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**Studying Configurations with QCA:
Best Practices in Strategy and Organization Research**

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Abstract

Qualitative Comparative Analysis (QCA) is increasingly applied in strategy and organization research. The main purpose of our essay is to support this growing community of QCA scholars by identifying best practices that can help guide researchers through the key stages of a QCA empirical study (model building, sampling, calibration, data analysis, reporting and interpretation of findings) and by providing examples of such practices drawn from strategy and organization studies. Coupled with this main purpose, we respond to Miller's (2017) essay on configuration research by highlighting our points of agreement regarding his recommendations for configurational research and by addressing some of his concerns regarding QCA. Our article thus contributes to configurational research by articulating how to leverage QCA for enriching configurational theories of strategy and organization.

Introduction

Configurational theorizing has a rich tradition in strategy and organization studies. While a first wave of configurational work has relied primarily on conceptual typologies and cluster analyses (e.g., Miller and Friesen, 1978; 1984; Miles and Snow, 1978; Meyer et al., 1993), the introduction of Qualitative Comparative Analysis (QCA) has led to a new wave of ‘neo-configurational’ studies that explicitly embrace causal complexity (Misangyi et al., 2017) and address the mismatch between theory and methods that had impaired earlier configurational theorizing (Fiss, 2007).

QCA-driven neo-configurational studies are also increasingly attracting the attention of strategy scholars who have pioneered the study of configurations (e.g., Ketchen, 2013). Miller (2017)’s essay is an example of this increasing attention and a welcome opportunity to continue a productive scholarly dialogue to enhance configurational theories of strategy and organization. In this spirit, the key objective of this article is to offer a set of best practices for high-quality QCA studies in strategy and organization research. Prior to laying out such best practices, in the next section we respond to Miller’s essay and address some of his concerns and clarify some misunderstandings regarding QCA.

Response to Miller (2017)

We share Miller’s spirit of aiming to enhance and promote a configurational understanding of organizational and strategy phenomena underlying both earlier configurational approaches and the neo-configurational perspective (e.g., Ketchen, 2013; Meyer et al. 1993; Grandori & Furnari, 2013). As we have recently argued together with colleagues (Misangyi et al., 2017), we see the resurgence of configurational research in management studies (also observed by Miller) as the emergence of a neo-configurational

perspective driven by a set-analytic approach¹. We also agree with Miller's (2017: 13) specific goal that we should strive to identify "richer, more full-blown organizational configurations" that shed new light on how organizations function", although we may differ in the path we draw towards that state, as we will illustrate in detail below. We also mostly agree with Miller's recommendations for strengthening empirical research on configurations, which include using theory to guide research, characterizing configurations richly, establishing the significance of configurations, checking robustness, studying transitions or evolutionary paths, and blending quantitative and qualitative analysis. Indeed, we think these recommendations hold for social science research generally.

Miller also points to some of the current shortcomings of QCA research. For instance, we concur that large-N QCA studies would benefit greatly from "access to qualitative data and attention to the causal pattern within particular configurations" (2017: 8). We have made this point in our earlier work (e.g., Greckhamer et al., 2013; Misangyi et al., 2017), although we also contend that "an iterative process between the findings and returning to empirical cases can prove to be fruitful in large-N settings [...] even without the intimate case knowledge typical of the small-N QCA approach" (Misangyi et al., 2017: 267; see also Ragin & Fiss, 2017). Likewise, Miller is correct that there are examples of large-N QCA studies in which the identified configurations account for a relatively small fraction of the sample studied i.e. 'low coverage' in QCA terminology. However, such examples are not indicative of theoretical or methodological problems with QCA but rather suggest that these studies' configurational models do not fully capture the complex causality underlying outcomes, possibly due to the lack of configurational theories in our field.

¹ Unlike Miller, however, we argue that this neo-configurational perspective is "scale-free" because it applies not only to the organizational level but also to phenomena at the intra-organizational and supra-organizational levels (Misangyi et al., 2017).

While we concur with Miller's overall intent, we believe there is a need to clarify several key aspects of his characterization of QCA. We will focus on three key concerns. Our first concern regards Miller's claim about QCA's inability to handle fine-grained data. He notes that QCA is applicable mainly when the data are "binary or ordinal characteristics," arguing that "QCA is more challenging to implement in conditions that demand more fine-grained scaling..." (: 8). It is important to note that this is not the case because QCA is not restricted to binary or ordinal characteristics, nor does it require researchers to bifurcate quantitative variables. Fuzzy sets, which allow very fine-grained analyses,² have been in use for almost two decades. As Ragin (2000) demonstrates, properly calibrated fuzzy sets combine variables' precision and explicit measurement with meaningful qualitative thresholds based on theoretical and substantive knowledge (we elaborate on best practices for calibration below).

Our second concern refers to Miller's contention that clustering approaches are superior when samples are large. However, the key distinction between cluster analysis and QCA relates to the alternative research questions that they each can answer. Hence, the relative superiority of one approach over the other depends on a study's focus, rather than to its' sample size. Cluster analysis aims to answer questions such as "what cases are more similar to each other?" whereas QCA aims to answer questions such as "what configurations of attributes are associated with an outcome of interest?" Regarding sample size, large samples neither render cluster analysis more meaningful nor they limit the applications of applying QCA. While QCA was originally designed for relatively small-N samples³ (e.g., Ragin, 1987), it has developed into a well-suited tool to analyze large-N samples

² While Miller acknowledges the existence of fuzzy sets in a footnote, his suggestion that they are challenging to implement is not warranted. Below we discuss best practices that researchers can follow to successfully execute QCA studies using fuzzy sets.

³ We distinguish between small-N QCA studies comprising about 12-50 cases, and large-N QCA studies comprising above 50 cases (Greckhamer et. al., 2013).

(Greckhamer et. al., 2013). More generally, we note the well-documented limitations of clustering techniques to derive meaningful results (e.g., Ketchen & Shook, 1996; Fiss, 2007), including extensive reliance on researcher judgement, the lack of test statistics, as well as the results' strong dependence on sample selection, on scaling of variables, and on the similarity measure and clustering method chosen.

Our third main concern regards Miller's suggestion that clustering approaches are more useful for identifying thematic patterns or "orchestrating themes." Clustering techniques per se provide little insight into why certain variables go together. In contrast, in QCA theory and substantive knowledge about a phenomenon are considered the starting points for researchers to build a well-specified configurational model and calibrate its constitutive elements (Ragin, 1987; 2000, 2008; Fiss, 2007; Fiss et al., 2013; Misangyi et al., 2017). Moreover, QCA and clustering techniques differ in that "While both approaches work with multidimensional spaces, QCA addresses the positioning of cases in these spaces via set theoretic operations while CA relies on geometric distance measures and concepts of variance minimization (Cooper & Glasser, 2011: 32). In addition, as Fiss (2011) demonstrated in his evaluation of the insights of Miles & Snow's (1978) typology by explicitly comparing QCA with cluster analysis, deviation scores, and interaction effects, QCA can handle causal complexity at a fine-grained level and enables researchers to unpack situations of first and second-order equifinality, substitution, or complementary effects between elements.

Best practices for empirical research using QCA

In the spirit of advancing configurational research, we now outline a set of best practices for conducting high-quality QCA research as a basis for developing configurational theories that are conceptually meaningful, empirically fine-grained and analytically rigorous. We would like to emphasize, however, that these best practices *should not be applied in a*

mechanistic manner and they cannot substitute for configurational theorizing at the outset of a study.

We organize these best practices by the sequential stages of a typical QCA research study (see Table 1)⁴ and offer examples of each best practice from strategy and organization research. Our aim is not to review QCA methodological principles that are discussed elsewhere (e.g., Ragin, 1987; 2000; 2008; Rihoux & Ragin, 2009; Schneider & Wagemann, 2012), but rather to provide a practice-oriented, hands-on guide to produce high-quality QCA research for strategy and organizations research.⁵

Insert Table 1 about here

Building the configurational model

High-quality QCA studies involve building theoretically sound configurational models, which begins with clearly defining the phenomenon or outcome to be explained. The selection of conditions expected to explain the outcome should be guided by theory or case knowledge and may involve an iterative process of model building and analysis, particularly in studies emphasizing theory building. While there are different ways of using theory to identify conditions (e.g. Amenta & Poulsen, 1994; Rihoux & Ragin, 2009: 19-32), key is the articulation of a *configurational rationale* for including conditions and theorizing their joint (rather than net) effects on the outcome. Sometimes, a configurational rationale is readily embedded in extant theories. For example, in their configurational model of business unit performance, Greckhamer and colleagues (2008) justify the inclusion of industry, corporate, and business-unit conditions based on prior theory regarding their interdependence.

⁴ In practice, empirical QCA studies may involve multiple iterations among these stages, so Table 1 is a simplified representation of this process.

⁵ Our intent is similar to Schneider & Wagemann's (2010), although we focus on standards of good practice accepted in strategy and organizational research.

However, because dominant correlation-based approaches have channelled theory-building efforts towards conceptions of independent, additive, and symmetrical causality (Delbridge & Fiss, 2013), for many outcomes configurational theories may not be readily available and researchers may need to develop configurational arguments to justify why conditions should be considered in conjunction to explain the outcome. For example, Grandori and Furnari (2008) justify the inclusion of four different types of organizational elements (market-based, bureaucratic, communitarian, democratic) by integrating different theories that had previously highlighted each of these elements in isolation. In yet other settings, researchers may leverage qualitative case studies and case-based knowledge to articulate a preliminary configurational model of how different conditions may interact and bring about the outcome (e.g., Aversa et al., 2015).

A key consideration in building a configurational model is the *number* of included conditions. It is good practice to both consider the maximum number of conditions that can be included based on a study's sample size and to keep the model parsimonious and non-redundant. Because an increasing number of conditions exponentially increases the number of logically possible configurations, it also increases the number of configurations that are likely to exhibit no cases. This problem of 'limited diversity' is inherent in virtually all social science data (Ragin, 1987). Fortunately, available guidelines help researchers to balance the number of conditions with the number of cases and the of the model's complexity (Marx, 2010). Even large-N QCA studies that may not readily face the problem of limited diversity in practice can include only a limited number of conditions because configurational models with many conditions may complicate findings' interpretation (Greckhamer et. al., 2013). One way to increase a model's parsimony is to combine several conditions into theoretically meaningful higher-order concepts. For example, Grandori and Furnari (2008) use theory to

aggregate lower-order organizational elements (e.g. individual-based incentives; firm-based incentives) into higher-order concepts (e.g., market-based elements).

Constructing the empirical sample

A standard of good practice in QCA studies is to sample cases *purposively*, using the outcome of interest to identify the population of cases. Purposive sampling has a long history in the case-oriented comparative tradition in which QCA is rooted and highlights that samples should be ‘theoretically defined’ (Ragin, 2008: 4) to ensure their relevance to a research question. For example, a researcher interested in understanding the factors of success of downsizing in large service firms would select large service firms that experienced downsizing. This sampling strategy may take however different forms depending on whether the QCA study is small-N or large-N (Greckhamer et al., 2013).

In small-N QCA studies, a sample of cases may be constituted by: 1) an entire population of cases relevant to explain the outcome (e.g., Aversa et al., 2015; Haxhi & Aguilera, 2017); 2) a sample of ‘representative cases’ from this larger population; 3) a combination of ‘positive cases’ that display the outcome and ‘negative cases’ that could be expected to display the outcome but do not (Ragin, 2000; Mahoney & Goertz, 2004). In small-N QCA studies it is also common practice to consider revising the sample by including additional cases to (or remove extant ones from) a sample based on theoretical grounds.

In large-N QCA studies, sampling should follow the logic of selecting theoretically relevant cases. This may mean selecting an entire population of cases relevant to explain an outcome (Misangyi & Acharya, 2014) or taking a stratified sample that well represents a population’s diversity of cases. Drawing a random sample may not be appropriate because generalizing findings to a population is warranted only when the sample represents the full

diversity of cases in the population and a random sample may not, for example, include rare configurations that are highly relevant for the outcome (Greckhamer et al. 2013).

Calibrating the data

QCA is a set theoretic method and both outcome and conditions are conceptualized as sets. Another key feature of high-quality QCA research therefore regards ‘calibration’, i.e. the process of determining cases’ membership in the sets representing the outcome and conditions (Ragin, 2008). QCA initially relied on a ‘crisp’ set approach (Ragin, 1987), which only distinguishes cases’ full membership and full non-membership into sets (i.e. ‘differences in kind’). However, Ragin (2000) expanded QCA to a fuzzy set approach, enabling researchers to also capture fine-grained differences in degrees of membership.

For both crisp and fuzzy sets, effective calibration is a *half-conceptual, half-empirical* process of identifying thresholds that meaningfully represent differences in kind and differences in degree among cases. This process should follow three principles: 1) to clearly define each set representing outcome and causal conditions (e.g., the set of large firms); 2) to use appropriate theoretical and substantive knowledge to identify sensible thresholds (or ‘anchors’) to determine e.g., which cases can be meaningfully considered to be fully in versus fully out the set of large firms in a given study setting; 3) to transparently report chosen thresholds, so readers can assess the validity and robustness of the calibration process and the resulting sets. These principles are essential for effective calibration of both qualitative (e.g. Crilly et al., 2012) and quantitative data (Misangyi & Acharya, 2014; see Misangyi et al., 2017).

For example, Misangyi and Acharya (2014) use a combination of theory and contextual knowledge to calibrate the membership of cases (firms) into sets of inside and outside director equity ownership. First, they used existing theory to define the dimensions of

‘director equity ownership’, identifying the amount of personal net worth a director invested in a firm as key dimension of this concept. Second, they used contextual data outside their sample to find the average net worth of directors in the USA and established a percentage thereof as meaningful thresholds for calibrating these sets.

This example demonstrates that calibration differs from *uncalibrated* measures in that uncalibrated measurement treats all variance equally while calibration identifies “whether the found variance corresponds to meaningful thresholds that distinguish differences in kind” among cases (Misangyi et al., 2017: 262). When criteria external to the study’s sample and theoretical knowledge to guide calibration are lacking, researchers may rely on expert panels or, at times as a last resort, use properties of the study’s sample (e.g., its cumulative data distribution or its frequency or density distribution) to determine thresholds that capture differences in kind and in degree among cases (e.g., Greckhamer, 2016). However, sample-based calibration should be avoided whenever possible.

Analyzing the data

While QCA entails various ways of analyzing set-subset relationships (Ragin & Fiss, 2017), a key tool is analysis of the truth table analysis using Boolean Algebra (Ragin, 2000, 2008). A truth table entails all logically possible configurations of conditions included in a study and contains 2^k rows (k = the number of conditions) each representing a specific configuration. Some logically possible configurations may be represented by relatively large proportions of cases, other configurations may be rare, yet others may not be represented by any cases in the sample (the latter are known as counterfactual configurations or logical remainders and are used in counterfactual analysis as discussed below). Consistency scores capture how consistently empirically observed configuration are linked to the outcome and thus they provide information regarding the model’s validity. Very low consistency scores

across configurations would suggest that the configurational model is a poor explanatory model for the outcome and should be reconsidered.

QCA typically aims to identify configurations of conditions that may cause an outcome (and its absence). Using set theory, QCA conceptualizes causality in terms of relations of necessity and sufficiency (Ragin, 2000; 2008). A configuration that is a consistent superset of the outcome (i.e., all occurrences of the outcome exhibit the configuration) indicates a situation consistent with necessity; a configuration that is a consistent subset of the outcome (i.e., all cases with a particular configuration display the outcome) indicates a situation consistent with sufficiency. QCA evaluates necessity and sufficiency relations through set-theoretic measures of consistency and coverage (Ragin, 2008), which serve analogous purposes of significance and effect sizes in regression analysis. *Consistency* measures “how closely a perfect subset relation [between a configuration and an outcome] is approximated” (Ragin, 2008: 44); in the simple case of crisp sets, consistency is the proportion of cases exhibiting the configuration that exhibit the outcome. *Coverage* gauges a configuration’s “empirical relevance or importance” (Ragin, 2008: 44); again for crisp sets this means the proportion of cases exhibiting the outcome captured by this configuration.

It is good practice to establish different consistency thresholds for necessity and sufficiency analyses and to not interpret subset relations that do not meet these thresholds. For necessity analysis, a consistency benchmark of at least $>.90$ is recommended, as is a high coverage measure to indicate that the potential necessary condition is empirically relevant (Ragin, 2008; Schneider & Wagemann, 2012; for an empirical example see Greckhamer, 2016). For sufficiency analysis, a fairly well established consistency benchmark is $\geq .80$ for raw consistency (Ragin, 2000; 2008). In fuzzy set analysis it is also important to consider PRI (proportional reduction in inconsistency) scores to avoid simultaneous subset relations of

configurations in both the outcome and its absence. PRI consistency scores should be high and ideally not too far from raw consistency scores (e.g., 0.7); configurations with PRI scores below 0.5 indicate significant inconsistency. In addition, researchers need to decide on a case frequency threshold for a configuration to be included in the sufficiency analysis. While in small-N QCA studies this threshold is typically one case, in large-N QCA studies setting this threshold implies a trade-off between more parsimonious findings and the inclusion of relatively rare configurations. While this trade-off may be approached differently depending upon research questions, it is recommended that a threshold is chosen that retains at least 80% of the cases (Greckhamer et. al., 2013), though preferably more. Finally, when theoretical rationales warrant the consideration of necessity relations, necessity analysis should be conducted prior to sufficiency analysis.

QCA allows researchers to make simplifying assumptions about counterfactual configurations and to assess how different simplifying assumptions impact the configurations found to be consistently sufficient for the outcome. Accordingly, another best practice concerns the transparent justification of simplifying assumptions included to distinguish between core and contributing⁶ conditions to enable readers to evaluate their plausibility (Soda and Furnari, 2012). Core conditions remain part of the solution when all simplifying assumptions are included, both those consistent with empirical evidence and theoretical knowledge (i.e., easy counterfactuals) and those consistent with empirical evidence but not with theoretical knowledge (i.e., difficult counterfactuals) (Ragin, 2008: 160-176)⁷. Thus, core conditions are “decisive causal ingredients” (Misangyi et al., 2017: 276) because they remain part of the solution even when assuming a state of the world in which difficult counterfactuals that are not supported by current theory occur (Soda and Furnari, 2012).

⁶ Contributing conditions may also be referred to as ‘peripheral’ conditions (e.g. Fiss, 2011).

⁷ QCA never includes simplifying assumptions that contradict empirical evidence.

Contributing conditions instead remain part of the solution when easy counterfactuals are included, but they are ‘stripped away’ from it by including difficult counterfactuals. To meaningfully distinguish and interpret core and contributing conditions, it is good practice to transparently report the assumptions included in the analysis as well as the theoretical rationales justifying their inclusion and plausibility (e.g., Greckhamer, 2011: 114-115).

It is also a good practice in QCA to analyze *separately* the configurations for the presence and the absence of an outcome. Being based on Boolean rather than Linear Algebra, QCA assumes that the occurrence of an outcome and its absence may be caused by different conditions (i.e., it assumes potential causal asymmetry). Put differently, the occurrence and the non-occurrence of an outcome may constitute two qualitatively different phenomena and it is good practice to provide separate explanations for them; they may potentially even require different causal models (Schneider & Wagemann, 2012).

Finally, we emphasize that data analysis in QCA—unless the focus is on theory-testing—is typically an iterative process, and considerations for consistency and coverage measures alongside considerations for model and thereby results parsimony can help researchers to refine their model. For example, analysis of an initial model may lead researchers to identify a number of contradictory configurations (i.e., configurations that entail both cases that show the outcome and the absence of the outcome) and, based on an examination of such cases and/or further theory building, researchers may re-constitute the population of interest by removing or adding cases, or they may include additional conditions that eliminate or reduce contradictions.

Evaluating the robustness of findings

As described above, as any empirical research every QCA study is ripe with decisions made by researchers (e.g., decisions in calibrating set membership, thresholds in data

analysis, iterative steps of model building and analysis, etc.). Hence, it is crucial to transparently report these decisions and their underlying rationales as well as to consider how they shape a study's findings (needless to say, transparent documentation of researchers' decisions is essential for all empirical research). At the same time, considerations of these findings' robustness should stay true to the logic of set-theoretic analysis rather than trying to mimic robustness tests in regression analyses (Schneider & Wagemann, 2012). QCA findings can be considered robust if slightly different decisions lead to similar enough findings in terms of necessity and sufficiency so that the paths identified and the consistency and coverage measures of fit do not warrant substantively different interpretations (Schneider & Wagemann, 2012).

Changes in calibration thresholds may change findings' consistency and coverage, but otherwise should not affect their substance (i.e., configurations identified in the solution). Because changes in the calibration of the cross-over point that identifies when cases are neither in nor out of a set (0.5 membership) may lead cases to shift from one to another row on the truth table and accordingly may change the patterns of empirically observed and unobserved configurations, this threshold should be chosen carefully. For example, Fiss (2011) evaluated the robustness of his findings by varying the cross-over point for causal conditions for which alternative cross-over points appeared plausible. Changes in the minimum consistency and PRI score thresholds also shape the results in that an increase (decrease) in these thresholds will lead to new solutions that are more (less) consistent and have lower (higher) coverage. Similar patterns hold for varying the frequency threshold for truth table rows. Relatedly, researchers should evaluate how any included simplifying assumptions may alter the core and contributing conditions identified. It is also usually recommended to report or discuss results based on multiple consistency thresholds (see e.g. Ragin & Fiss, 2017).

Finally, it is important to note that adding conditions to a configurational model is unlike adding control variables in regression models. The inclusion of new conditions changes the logically possible (and thus the empirically observed) configurations of conditions and thus will likely change the findings. Thus, altering the configurational model should be part of a potentially iterative and theory-guided process of building the configurational model, unless the study's goal is theory testing.

Reporting and interpreting findings

Several best practices facilitate the reporting of QCA results. First, for transparency it is desirable to represent the (limited) diversity of cases by reporting the truth table (e.g., Greckhamer, 2011; Garcia-Castro et al., 2013). However, because a truth table's complexity increases exponentially with the complexity of the analyzed model, reporting it within the constraints of a journal article may be challenging. Garcia-Castro and colleagues (2013) address this challenge by producing 'nested' truth tables for the firms they analyze. Alternatively, providing truth tables as online supplements to journal articles may be practical.

The current best practice to represent findings from sufficiency analyses in management studies is to follow the 'configuration chart' notation system introduced by Ragin and Fiss (2008), which displays the equifinal configurations consistently linked to an outcome⁸. Greckhamer (2016) has suggested a way to integrate necessity analysis results into this notation system. Additionally, the use of Venn diagrams allows researchers to combine information about consistency levels while mapping the configurations associated with an outcome (e.g., Ragin & Fiss, 2017).

⁸ A complete Excel template for creating configuration charts is available at <http://www-bcf.usc.edu/~fiss/stm%20links.html>

Another key good practice involves reporting the consistency and coverage scores to make sense of and interpret QCA results in terms of validity and empirical relevance of the overall solution and of the individual configurations included in it. In addition, reporting unique coverage scores of each configuration enables interpretation of the proportion of cases covered *uniquely* by this configuration (whereas the raw coverage score includes cases covered by multiple configurations). Comparisons of raw and unique coverages indicates the extent of overlapping versus neatly separated configurations (see Aversa et al., 2015: 665). Taken together, these parameters provide fine-grained ways to not only interpret the causal complexity underlying the outcome but also to distinguish the importance and validity of each of the equifinal configurations identified, which is a key advantage of QCA vis-à-vis cluster analysis or other correlational methods.

To interpret QCA findings, whenever possible, researchers should return to case data in order to facilitate configurational theory building through case-level analyses. This may help researchers to interpret the essence of a configuration and to potentially capture it through a descriptive label. Small-N QCA studies may readily involve in-depth data about cases that can support and qualify the QCA findings through qualitative analysis. For example, Aversa and colleagues (2015) complement their QCA analysis of high- and low-performing business model configurations with in-depth analyses of two polar cases selected to explore the mechanisms underlying the identified configurations (for a large-N example of a similar process see Dwivedi et al., 2018).

More generally, formal criteria can guide the selection of different types of cases (e.g., deviant, conforming) to help interpret QCA findings (e.g., Schneider & Rohlfing, 2016). Even in large-N QCA studies, when in-depth knowledge about cases is more difficult to access, researchers should try to analyze and compare cases that represent different configurations consistently linked to the outcome to build additional insights. From its very

origin, QCA embraces the idea that researchers should establish ‘intimacy’ with the cases and complement cross-case comparisons with analyses of individual cases (Ragin, 1987). One way to do so is to identify specific cases that are covered by each configuration in the solution, and to evaluate and report the extent to which they are proto-typical rather than deviant cases vis-à-vis a given configuration.

Conclusion

Configurational theorizing should be the foundation of any QCA analysis to avoid the mechanistic deployment of its technique. QCA studies following the best practices outlined above enable the development of neo-configurational strategy and organization theories that are conceptually meaningful, empirically fine-grained and analytically rigorous. Such configurational theories are likely to be conceptually robust and useful because they are rooted in data calibrated on the basis of case-based and theoretical knowledge, which facilitates the interpretation of the configurations identified via QCA. At the same time, these QCA studies can analyze fine-grained data to study causal complexity with analytical rigor because they are built on the analytical apparatus of set theory, Boolean Algebra, and their associated tools (e.g., truth table, consistency and coverage scores, set coincidence, etc.). In short, we have shown that QCA is well positioned to effectively tackle research questions regarding what configurations of factors are associated with a given outcome of interest by striking a delicate balance between complexity and parsimony, meaningful detail and analytical precision. The conceptual and methodological thinking around configurational research supported by QCA has evolved tremendously in recent years and we strive to continuously advance it by clarifying misunderstandings and by offering guidelines that enable high standard research and significant insights.

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Table 1: Best practices in QCA studies in strategy and organization research

Research design stage	Best practice	Studies featuring the best practice	Goal achieved through the best practice
Building the configurational model	• Clearly define the outcome of interest • Select conditions based on extant theory or new theory rooted in case knowledge explaining why conditions should combine to produce the outcome • Consider the maximum number of conditions based on the number of cases in the sample	Grandori and Furnari (2008), Greckhamer et.al. (2008), Fiss (2011) for deductive model building Aversa et al. (2015), Bromley et al. (2012) for inductive model building based on case studies Marx (2010) for benchmarks on the number of conditions	To meaningfully specify the type and number of conditions included in QCA
Constructing the empirical sample	• Construct a theoretically relevant sample to study the outcome	Greckhamer et al. (2013) on small-N vs large-N sampling Haxhi & Aguilera (2017) on small-N sampling	To specify the sample's relevance to the research question and the study's boundary conditions
Calibrating the data	• Conceptualize included conditions as sets based on theoretical knowledge	Crilly et al. (2012) for calibration of	To clearly define sets included in a study's model

	• Calibrate cases into sets on based on theoretical and contextual knowledge	qualitative data Greckhamer (2016), Misangyi & Acharya, (2014) for calibration of quantitative data	To focus on theoretically relevant variance by capturing meaningful differences in kind (i.e., crisp sets) or differences in kind and degree (i.e., fuzzy sets)
Analyzing the data	• Explore and interrogate the truth table ○ Specify raw consistency thresholds for necessity and sufficiency analyses (and for fuzzy sets, PRI score thresholds for sufficiency analysis) ○ Specify frequency threshold for sufficiency analysis ○ Specify simplifying assumptions based in theoretical knowledge (counterfactual analysis) • Analyze both conditions linked to the presence and the absence of the outcome (causal asymmetry) • Return to model specification if needed based on necessity and sufficiency analyses results	Fiss (2011), Greckhamer (2016) for examples of sufficiency and necessity analyses Greckhamer (2011), Soda and Furnari (2012) for counterfactual analysis	To identify (configurations of) conditions that may be necessary or sufficient for outcomes To show the empirical relevance of configurations consistently linked to outcomes To examine potential causal asymmetry To distinguish core and peripheral conditions in configurations consistently linked to outcomes
Evaluating the robustness of findings	• Evaluate the robustness of findings to changes in calibration anchors and changes in consistency and frequency minimum thresholds • Evaluate the robustness of findings to changes in simplifying assumptions based on existing theories	Garcia-Castro et al. (2013), Fiss (2011) for robustness of findings to calibration and thresholds	To evaluate the impact of researchers' decisions on findings and check the robustness of configurations identified via QCA

Reporting and interpreting findings	• Report results of necessity and sufficiency analysis, including consistency as well as raw and unique coverage scores of configurations • Report truth table or ‘nested’ truth tables • Report the extent of limited diversity (% of truth table rows with no cases) • Supplement configurational analysis with case-level analysis: ▪ In small-N QCA: by supporting QCA results by qualitative case analyses ▪ In large-N QCA: by analyzing and comparing cases representing different configurations consistently linked to outcomes of interest • Report evidence regarding robustness of findings (i.e., how results vary depending on changes in calibration thresholds, in consistency and frequency thresholds, and in simplifying assumptions)	Ragin and Fiss (2008) for reporting sufficiency analysis and Greckhamer (2016) for integrating necessity analysis into this format Aversa et al. (2015) for small-N QCA and going back to cases Garcia-Castro et al. (2013) for nested truth table, large-N, and reporting evidence for robustness Ragin & Fiss, 2017 for use of Venn diagrams to report solutions	To support interpretation of configurations consistently linked to outcomes To build configurational theory as to why attributes of configurations combine the way they do and why they are associated with certain outcomes

SHORT BIOS

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