also implied by Ferraro et al. (2015), who claimed that GC is related to concepts such as 'large-scale design problems (Simon, 1996), wicked problems (Rittel & Webber, 1973), messes (Ackoff, 1981), and commons problems (Dietz et al., 2003; Hardin, 1968; Ostrom, 1990)' (p. 382). Several subsequent articles adopted this claim and expanded the family resemblance of GC to 'superwicked' problems (Wright & Nyberg, 2017), social problems or social issues (Carl, 2020; George, 2016; Howard-Grenville, 2021; Stahl et al., 2020). The concept of wicked problems was mentioned most frequently (e.g., see Marti, 2018; Orsato et al., 2019; Robinson & Mazzucato, 2019; van Wijk et al., 2019).

Family resemblance relies on defining membership based on a shared set of attributes where no empirical referent needs to share all attributes that are defining for a concept (Collier & Mahon, 1993). This approach may enable integration of existing research on related concepts into efforts of theory development around GC while maintaining a sufficiently broad empirical focus. Unfortunately, we discovered few efforts to define the attributes that GC shared with related concepts and the boundaries that separate GC from related concepts. Gerring (1999) worried that

this sort of semantic confusion throws a wrench into the work of social science. ... Older concepts are redeployed, leaving etymological trails that confuse the unwitting reader. New words are created to refer to things that were perhaps poorly articulated through existing concepts, leaving a highly complex lexical

terrain (for the old concepts continue to circulate). Words with similar meanings crowd around each other, vying for attention and stealing each other's attributes. (pp. 360–361)

Sartori (1975) interpreted this approach to concept formation as scholars 'playing musical chairs with words' (p. 9) under a perceived pressure to appear original and innovative.

To advance this architecture, researchers could combine analytically competent attributes of concepts such as wicked problems to specify a related but also unique GC concept. For example, Dorado et al. (2022) proposed that the obstacles for businesses engaging with GC could define a family resemblance between GC and related concepts. But the analytical utility of adopting this GC concept has not yet been established, and a focus on obstacles might constrain GC research. Alternatively, researchers could start with GC phenomena that other concepts refer to and identify analytically competent attributes, such as those that have the strongest practical implications. Bailey and Breslin (2021) took an illustrative step in this direction by identifying insights of practical and theoretical significance from recent reviews on concepts and attributes such as resource dependency, resilience, or leadership.

Phenomenon-driven architecture

Almost half of all papers in our corpus used a phenomenon-driven conceptual architecture where authors nominated phenomena such as climate change or poverty as seemingly self-explanatory GC referents without providing any rationale for this claim. This problem and phenomenon orientation is a refreshing attitude that counters criticisms that management scholarship is too theory-driven (Graebner et al., 2022) and insufficiently relevant (Harley & Fleming, 2021). However, a purely phenomenon-driven approach that cannot specify the analytically important and causally competent shared attributes is unlikely to generate valid and useful knowledge claims across GC phenomena.

The lack of any specification of analytically important attributes threatens to accelerate a trend observed by Clegg (2002) as 'organization theory [becoming] increasingly its own series of language games' (p. 435). GC language games that we notice in this GC architecture include arbitrary nominations of phenomena as instances of GC or arbitrary distinctions of the grandness (Colquitt & George, 2011, p. 432) of social problems. A continuation of GC language games would be detrimental to progressive management scholarship (Astley & Zammuto, 1992; Tsoukas, 1991). Further adoption of this – already

dominant – conceptual architecture risks perpetuating the rapid accumulation of varied types of phenomena under one conceptual umbrella. We are concerned that such an approach fuels unconnected studies of allegedly interesting and important GC phenomena. We also worry that knowledge claims may be insufficiently contextualized to inform practice.

All three conceptual architectures promote an expansion of the diversity of empirical GC referents. A potential way forward might mimic the approach taken by Barrett (2007), who theorized different types of providing public goods. A generic term like 'public goods' could be conceptually linked to any number of empirical referents and would then pose the same analytical challenges that we witness for the GC concept in management research. Barrett (2007) wrote: 'Since global public goods differ in fundamental ways, however, we cannot simply lump them together. We need to classify them' (p. 2). He identified the ways in which public goods are supplied as analytically important attributes, and used these attributes to differentiate between four classes of public goods.

TOWARDS PRINCIPLES FOR MANAGEMENT RESEARCH ON CHALLENGES OF SOCIETAL CONCERN

Retiring GC as a concept

Our analysis of the theoretical and empirical content of the GC concept in management scholarship on GC exposed unnoticed variation in assumptions, perspectives, and approaches that management scholars adopted. We conclude that current efforts to conceptualize GC do not support systematic theory development and that the concept primarily accumulates disparate phenomena as empirical referents.

Suddaby (2014) was quite concerned about the fate of such incoherent efforts, stating: 'If new and old researchers cannot agree on or communicate the basic elements of a phenomenon, the accumulation of knowledge cannot occur' (p. 352). He worried that the inability to differentiate concepts and draw clear conceptual boundaries created a 'Tower of Babel' effect where a proliferation of different terms and labels for similar phenomena generates a fragmentation of efforts. Clearly defined concepts are fundamental to theorizing because they can be operationalized, enable comparison of research findings, and 'encourage researchers to generate more effective research questions, apply appropriate and epistemologically consistent methods, and identify exceptions to the categories that open opportunities for future research' (Suddaby, 2014, p. 355).

The well-intended statements by influential editors who welcome the expansion of empirical referents of GC (and the concomitant expansion of attributes) without a more explicit scheme for classifying GC risk letting GC research become caught up in an endless language game and turning GC into an overly broad concept. Hirsch and Levin (1999) described such broad concepts as umbrella constructs, arguing that if such concepts fail to develop a consensus about their theoretical and empirical content, they will have their validity challenged. If their argument was correct, we would not worry about the fate of GC research, as the validity police function of our scholarly collective would ensure efforts towards conceptual clarity or retire the concept in due course. However, Alvesson and Blom (2022) recently noted that the validity police function is ineffective and that even ambiguous umbrella concepts can have a long lifespan.

As such, used as an umbrella concept (Gümüşay et al, 2022) GC would offer little value to science and might mainly benefit individual editors and scholars who – by framing their research in terms of GC – might get papers published more easily. We are not the first to propose ideas for improving GC research. Brammer et al. (2019) intended to provide more precision in how the term GC was used in research by pursuing ‘a synthetic approach that draws together strands of the existing literature into a holistic characterization’ (p. 521). However, reviewing GC literature in management, we did not find any systematic efforts towards improving conceptual clarity. This finding creates an important puzzle: How can we know whether we are successful if we do not define what we seek to explain? Our analysis does not support the idea that GC as a concept could facilitate theoretical development on important issues of societal concern. At the same time, we should not lightly dismiss the enthusiasm and momentum around the term GC. We, therefore, suggest using GC to refer to a set of research principles rather than as a concept that – like Frankenstein, the sad monster – could hardly walk, let alone run. As a hopeful monster, the term GC would stand for a principles-based research effort that aligns our scholarship around important whys and hows of GC research.

GC as a set of research principles

Applying a set of clearly articulated principles for GC research would enable us to address long-standing criticisms of management research. Influential voices have criticized management scholarship as being multi-paradigmatic and thus pre-scientific because scholars grounded in different research principles cannot or do not want to understand and engage with each other (Azevedo,

2002; Baum & Rowley, 2002; McKelvey, 2003; Pfeffer, 1993; Tsoukas, 2000). The lack of consensus on research principles is reflected in endless debates and numerous editorial essays on what a theory is, how to accumulate knowledge, and whether research is relevant for society (Alvesson & Sandberg, 2013; Harley & Fleming, 2021; Sutton & Staw, 1995). Culprits are easily identified: the incommensurable variety of ontological and epistemological commitments that underly research conventions, bias doctoral training programs, and affect publication processes. To make progress, we will need to get these critical voices out of the editorial essay parking lot and enact them in research practice. We see the continued excitement about GC research as an opportunity to agree and enact a shared set of principles that guide and guard a progressive agenda on GC in management research.

Guidance by research principles allows GC research to stick to its own origin story grounded in those editorials that marked the beginning of GC scholarship in management and organizational studies. For example, Colquitt and George (2011) proposed the commitment to research principles as a requirement for making theoretical advances on GC that traditional scholarship cannot create, instead of proposing a future of GC based on conceptual development. Jointly with George et al. (2016), these early editorials provide us with core principles for GC research that include urgency and broadening our theoretical and empirical scope. Unfortunately, these three principles alone do not shield us from incommensurable, multi-paradigmatic research efforts hampering theoretical development and practical usefulness. We therefore add the principle of realism as a metatheory that aligns these principles with practice-oriented theorizing. These four principles and their implications for management research on GC can be summarized as follows.

Urgency

Urgent action on global problems is not compatible with GC research as just another language game. George et al. (2016) reminded us that there is no plan B for action as there is no planet B [and therefore] there is a moral imperative that we act to guide business leaders, employees, and stakeholders with systematic, unbiased, and empirically robust evidence on mechanisms with which to tackle the persistent, but tractable, global problems confounding us (p. 1893).

Urgency as a core principle for GC research implies that we can no longer ignore long-standing criticisms of the shortcomings of management research, such as the critique that the relation between management research and management practice is broken (Astley & Zammuto,

1992; Banks et al., 2021; Vermeulen, 2005). We must define an effective agenda for GC research and stop proposing calls for special issues on GC to promote uncoordinated research that is merely aligned by an ambiguous term. But urgency alone is not sufficient as a principle. The rapid proliferation of articles in the first decade of GC research indicates that a sense of urgency, without complementary principles, does not create a cumulative research effort.

Broadening our theoretical scope

Colquitt and George stated that ‘studies cannot tackle grand challenges if they are not ambitious in scope’ (2011, p. 434). The authors urged researchers to theorize unfamiliar topics and to engage a broader set of literatures to help overcome taken-for-granted assumptions and to change the conversation within and across existing literatures. Adequate understanding of the causal architectures of GC requires researchers to leave their physical and intellectual comfort zone and engage closely with the phenomena they study, nurturing curiosity about unfamiliar literatures and research domains.

Broadening our theoretical scope by exploring other fields and disciplines expands the repertoire of mechanisms that we consider and explore in GC research. For example, Dorado et al. (2022) provided a knowledge map of several types of obstacles to engaging with GC grounded in various literatures that might help to specify the concrete obstacles for a given GC. Broader theoretical perspectives increase the explanatory depth of phenomena as well as informing potential practical interventions. Enacting this principle may have the two-way effect of making management research more accessible to scholars from other disciplines while also enabling more substantive contributions to other disciplines. Broadening the theoretical scope of GC research necessitates nurturing attitudes of patience and commitment to build new research skills, becoming familiar with new but relevant literatures, and adequately engaging with difficult-to-grasp phenomena. Adopting this principle has serious implications for training, research, careers, and publication practices. As Fainshmidt et al. (2021) pointed out, management research on GC requires much more ambition to recalibrate ‘how research is conducted, evaluated, and disseminated to society’ (p. 1416).

Broadening our empirical scope

Colquitt and George (2011) proposed that research on GC needs to cast a wider net and that authors should not ‘slice their data too thin’ (p. 434). This principle aligns

with the third mandate of organizational scholarship proposed by Stern and Barley (1996). Such a mandate calls for empirical and theoretical attention to how organizations influence and shape the trajectories of social systems and expands traditional ways of studying how organizations address or are affected by GC (Mair & Seelos, 2021). Illustrations for a traditional perspective are Brown et al. (2022), who identified organizational survival as the key dependent variable in GC research, or Voegtlin et al. (2022), who identified responsible innovation as an approach by which businesses help solve GC. A third mandate perspective expands a focus on solving GC towards imagining and assessing possibilities and roles for organizations to generate fewer ‘grand’ problems in the first place (Ackoff, 1981). GC research aligned with the principle of broadening the empirical scope engages closely with phenomena to let theorizing be guided by important and difficult-to-observe aspects. Aligned with this principle, recent essays suggest that GC research needs to engage much more closely with relevant stakeholders and embrace complexity (Wickert et al., 2021; Williams & Whiteman, 2021).

However, empirically broad scope requires transparency about how we define the boundaries and develop analytical scaffoldings to translate complex social context into concrete phenomena for investigation (Mair & Seelos, 2021). Defining boundaries includes transparent decisions about which voices are selectively represented in research data and which stakeholder concerns may be ignored. Murphy et al. (2021) offered methodological guidance and encouragement for GC research that is broad in empirical scope. Such research might require multiple parallel research projects aligned by shared research principles but involving multidisciplinary teams who engage with a diverse set of stakeholders (Murphy et al., 2021). The authors also suggest that empirically broad efforts will require a radical rethinking of research, coordination, and publication practices. At the same time, ‘the route to good theory leads not through gaps in the literature but through an engagement with problems in the world’ (Kilduff, 2006, p. 252), and thus we consider broadening the empirical scope of GC as essential for theoretical development.

Realism and practical relevance

Almost all GC articles we reviewed appear to follow traditional relativist or positivist principles and methods. But relativism and positivism are based on incommensurable ontological and epistemological positions assumptions that determine how scholars think about the acquisition, role, and validity of knowledge about GC (Azevedo, 2002). This epistemic divide creates parallel scholarly worlds without incentives for relating, bridging, or

integrating perspectives, approaches, and findings. We fear that applying the principles of urgency and broadening theoretical and empirical scope without explicit reflection on epistemic commitments risks generating disconnected research findings and fuels a continued inflation of meanings and uncoordinated research agendas that threaten to let the term GC become a dead metaphor (Tsoukas, 1991).

Therefore, aligned with other prominent scholars, we believe that the explicit adoption of ‘scientific realism, the most widely accepted epistemology among current philosophers’ (Baum & Rowley, 2002, p. 21) by a critical mass of scholars can exert an integrative role in otherwise disparate GC research efforts. Realist ontological commitments and epistemological principles constitute an effective metatheory for theorizing managerial efforts in complex settings (Seelos & Mair, 2014; Tsoukas, 2000). As a metatheory, realism helps ‘to organize a complex empirical world’ and prevent researchers from being dazzled by the complexity of social phenomena (Bacharach, 1989, p. 496).

Realism can help integrate and overcome the limitations of positivist and relativist positions. Positivism limits the roles of researchers to just one: a neutral observer of reality. This role denies creative forms of engaged and activist scholarship that may be required for studying GC and testing the real-world efficacy of our knowledge claims. However, empirical regularities discovered by positivist scholarship can be the basis for realist inquiry centred on understanding causal black boxes by identifying how social mechanisms create observable events in a context-dependent manner.

A focus on realism does not replace – but can facilitate – linking findings from relativist scholarship to an objectively existing world and developing discourse-independent criteria for evaluating knowledge claims. Relativism can help unearth social mechanisms, frames, and meaning structures that cannot be identified from the role of a neutral unbiased observer. But relativist concepts and terms do not correspond to an objective reality; instead, they exist relative to conceptual schemes (Mauws & Phillips, 1995). The term GC can mean anything and thus nothing, and knowledge claims of one scholarly community cannot be evaluated by another community operating from a different language game, as Norris (1996) explained: ‘Every single truth-claim that was ever entertained by a community of like-minded knowers must count as valid when referred to the language-game, vocabulary or belief-system then in place’ (p. 172). Realist scholarship can critique relativist knowledge claims by a commitment to an objective referent. Sayer (2000) offered the following to justify this position:

It is the evident fallibility of our knowledge – the experience of getting things wrong, of having our expectations confounded, and of crashing into things – that justifies us in believing that the world exists regardless of what we happen to think about it (p. 2).

Realism ideally supports cumulative GC research by validating theories in terms of their practical adequacy, ‘that is in terms of the extent to which [a knowledge claim] generates expectations about the world and about results of our actions which are realized’ (Sayer, 2000, p. 43). This focus on practice counteracts tendencies to limit practical implications of research to a sidenote in the discussion section of articles and integrates a fourth principle that Colquitt and George (2011) emphasized for effective GC research: ‘Actionability: Insights for Practice’ (p. 434).

We do not expect every GC research project to adopt realism explicitly. However, even a smaller epistemic community around realist GC research supported and encouraged by senior scholars and editors would create important momentum while leaving sufficient space for scholars following different ontological and epistemological traditions.

CONCLUSION

Reviewing management research on GC revealed an enthusiastic adoption of the term GC and a growing momentum of publications driven by the encouragement of senior editors of leading journals, calls for special issues, and – unfortunately – low barriers to entry that enable almost any topic to be portrayed as GC research. A decade ago, we (the authors) were excited about management scholars’ increasing attention to studying phenomena related to challenges of societal concern. We still are. But our review exposed variety and inconsistency of empirical referents and attributes of the concept GC and a lack of cumulative theorizing and deriving practical insights. Based on these findings we propose retiring GC as a concept.

To keep the enthusiasm and momentum around GC research but raise theoretical and practical ambitions, we suggest replacing the focus on conceptual development with a commitment to a principles-based research agenda for GC research. Jointly, the four principles we propose constitute a genuine origin story for management research; we believe they can reverse the trend to develop knowledge in disjointed communities around criteria that do not enable comparison or objective validation and will encourage a rethinking of the infrastructure around

knowledge generation and dissemination in management research. We are aware that dominant epistemic positions and methodological preferences, as well as power asymmetries in review- and career-advancement processes, make the enactment of the proposed principles very risky, particularly for early-stage scholars. Yet, we see GC research as a unique opportunity for management scholars to finally enact long overdue and extensively discussed revisions in what knowledge we prioritize and how we go about generating it.

A progressive research agenda centred on developing theory that is practically useful and based on deep engagement with GC has the potential to allow management researchers not only to use literatures from various disciplines, but also to inform and contribute to those disciplinary literatures and make knowledge more accessible to decision makers.

Finally, we hope that our approach to conduct a critical but constructive assessment of research in a nascent field of research provides a useful alternative approach to the policing function by reviewers and editors that counteracts unproductive conceptual development (Hirsch & Levin, 1999). Reviews like ours help to evaluate whether research agendas can best be supported by the policing function to help refine concepts or whether a principles-based research agenda might be more effective. For example, research on social entrepreneurship might have benefited from such a review a decade ago. Suggesting the adoption of research principles for social entrepreneurship research might have facilitated more theoretical and empirical progress and prevented persistent ambiguity about what the term means in theory and practice (Chliova et al., 2020).

We are hopeful that the second decade of GC research will be marked by ambitious advances in both theory and practice. At the same time, we recognize that to get there it will take a shared willingness to revisit some of the most fundamental rules of how management research is conducted.

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ORCID

Johanna Mair  <https://orcid.org/0000-0001-5999-9232>

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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