



# Developing and Publishing Strong Empirical Research in Sustainability Management—Addressing the Intersection of Theory, Method, and Empirical Field

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## OPEN ACCESS

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### Specialty section:

This article was submitted to  
Sustainable Organizations,  
a section of the journal  
Frontiers in Sustainability

**Received:** 15 October 2020

**Accepted:** 31 December 2020

**Published:** 05 February 2021

### Citation:

Seuring S, Stella T and Stella M (2021)  
Developing and Publishing Strong  
Empirical Research in Sustainability  
Management—Addressing the  
Intersection of Theory, Method, and  
Empirical Field.  
Front. Sustain. 1:617870.  
doi: 10.3389/frsus.2020.617870

Students starting their research into sustainability management are often driven by a normative assumption of wanting “to do something good” or “save the world” from this or that problem. This also holds for many researchers, where the pressure to do research that has an immediate impact on the local business or natural environment is paramount. This often gets into the way of developing sound research that might pass the review process in strong academic journals. Good (empirical) research builds on the interplay of the theoretical foundation, appropriate research method, and a well-justified selection of the empirical field. The discussion paper offers some guidelines and reflections on how to do this. The core point is that academic papers get cited for their theoretical contribution, so this has to be in the foreground of research question and design. If implemented in the wrong manner upfront, this can usually not be corrected later on, preventing the research to be published in top journals. This has to do with the interplay of theory, method, and empirical field. While we would see theory as the winning factor, methods and empirical field-related choices often constitute what might be called qualifying for hygiene factors. Methods and empirical field would hardly sell the paper on their own, but if done wrongly, they will prevent it from having a chance of being accepted. The paper explores some core ideas around theory, methods, and empirical field and offers some related guidelines on how to link them. This is illustrated at some points borrowed from debates in sustainability management.

**Keywords:** theory, method, empirical research, sustainability management, relevance of research

## INTRODUCTION

Identifying an excellent piece of research is often quite challenging. In many cases, only the test of time will identify a paper that makes an impact on their field and even be called a seminal paper. Even when a paper is accepted after a sound review process, it is hard to predict how the research community would react and act on it. Yet, there are some guidelines, where following them, make it more likely, that a paper would pass the review process.

The related choice of (1) theory, (2) research method, and (3) empirical field are highly interrelated. Once certain investments into the research process have been made, they are often

hard to change, linking this to typical aspect of the (sustainable) new product development process (Gmelin and Seuring, 2014), where 80% of the decisions have been made, while only 20% of the cost or time have been incurred.

This links into a further motivation for writing this piece. Visiting different parts of the world was an opportunity for getting into contact with many highly motivated researchers. Quite so often, they were dedicated to “making an impact” with their research, a typical aspect of transdisciplinary research (Lang et al., 2012). This is often demanded in emerging economies or low income countries, where funding for universities might be coupled to an expectation that it would have a positive impact on its environment. While such often-practical implications are not wrong in itself, this notion was often in the way of stepping one step back and looking at their own research in a more reflective manner. Even more, the inherent choice of theory, or a lack of theoretical grounding, the (missing) justification for the research method employed and the empirical field in total usually did not qualify that research and respective findings for publication in highly regarded academic journals. It is often hard to explain what is “wrong” in such cases. Here, we emphasize obtaining research results that are publishable in journals, which many researchers would regard as being of high standards and, usually, having sound impact factors, Citescore values, or other journal related indicators. There would be alternative measures of research impact, linking it, e.g., to transition management (Stephens and Graham, 2010) and action-oriented research (Caniglia et al., 2020), thereby also asking for the impact on real business contexts. The paper addresses the question: How can theory, method, and empirical field be interrelated in creating strong research questions?

The paper explores some reflections on developing strong research. Admittedly, this is a somewhat biased perspective, which is highly dependent on the authors’ personal experience and perceptions. Still, taking the intersection of sustainability management and supply chain management as an example, some guidelines can be put forward. This is also, why much of this paper will use the else not very popular “I” and “we” style, emphasizing that a personal perspective is put forward. This is also the justification for citing many references from own research, being justified as they will serve as illustrative examples.

The discussion paper is structured into two parts. The first one introduces some core terminology on theory, method, and empirical field. The second part looks then at typical choices to be made when conducting research. While some overlap can hardly be avoided, the three issues are analyzed on their own; before then, their interplay is analyzed. A final note goes to the aspect journal quality, which seems to be needed for completeness. A brief conclusion ends this paper.

## CORE TERMINOLOGY

The three core points addressed in this paper are (1) theory, (2) research methods, and (3) empirical field. Here is a brief outline, not aiming at giving full explanations, which are available in respective books and guiding papers.

## Theory

As the first reference point for the term theory, the four criteria put forward by Wacker (1998) are employed:

- **Definitions of core terminology:**  
Definitions are usually the starting point of every academic debate, so that a common ground is reached. In sustainability management, the triple bottom line approach (Elkington, 1998; Dyllick and Hockerts, 2002; for a critical assessment Elkington, 2018) serves as such a foundation, but there are multiple other links, e.g., into organizational sustainability (Lozano and Garcia, 2020).
- **Boundaries, where a theory applies and where not:**  
Boundaries are often quite hard to establish and can shift over time. Much of the sustainability debate started more on the environmental management side and only gradually expanded into social arguments. At the moment, it might be hard to see what sustainability management does not cover as the term is used quite often in an encompassing manner. For the respective piece of research, clarifying the boundaries and the unit of analysis is of central relevance (see Busse et al., 2017) or linking it to the core or the Lakatosian “protective” belt of theory in a field (see Gold, 2014).
- **Variables or constructs and their interrelations:**  
Variables or constructs are the typical operational entities used for the analysis. Quite often, they are further broken down into items or indicators. In sustainability management, the triple bottom line might rather form a concept, which is operationalized into the three dimensions (environment, economic, social). These dimensions are then further measured, by employing related indicators. This also holds for stakeholder theory-related aspects, which consist of different conceptual elements (e.g., Schaltegger et al., 2019).
- **Predictions:**  
A theory should allow to make predictions by linking the constructs or variables to the real world on what could (1) be observed, or (2) might be developed, or (3) how certain things might be implemented. As one example in sustainability management, there is a wide debate on the application of environmental and social standards, how they can be developed, and what effect might be reached putting them into practice and whether they would contribute to sustainability (De Lima et al., 2021). The typical prediction is that implementing the standards allows overcoming certain problems and leads to improved environmental and social, sometimes even economic, performance.

These four criteria might be complemented by the classical pieces on theory by Sutton and Staw (1995) on “What theory is not” and the comment to this by Weick (1995) on “What theory is not, theorizing is.” Taken together, these two short notes offer great advice for academic research. They point to shortcomings and an overreliance on, e.g., references, data, list of variables, or diagrams, as these are misinterpreted as theory. Such elements have to be set into their theoretical context. This emphasizes the aspect of theorizing or moving toward theory, which often does not emerge in one piece, but is crafted by multiple contributors over time (Starbuck, 2004).

## Research Methods or Research Process

There are multiple textbooks on all kinds of research methods. A kind of overarching picture is presented by Saunders et al. (2019), which sums up core decisions on the research methodology in their research onion (see **Figure 1**).

Looking in more detail at the research process, Stuart et al. (2002) provide a basic research process in five steps. While Stuart et al. (2002) use this to explain case study research, the five steps are so basic that they would apply to all fields and kinds of empirical research in the social sciences (see **Figure 2**). This might have to be modified or amended, but is a straightforward guideline ensuring that core steps of the research process are carried out and documented in a stringent manner. Developing a research question, entering the field, collecting and analyzing data, and making sense of the data by writing up offers a quite basic outline of almost any research process. Applications can be found in numerous other books and papers, so there is no further explanation of these steps needed here (see, e.g., Seuring, 2008).

While this will be repeated in this paper several times, it cannot be overemphasized that each choice made need to be justified. Taking the research onion (Saunders et al., 2019) and the research process as a starting point and justifying each choice made will ensure that the reader can understand this quite well. The challenge might be to do so in a concise manner, keeping the overall length and composition of the paper in mind.

## Empirical Field

An editorial by Crane et al. (2016) is titled: “Publishing Country Studies in Business & Society Or, Do We Care About CSR in Mongolia?” As they point out, the title is certainly not discriminating authors from Mongolia or empirical research being based on data from Mongolia, nor from any other country. Reverting the title, it is preferred to argue about the point that research findings and contributions from Pakistan, Ghana, Slovenia, or Chile should also be of interest to researchers in other parts of the world. Hence, solving a practical problem on a local level might be relevant to people in the close environment, but might not receive much interest beyond this point. Researchers often take too much for granted that what they or even more their environment views as relevant. Yet, the question is, whether this would also be seen as relevant by other researchers. Looking at the many different settings we have globally, this needs to be well-justified, thereby aiming for generalizability of the findings.

More positively spoken, it is certainly not wrong to collect data in your local environment, be this in Norway, Mexico, Indonesia, Uganda, or New Zealand. In all cases, you have to consider, which similar or divergent context would be found globally, where certain conditions would apply so that a transfer of the research might be justified. This must be reflected upon in the limitations section of the paper.

## STARTING AND CONTINUING THE RESEARCH

Textbooks on research methods usually contain a chapter on developing research questions. In the context of research practice,

much of the research rather develops step by step. After the completion of the PhD thesis, few researchers staying in an academic environment change their topic completely. This has pros and cons at the same time:

- Pro: You can continue a well-developed stream of research, build on your knowledge in theory, method, and empirical field.
- Con: Always staying on the same direction might limit what you can achieve further. If you are in a field rather declining in relevance, when is the right time to step out?

Related to this is the question, what can better be adjusted, where a typical sequence might be: (1) empirical field, (2) theoretical foundation, or (3) methodological choice? Please note, that this sequence is used here to explain and illustrate a certain issue. This is not a sequence for the choice of research questions or topics overall. At the end of each section, we will provide a proposition for what to keep in mind.

## Empirical Field

Many pieces of research are starting in a particular empirical context. This might even be the case for large research grants, such as the ones funded by the European Union Horizon 2020 program for Innovative Training Networks. These projects often center on a rather practical or applied topic, e.g., Circular Economy (see <http://www.retrace-itn.eu> last access December 28, 2020) as just one example. This is justified as such topics trigger a broader research interest and most likely can have a significant impact. It is hard to devise particular hints for which empirical fields or phenomena are worth to receive related research. If you manage to be early in a field of rising attention, this might be very beneficial for receiving, later on, great recognition, and being very careful with suggestions here, many fields are worth being researched.

In recent years, many topics around sustainability and sustainable products (Gmelin and Seuring, 2014; Dyllick and Rost, 2017; Lozano and Garcia, 2020) certainly have been on the rise, and their relevance is well-documented. This does not only hold for climate change or resource consumption but also cover biodiversity, marine ecosystems, plastic pollution of the environment and oceans, or working conditions in global supply chains (as rather arbitrary examples, e.g., Seuring, 2012; Khalid et al., 2020). This is a list only containing a few items, so there are certainly more topics warranting future research.

Looking to the wider business environment, the increasing use of digital technologies is certainly such a topic, where the link to their sustainability impact is evident, but not much explored so far (Liu et al., 2020). This will certainly be a topic staying on the agenda for the next decade. The impact of digital technologies on businesses and society at large will be far reaching; the sustainability implications are only explored toward some first aspects. An obvious issue, such as the energy consumption of computer systems, is only the surface level, as, e.g., Corbett (2018) rightly points out.

The empirical field needs constant adjustment and will be checked for every major piece of research started. Classical choices, such as agriculture and food (industries) can hardly

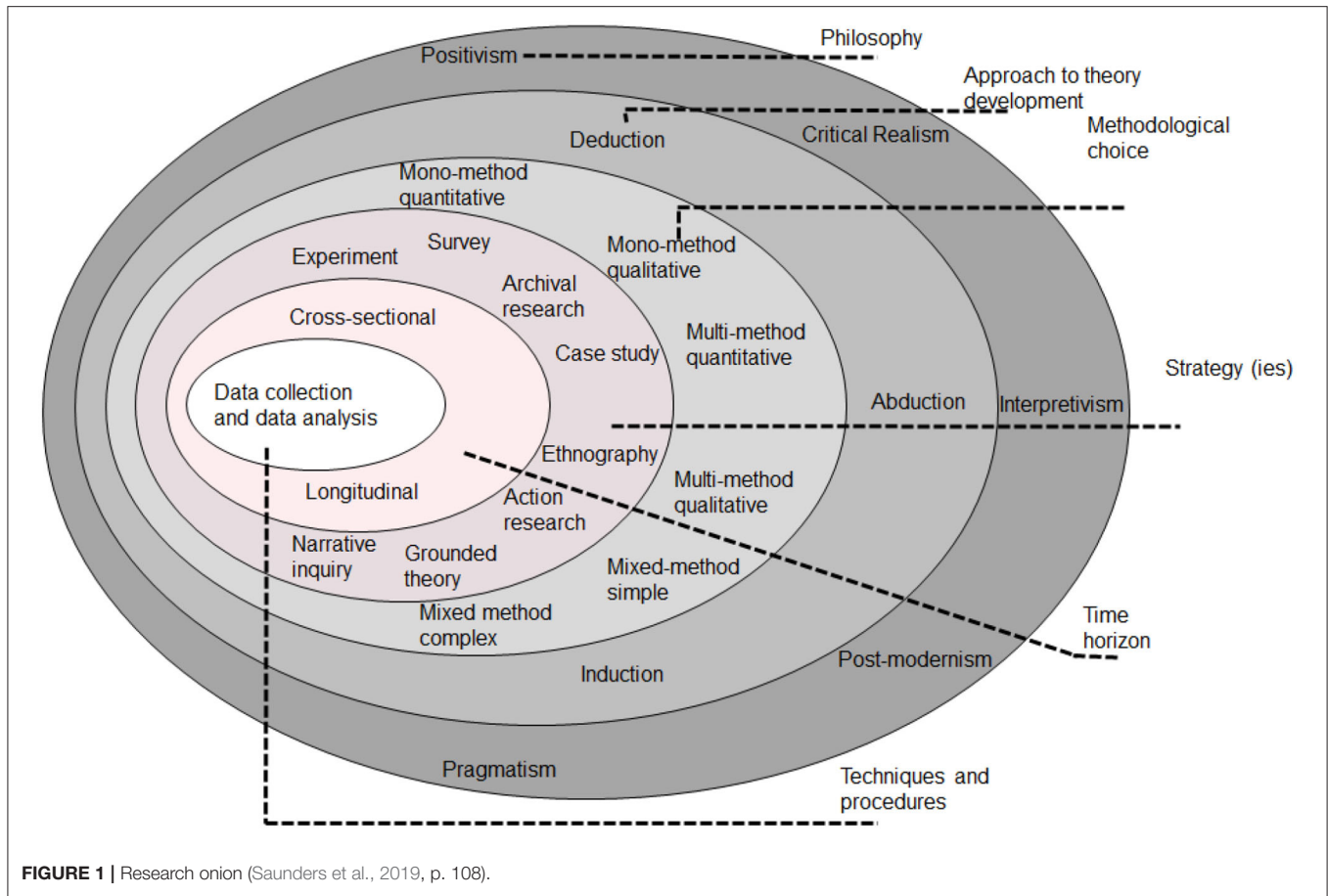


FIGURE 1 | Research onion (Saunders et al., 2019, p. 108).

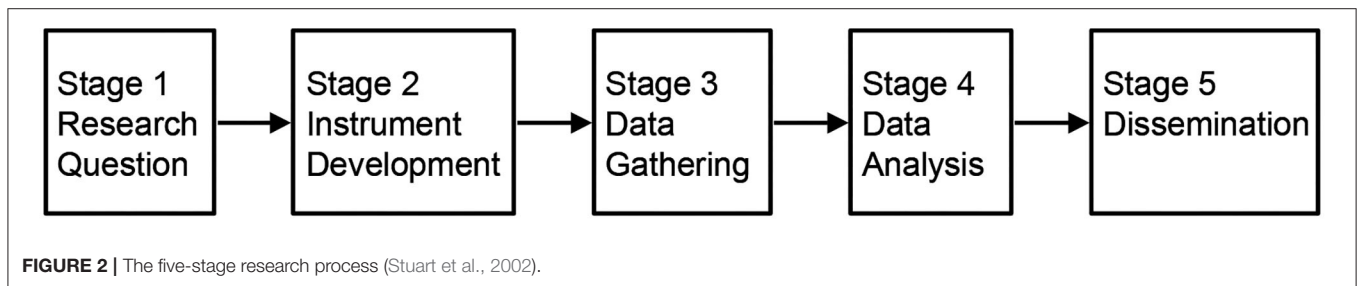


FIGURE 2 | The five-stage research process (Stuart et al., 2002).

be wrong, as they impact on every human beings' life. Still, conducting the 200th study on why consumers do not buy more organic food, thereby relating to the attitude–behavior gap (Aschemann-Witzel and Niebuhr Aagaard, 2014), might be hard to publish in leading journals. The new study would have to offer a novel contribution, moving beyond the already known elements of theory in the field. A similar example would be drivers and barriers for sustainable supply chain management. After several review papers on the topic have been published already (Diabat and Govindan, 2011 for an early one in the field; Sajjad et al., 2015), there is hardly any additional insight to be gained, even if a different industry in a different country would be addressed. It might be almost trivial, but a sound reasoning of the choice of the practical problem and empirical

field chosen needs to be presented. This seems to be often overlooked by taking the own research environment and the presumed relevance for granted. Putting this to test upfront would avoid many frustrations in the later publishing process. This also holds for the theoretical grounding of every piece of research, so this is explored next.

Proposition 1: Do not take your research field for granted. Make sure, your wider research community would be interested in it and try to look at topics that will stimulate more research in the future.

### Theoretical Foundation

As expressed by Weick (1995), theory is constantly evolving. Even if we have achieved or confirmed certain operationalization,

there is always a next step. A simple illustration might be the value, rarity, immutability, and organization (VRIO) framework of the resource-based view (Barney, 1991), which are often taken up, but alone would not suffice to make a research contribution. As a brief example, this is reflected in the move to dynamic capabilities overcoming some of the limitations of the comparatively static VRIO framework (e.g., Gruchmann et al., 2019).

This links to the previously introduced four elements of theory; so here, the level of variables, constructs, and their interrelations is analyzed. A typical (sub-)question might be what kind of theory might be used or whether so-called grand theories, mid-range, or local ones might be taken up. This seems rarely discussed in sustainability management, the paper by Lozano et al. (2015) rather being an exception. As an example, Lozano et al. (2015) mentioned, e.g., the agency theory, the resource-based view, or stakeholder theory, so grand theories many researchers in sustainability management and other related fields would be familiar with. In a similar line of arguments, Spina et al. (2016) assessed the use of grand theories in purchasing and supplying management research, thereby pointing to, e.g., transaction cost economics, resource-based view, contingency theory, or game theory. Swanson et al. (2020) complemented such an approach, by pointing out that grand theories might be too general to explain certain mechanisms in detail. Stank et al. (2017, p. 7) provide characteristic features of middle-range theories, which offer good positioning also for sustainability and organizational and inter-organizational aspect-related research:

- Synthesize empirical findings that have emerged through research in a particular domain of knowledge
- Rely on a limited set of realistic assumptions appropriate for the focal domain
- Define concepts in a manner that is specific to the focal domain
- Restrict theoretical propositions regarding the relationships among concepts to the focal domain
- Make predictions that are specifically relevant to resolving theoretical and practical problems within the focal domain
- Provide a basis for potential linkages to more general theories that could potentially extend knowledge into other domains.

So, even a mid-range theory would comprise certain “accepted” variables or constructs, what need to be operationalized in conducting empirical research. This leaves open, whether a deductive, abductive, or inductive approach would be applied for doing so (see, e.g., Seuring et al., 2020). It might be allowed to say that the authors are quite open to all such approaches, while a paper following a typically deductive logic is often easier to comprehend and therefore easier to “sell” to reviewers and editors.

In line with the arguments already made on the empirical field, there is no right or wrong theory, but there is a sound justification for a choice of theory. Extending boundaries (Busse et al., 2017) or applying or borrowing theories to new fields (Gold,

2014; Stank et al., 2017) is often fruitful. The typical “so-what” question would still apply, requiring a justification for selecting the respective theory.

A note goes to the need for fit among empirical field or better the unit of analysis and the theory chosen. This also needs to fit to each other. This might be an issue warranting more debate, as, e.g., the dynamic capability approach has been applied to green transformation of companies (Da Giau et al., 2020), social enterprises (Ince and Hahn, 2020), sustainable supply chain management (e.g., Beske et al., 2014; Gruchmann et al., 2019), and sustainable innovation (Inigo and Albareda, 2019). This inherently argues that dynamic capabilities, a theoretical framing developed for single companies, can be applied to the supply chain level.

Proposition 2: Carefully consider your theoretical foundation. Be aware what the core constructs or variables to analyze in your research would be. Do not easily say there is no research on the topic yet.

## Methodological Choice

What has just been said for the choice of theory and early been highlighted for the empirical field also holds for the choice of an empirical (or analytical or experimental) method. The core aspect is a sound justification and being aware of the strength and weaknesses of a certain method. Many researchers are open to different research methods and methodologies, and often, they complement each other (Seuring, 2012).

A first point is typically sample size, which has to be determined for a questionnaire-based survey as much as for a number of experts in a Delphi study design or interviews for data collection in case study-based research. Beyond, there are hardly further points to be made on the research methodology, which are not already explained in detail in typical textbooks (e.g., Saunders et al., 2019).

Proposition 3: Be aware of the strength and weaknesses of the (empirical) method chosen. Start with a challenging plan, as there cannot be too much data.

## The Interplay of Empirical Field and Method

This leads to an interesting intersection with the empirical field that we briefly like to illustrate. Research in base-of-the-pyramid environments might find it hard to collect data with tools highly accepted in developed countries. Companies in such an environment might see researchers rather skeptical and mistrust them, which could be a consequence of governance issues at large, but hampering related data collection, which might only be conducted in personal interviews, thereby limiting sample size (e.g., Khalid et al., 2020). Of course, this limitation is hard to justify and might be rejected by reviewers. In this way, we might not be able to collect related data and miss out more inclusive research. A further aspect might be illiteracy of interviewees and the lack of trust, needing both personal contact as well as intermediaries (e.g., Yawar and Kauppi, 2018; Brix-Asala and Seuring, 2020). So, the empirical field to a certain extent determines the choice of method. Yet, turning the argument round an attempt of rather being able to conduct survey base

research in such base-of-the-pyramid environments would be very welcome, thereby moving to more theory testing approaches that seem to be well-justified on a topic coming to a certain level of maturity. Returning to the previous statement on data from different countries and contexts, this will be very welcoming, if combined with a strong theoretical foundation.

Proposition 4: Is the typical unit of analysis employed in the research method applicable to the empirical field? Does the field allow access to informants that are required both in the quantity (response rate) as quality of information needed?

## The Interplay of Empirical Field and Theory

It would be hard to argue in a similar manner about empirical field and theory. A core point might be that pure replication studies are often hard to publish in more organizational and management-related research, while they are of key importance, such as evident, e.g., in medicine. This might somewhat be a critical issue of our field of research, which can hardly be resolved by the individual researcher or single piece of research. As mentioned before, there is hardly a perfect choice, but a strong and convincing justification of choices being made, keeping the already mentioned boundary conditions (Busse et al., 2017) in mind and making sure that the unit of analysis in the field matches the typical theoretical approach. An example can be given at the example of transaction cost theory, where the unit of analysis is the single transaction (Williamson, 2008). Relating this to the four elements of theory introduced earlier, respective definitions taken up from the theoretical side must find their equivalent in the empirical field and should be applicable in the respective context. The boundaries of the theory and its application have to fit the empirical field or the other way around. It has to be checked, whether the empirical field can be analyzed with the particular theory. Extending the theory to a new empirical field might be possible, but might have to be argued for in a careful manner.

Variables or constructs need to be meaningful, which relates to face validity in the empirical research. Farmers, managers, or consumers interviewed must make sense in the eyes of the researched upon. This does not imply that the person responding to research questions would have to comprehend every part of the theory. They have to be able to respond in a meaningful manner. Particularly in research environments, where less formally educated people might serve as informants, such as the growing body of research on base of the pyramid environments (see, e.g., Rehman et al., 2020), this might impose challenges to data collection (Khalid et al., 2020). Still, even theoretical approaches such as institutional voids can still be studied gaining insights and allowing to develop it further (e.g., Brix-Asala and Seuring, 2020). One practical challenge would be that researchers have to avoid being arrogant on their field of research and treat any respondent with respect. This should then allow to draw conclusions and propose predictions feeding back into the empirical context and allowing to make a contribution both on the practical as well as the theoretical side.

Proposition 5: As a thought experiment, think of what the application of the theory chosen to the empirical field might yield? Check, that the theory is applicable to the field,

so the conceptual boundaries relate to each other. What expected outcomes might the research yield driving the theory development forward?

## The Interplay of Theory and Method

While the intersection of theory and method is highly important for a strong contribution, it seems to be close to impossible to propose clear advice. In line with the aspects already mentioned, just replicating what others have done is often seen as highly critical. A particular challenge is emerging if established constructs are taken up from already published research. Even if these papers were published in highly reputed journals, just borrowing several constructs from one paper and others from a second one for creating a new survey might not do the job. A stronger interrelation or interaction among theory and method would be required. This is mentioned while avoiding to be simplistic and providing a “cookbook” solution. This holds for quantitative as much as qualitative research (e.g., Gehman et al., 2018).

One note goes to the fact that this would change over time, such as illustrated at the example of sustainable supply chain management (Seuring, 2012). If a topic newly emerges, not much empirical data might be around. Then, even some initial case-based research might be a great insight already, providing thick descriptions of emerging phenomena. As the field matures, it would move to other methods, such as survey and more detailed insights on the interaction of certain constructs in the field. This can then be summarized, e.g., in a meta-analysis (Golicic and Smith, 2013). Agreeing with Carter and Washispack (2018), at such a stage, yet another literature review or bibliometric analysis pointing to the most cited papers or authors in the field would not make much a contribution anymore (e.g., Nimsai et al., 2020). Hence, rather in-depth analysis of certain topics in detail and stronger grounding in a theoretical base would be required. Summing up a field and providing a sound contribution would be a typical demand, such as, e.g., given in the paper by Reike et al. (2018) on the circular economy, where related activities are conceptualized into 10Rs, i.e., from re-fuse to re-mine. A second example, staying with the Circular Economy topic is the link to business models, such as systemized by Lüdeke-Freund et al. (2019). This then leads to interesting intersections, such as the one with sustainability assessment methods (Walzberg et al., 2021) or sustainable supply chain management (Genovese et al., 2017).

At the moment, the intersection of information technology, operations, and sustainability might be such a new field (Bai et al., 2020; Liu et al., 2020). Such topics might justify more conceptual analysis, such as Saberi et al. (2019) on Blockchains in supply chains, but would certainly also benefit from data collected in the field. In this respect, the interplay of theory and method develops over time (e.g., Seuring, 2012; or see the editorial by Boyer and Swink, 2008).

Proposition 6: Assess the intersection of theory and method. Does this promise to match each other and yield insights driving theory development forward?

Reflecting on the six propositions should allow to link empirical field, method, and theory to each other, so that the hint given in proposition 6 would also relate to the intersection of all three topics.

## CHALLENGING JOURNAL QUALITY AND RESEARCH IMPACT

Much of this discussion paper pointed toward publishing in top journals, while avoiding to clarify this term itself. There is much debate on journal quality and journal rankings, which is only mentioned to guide the interested researcher further into the topic. Grey (2010, p. 683) argued that “the constitution of journals as ‘top journals’ is clearly an accomplishment of power. There is a circularity, in which to publish in the ‘best’ journals, one must produce the ‘right kind’ of work.” As a consequence, PhD students sometimes limit their research choice by what might have a chance to be published in the “right” journal. This can be quite critical and might not trigger really interesting research, but rather confirm what we know already and follow in the already beaten path. This has a lot to do with how research performance is measured, an issue also attracting increasing attention (e.g., Aguinis et al., 2020).

It is admitted that this discussion paper takes a single-sided research-driven perspective. Hence, a second brief note is made on the point that this can be comprehended quite differently. As one example, e.g., Nicholls-Nixon et al. (2011) point out that many scholars in Latin America have advocated practical impact as their political, economic, and social contexts suffer from institutional voids and related uncertainties. In line with this, the survey of Bartunek et al. (2006) asked what makes management research interesting. Specifically, they compared the reasons for rating an article as interesting following the perspective of Revista de Administração de Empresas (RAE) and Academy of Management Journal (AMJ) board members. For RAE members, it was found that impact (including practical impact) was most important. The overlap between the perspective of AMJ and RAE board members concerns research quality, including well-crafted theory, good technical or method jobs, etc. The authors conclude that this variance of results points to the likelihood that readers in different parts of the world have diverse criteria for scholarly interest. There is no single conventional norm to which all scholars should ascribe in common terms and with mutual understanding, but rather a multi-vocality of aspirations in doing research. This should be kept in mind developing and evaluating research in the way it is promoted in this paper.

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## CONCLUSION

The starting point for this discussion paper is the creation of strong research questions or expressing it differently making sound choice on theory, method, and empirical field. While the single topics can already be challenging for themselves, strong research builds on a sound choice and justification of their interplay. The starting observation that this is often taken too easy and a sound planning of a respective research process often neglected results from interaction with many researchers and students in different contexts.

The paper alone will not be able to address all issues and provide detailed guidelines. There is more hope that pointing to the necessity for a sound interplay would make researchers be aware of their choice and drive them to (a) put more time into the research plan and (b) offer better justification in their later writing.

We may be allowed a last word and a kind of a warning. In some cases, this paper has pointed to several examples from research in a very brief manner. This should usually be avoided, as it does not offer a deeper analysis and links it to the overall debate in the paper. In this paper, such references serve an illustrative basis, connecting the arguments made to the wider literature in organizational sustainability. In this way, the discussion paper partly does wrong what it aims to criticize. So allow us the encouraging words: Keep writing just better.

## DATA AVAILABILITY STATEMENT

The original contributions presented in the study are included in the article/supplementary material, further inquiries can be directed to the corresponding author/s.

## AUTHOR CONTRIBUTIONS

SS: Conceptualizing and writing the paper. TS and MS: reviewing and improving the paper, helping to develop the ideas upfront and pointing to the need for it. All authors: contributed to the article and approved the submitted version.

## ACKNOWLEDGMENTS

We are thankful to Sadaf Aman, Felipe Alexandre de Lima, Lara Schilling, and Jayani Sudusinghe, all at the Chair of Supply Chain Management, University of Kassel, Kassel, Germany, for their critical feedback on the paper.

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**Conflict of Interest:** The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

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