Chapter 9
QCA and Its Critics

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We agree with the critics … that case knowledge should play a central role in set-theoretic research; and that set calibration is both crucial and improvable. We disagree whenever matters of current QCA practice are confounded with the method's principles, and when statements about QCA's viability and quality are based on misunderstandings about its inner working.

Ingo Rohlfing and Carsten Q. Schneider [1]

In the three decades since *The Comparative Method* (Ragin 1987), scholars and practitioners have recognized QCA as a valuable approach to comparative studies that helps to overcome the gulf between qualitative and quantitative research traditions. The considerable increase in the number of published studies and the dissemination across the social sciences – documented in Chapter 1 of this book – speak to the fact that QCA has become an established method in the social scientific toolbox (see also Marx et al. 2014; Rihoux et al. 2013).

Yet, since its founding, the method has also spurred a diverse array of critiques. I argue that this is precisely because of the method's *hybrid* nature – bringing together qualitative and quantitative elements – which propels criticism from scholars trained in statistical methods, who usually work with hundreds or thousands of observations, as well as from those who conduct intensive studies on a handful of cases at most. While not being limited to a certain number of observations, QCA typically operates with 20 to 50 cases, which means it is situated right in between these poles.[2]

Clearly, it is a sign of QCA's *maturity* as a social scientific method that the approach is being scrutinized and critically evaluated by scholars with diverse expertise and backgrounds. This is a welcome development because it fosters improvement of the method and its analytical routines, and it will deepen the understanding of the method. In that spirit, early critiques of

QCA have led to the introduction of fuzzy sets to overcome the dichotomization of the data (Ragin 2000), measures of fit as numerical indicators to assess the strength of set-theoretic relationships (Ragin 2006), standards of good practice for QCA applications (Schneider and Wagemann 2010), and many refinements of the analytical procedure, highlighted throughout this book, which relate both to the method's analytical protocol, including the treatment of logical remainders (Schneider and Wagemann 2013), and its implementation in various software solutions, particularly the R packages (Dușa 2019b; Oana and Schneider 2018; Oana et al. 2021), but also for other platforms and as stand-alone software (Cronqvist 2019; Drass and Ragin 1986; Longest and Vaisey 2008; Reichert and Rubinson 2014).[3] At the same time, QCA variants have been developed to address specific research needs (see Chapter 8).

The downside is that critiques are sometimes taken out of context and treated as matter of fact, rather than as contributions to ongoing conversations. The scope of the methodological exchanges has exacerbated the problem, making it challenging for new users to attain a full view of the arguments.[4] In this chapter, I aim to provide a concise and up-to-date summary of existing critiques and responses, and clarify some recurring misunderstandings about QCA. With this in mind, emphasis is placed on the broad contours of the exchanges, rather than providing detailed accounts of every contribution made so far. I organize the discussion around four persistent themes in the methodological debates: (1) analytical robustness, (2) comparisons with other methods, (3) formalization and algorithms, and (4) causal analysis and solution terms. The chapter closes with a summary on the strengths and limitations of QCA.

Analytical Robustness

Recently, a number of studies have run simulations with artificial data to assess the analytical robustness of QCA results, including its sensitivity to changes in the number of cases, the calibration of conditions, and the impact of potential measurement error on the analytical results (Arel-Bundock 2019; Hug 2013; Krogslund et al. 2015; Krogslund and Michel 2014; Lucas and Szatrowski 2014; Seawright 2014). While these studies differ in their research designs and the claims made, a common thread is that QCA is found to be prone to yield faulty conclusions because the findings are not stable when cases are removed from the analysis, when calibration thresholds are set differently, or when measurement error is taken into view.

Along those lines, Samuel Lucas and Alisa Szatrowski (2014) conclude from their simulations and a reanalysis of empirical data on space shuttle launches that preceded the *Challenger* accident, that "our pre-*Challenger* launch data reanalysis suggests that QCA studies likely reach faulty conclusions" (2014, 67). Vincent Arel-Bundock (2019) infers from his Monte Carlo simulations "that crisp set QCA algorithms can be very sensitive to measurement error" (2019, 16), and, similarly, Chris Krogslund, Donghyun Danny Choi, and Mathias Poertner (2015) argue that QCA results "are questionably robust to even small changes in its calibration and reduction parameters", hence they suggest that when calibrating conditions, QCA users should "first report results for a large number of different values of each calibration parameter" in order to help "convey the overall robustness of the results to the reader" (Krogslund et al. 2015, 38-40).

QCA proponents have responded to the simulation-based critiques in two ways. The first type of response has emphasized the importance of *case knowledge* and the roots of QCA as a *qualitative* method of inquiry – two characteristics that are essential to QCA but which tend to be overlooked when the method is treated solely as a data analysis technique (Olsen 2014; Ragin 2014).[5] Based on this reasoning, simulations are inherently flawed because they lack the crucial component – case knowledge – without which it is not possible to make informed decisions throughout the design of a QCA study, including the conceptualization and selection of conditions and their calibration, the analysis of the truth table, the treatment of logical remainders, and the substantive interpretation of the analytical findings. Wendy Olsen puts it concisely:

> Thus, the argument using artificial data […] is poorly constructed. It does not refute QCA, because no background knowledge can be obtained: these data do not reflect reality. (Olsen 2014, 102)

The second type of response comes from QCA proponents who, in principle, see merit in simulations with artificial data but who take issue with the way in which simulations have been conducted and the claims that have been made by QCA critics on this basis (Fiss et al. 2014; Rohlfing 2015; 2016; Rohlfing and Schneider 2014; Thiem 2014; Vaisey 2014).

For instance, Ingo Rohlfing (2015) concludes from his assessment of the simulations in Krogslund et al. (2015) that, while some of their results are confirmed, "salient aspects of QCA are not adequately captured" and that "QCA is more robust than [Krogslund et al.'s] simulations suggest" (Rohlfing 2015, 4). In a similar vein, Peer Fiss, Axel Marx, and Benoît Rihoux (2014) examine the simulations of Lucas and Szatrowski (2014) and arrive at the conclusion that "if based on a technically correct understanding of QCA, simulations confirm QCA's strength and analytical usefulness" (Fiss et al. 2014, 98). Finally, Stephen Vaisey (2014) also conducts a reanalysis of Lucas and Szatrowski (2014) and finds that "my analysis using the same data and program has QCA get the right answer" (Vaisey 2014, 108). Vaisey acknowledges that Lucas and Szatrowski (2014) identify problems of *naive* data analysis, which can lead to spurious results. However, he underlines that "straightforward workarounds" exist to mitigate such problems (Vaisey 2014, 110).

In sum, while the critical studies express some legitimate concerns about analytical robustness, their wholesale conclusions miss the mark. First, it must be noted that the criticism on analytical sensitivity *applies to many social scientific methods*, including statistics, where the data and coding decisions naturally affect the results. Surely, dropping a single case from a regression should not have an observable effect, but adding or excluding a country or region with several hundred observations will certainly impact upon the results. For example, a study on the conflict behavior of democracies will yield different findings when using a threshold of 8, rather than 7 on the combined Polity IV autocracy-democracy scale. Likewise, the choice of conflict data and its coding will certainly affect the results.

Second, the simulations also miss the *case-based nature* of QCA. As this book has emphasized, case selection is a crucial part of research design and criteria must be explicitly justified and made transparent (Chapter 2). The processes of *casing* and case selection are essential to the research logic on which QCA is built (Ragin 1992; Ragin and Becker 1992). Good applications provide a thorough justification for their case selection. They should also discuss when the coding or interpretation of a particular case may be controversial, and hence they provide robustness tests by analyzing the data with and without the particular case. Rather than "anything goes", as some critics seem to imply, there will be a limited number of reasonable analytical options for decisions on the selection of cases, conditions, and their calibration. These decisions should be made explicit, so that others can scrutinize them and assess their plausibility. All of this is good QCA practice (Schneider et al. 2019; Schneider and Wagemann 2010; 2012; Wagemann and Schneider 2015).

That said, the above should not be taken to imply that analytical robustness is of no concern for QCA studies. To the contrary, the method's relative sensitivity to changes in the research design, including the selection of cases, conditions, and calibration criteria, calls for theoretical and substantive justification and the considerations that informed such decisions should always be made explicit. This mirrors the discussion of good research practice, as emphasized throughout this book, and specifically Chapter 2 on research design, and Chapter 5 on calibration. Beyond that, researchers have also developed approaches to address sources of error and uncertainty (Maggetti and Levi-Faur 2013; Skaaning 2011),[6] and specific tools to detect false positives (Braumoeller 2015) and false negatives in QCA results (Rohlfing 2018).

The Space Shuttle Launch Data

As mentioned above, the critique by Lucas and Szatrowski (2014), henceforth referred to as L&S, used historical space shuttle launch data to conduct an empirical test with QCA. The background to this is the Space Shuttle *Challenger* accident of January 1986, after which a commission was charged with analyzing the accident and identifying its causes (Rogers et al. 1986). The Rogers Commission concluded that "failure was due to a faulty design unacceptably sensitive to a number of factors. These factors were the effects of temperature, physical dimensions, the character of the material, the effects of reusability, processing, and reaction of the joint to dynamic loading" (Rogers et al. 1986, 73). Moreover, it was found that "Those who made that decision [to launch the space shuttle] were unaware […] of the initial written recommendation of the contractor *advising against the launch at temperatures below 53 degree Fahrenheit*" (Rogers et al. 1986, 83, emphasis added). The bottom line is that low temperature was identified "as a cause of danger independent of field and nozzle pressure" (Lucas and Szatrowski 2014, 15).

Against this backdrop, L&S used the Rogers data because it provided "a rare real-world data set of known causes [of O-ring erosion, as the identified cause of the accident], making them potentially useful for assessing QCA" (2014, 15). This data comprises 23 space shuttle launches between 1981 and 1986, with information on field pressure, nozzle pressure, temperature, and O-ring erosion. For their QCA analysis, L&S report a parsimonious solution that is "the interaction of high nozzle pressure and low temperature", from which they surmise that "QCA finds that temperature *has no independent effect*, posing a danger only when low while field and nozzle pressure are high", which lets them conclude that "QCA *clearly fails* in real-world testing" (2014, 15, emphasis added).

For his reply to L&S, Charles Ragin (2014) reanalyzed the shuttle launch data with QCA. His analysis provided results that were in line with what is known about the accident's causes and which depart from those reported by L&S. Table 9.1 shows the raw data from Rogers et al. (1986), the calibrated fuzzy-set conditions, and the crisp-set outcome, based on the calibration thresholds stated in L&S and Ragin (2014). In my own reanalysis, I follow the calibration criteria of L&S (2014: 15) to create two pressure conditions, *high field pressure* and *high nozzle pressure* (fully out = 50, cross-over = 125, fully in = 200). However, as Ragin notes, in order to attain a meaningful set *low temperature*, the empirical anchors must be set differently than specified by L&S (fully out = 65°F, cross-over = 60°F, fully in = 45°F). This is because the calibration criteria for low temperature stated in L&S (2014, 15) yield a truth table without any rows that meet the minimum consistency threshold of 0.75. Hence, for my reanalysis, I adopt Ragin's anchors for the low temperature condition (2014, 91).

Table 9.1 Space Shuttle Data, Raw and Calibrated

Case	Space Shuttle flight	Raw data			Calibrated fuzzy sets			Outcome
		Field pressure	Nozzle pressure	Temp. °F	High field pressure	High nozzle pressure	Low temp.	O-ring erosion
1	STS-1	50	50	66	0.05	0.05	0.03	0
2	STS-2	50	50	70	0.05	0.05	0.00	1
3	STS-3	50	50	80	0.05	0.05	0.00	0
4	STS-5	50	50	68	0.05	0.05	0.01	0
5	STS-6	50	50	67	0.05	0.05	0.02	0
6	STS-7	50	50	72	0.05	0.05	0.00	0
7	STS-8	100	50	73	0.27	0.05	0.00	0
8	STS-9	100	100	70	0.27	0.27	0.00	0
9	STS-41-B	200	100	57	0.95	0.27	0.64	1
10	STS-41-C	200	100	63	0.95	0.27	0.15	1
11	STS-41-D	200	100	70	0.95	0.27	0.00	1
12	STS-41-G	200	100	67	0.95	0.27	0.02	0
13	STS-51-A	200	100	67	0.95	0.27	0.02	0
14	STS-51-C	200	100	53	0.95	0.27	0.80	1
15	STS-51-D	200	200	67	0.95	0.95	0.02	1
16	STS-51-B	200	100	75	0.95	0.27	0.00	1
17	STS-51-G	200	200	70	0.95	0.95	0.00	1
18	STS-51-F	200	200	81	0.95	0.95	0.00	0
19	STS-51-I	200	200	76	0.95	0.95	0.00	1
20	STS-51-J	200	200	79	0.95	0.95	0.00	0
21	STS-61-A	200	200	75	0.95	0.95	0.00	1
22	STS-61-B	200	200	76	0.95	0.95	0.00	1
23	STS-61-C	200	200	58	0.95	0.95	0.60	1

Data source: Rogers et al. (1986: 130-131).

As shown in Table 9.2, the parsimonious solution *correctly* identifies low temperature as a sufficient condition for O-ring erosion – a cause that, ultimately, led to the space shuttle accident. The conservative solution further entails high field pressure as a contributing cause. This simply means that the data offers no empirical basis to rule out high field pressure as a potential cause, because there is no case with low temperature and low field pressure among the observed cases (Ragin 2014, 91). Moreover, the low coverage values of the QCA solutions indicate that low temperature is but one of several pathways to O-ring erosion. As highlighted in the above quote from the commission report (Rogers et al. 1986, 73), other factors potentially contributed to the fatal launch. To put it differently, O-ring erosion also occurred in the *absence* of low temperature. In fact, 9 out of 12 cases of O-ring erosion happened under *not*-low temperatures (compare Table 9.1). This is not discussed by L&S, but a case-oriented investigation would start from this observation to identify other relevant conditions and thus enhance the coverage of the QCA study, to eventually provide a full account of the conditions under which O-ring erosion occurs in space shuttles. Remarkably, the regression analysis reported by Ragin shows no significant effect for *any* of the expected causes (Ragin 2014, 92).

What this example demonstrates is that – contrary to the claims made in L&S – QCA *does* return meaningful analytical results when it is used appropriately. But this requires a sensible calibration and directionality of the target conditions and an analysis that is conducted in line with established standards of good research practice. When using the calibration criteria for low temperature as they are described in Lucas and Szatrowski (2014, 15), then the analysis results in a truth table without rows that meet the consistency threshold of 0.75. Once this error in the calibration is corrected, the analysis properly identifies the known cause for the shuttle failure, as described in the Rogers commission report (Rogers et al. 1986). Moreover, the QCA analysis appropriately highlight *causal complexity*, rather than focusing solely on low temperature (which is one of *several* relevant factors examined and identified in the Rogers report). Hence, it is clear that *multiple pathways* in the Rogers data led towards shuttle failure and that low temperature alone accounts for just a small part of the outcome. QCA researchers would take this as a starting point to gain a more complete explanation for the phenomenon.

Table 9.2 Conservative and Parsimonious Solutions

Conservative solution:	Low temperature × high field pressure ⟶ O-ring erosion (Consistency 0.961, coverage 0.184)
Parsimonious solution:	Low temperature ⟶ O-ring erosion (Consistency 0.961, coverage 0.184)

Comparisons with Other Methods

A recurrent theme in the methodological debates about QCA is its comparative assessment vis-à-vis other methods of inquiry. Most of the contributions have focused on comparing QCA to quantitative tools, such as regression analysis and related approaches (Clarke 2020; Lieberson 2004; Paine 2016a; 2016b; Seawright 2005a; 2005b; Tanner 2014), but there have also been comparisons with qualitative research methods such as process tracing and comparative case studies (George and Bennett 2005; Munck 2016), and a growing literature emphasizes the complementarity of QCA and process tracing (Beach et al. 2019; Beach and Rohlfing 2018; Goertz 2017; Meegdenburg and Mello forthcoming; Rohlfing and Schneider 2018; Schneider and Rohlfing 2019), as mentioned in the section on multi-method research in Chapter 2.

Writing from a quantitative perspective, Jason Seawright (2005b) sees QCA as a "major practical competitor" to statistical approaches for the purpose of cross-case inferences, but he concludes that, due to some of its more restrictive assumptions, "QCA is not an improvement over regression analysis" (2005b, 24). According to Seawright "scholars using QCA are reasonably close to employing a regression framework, and, in some respects, quite far from the case-study tradition" (2005a, 41). Hence, he argues that for QCA scholars, "embracing more elements of the regression tradition may therefore be compatible with what they are already doing" (Seawright 2005a, 41). A similar perspective is expressed by Stanley Lieberson (2004), who focuses on what he regards as QCA's major limitation, namely that it is a deterministic method without instruments to account for "chance and probabilistic processes" (2004, 13).

In two more recent contributions, Jack Paine (2016a; 2016b) formulates views similar to those of Seawright and Lieberson, suggesting that set-theoretic methods "share common foundations with quantitative research" but that these are neither an improvement over regression analysis nor over qualitative methods like process tracing (Paine 2016a, 28-29). Therefore, he argues that the "qualitative methodology would be better served by focusing on tools such as process tracing that do possess distinctive advantages relative to quantitative methods" (Paine 2016b, 798). Likewise, Gerardo Munck (2016, 5) suggests in a short comment on the method that the integration of process tracing into set-theoretics methods might provide them with a comparative advantage over regression analysis, while also arguing that process tracing "clashes with the idea of causal relations and logical relations" (Munck 2016, 5).

QCA scholars and those sympathetic to the method have engaged with these and related comments by lining out commonalities and differences as opposed to regression analysis, and by clarifying misconceptions about the principles of set-theoretic methods (Mahoney 2004; Ragin and Rihoux 2004a; 2004b; Rohlfing and Schneider 2014; Schneider 2016). In that vein, Ragin and Rihoux (2004b) highlight that "analyzable truth tables are not the starting point of comparative research; rather, they are formed near the end of a long process of case-oriented comparative investigation" (2004b, 22). Some of the criticism has also been overtaken by methodological developments. For instance, the debate surrounding probability and determinism (see also Chapter 2), as raised by Lieberson, has lost much of its relevance since the introduction of measures of fit to account for imperfect set relations (Chapter 6). Hence, "QCA is not a deterministic method", as Carsten Schneider and Claudius Wagemann (2012, 316) underline (see also Goertz 2005).

As for the comparison between regression analysis and QCA, Ragin and Rihoux emphasize that these methods are based on *different assumptions* and that they are rarely "competing for the same turf" (2004b, 22). For Gary Goertz and James Mahoney (2012), set-theoretic methods and statistical analyses belong to different *cultures*, with their own, internally-coherent scientific perspectives. Alrik Thiem, Michael Baumgartner, and Damien Bol (2016) go a step further in delineating *fundamental differences* in the mathematical and conceptual roots of these methods.

Irrespective of whether one agrees with these characterizations, there is a consensus among QCA scholars that set-theoretic methods *do not aim to replace* regression analysis or related approaches, contrary to what is insinuated in some contributions to the methodological debates. In fact, some work has made inroads into combining large-*N* QCA with regression analyses (Fiss et al. 2013; Vis 2012). As Vis (2012) argues, "[a]dding a configurational approach to a regression analysis helps to uncover patterns in the empirical data that otherwise would have remained hidden" (2012, 192). This rather speaks to the *complementarity* of the two methods, under the condition that certain requirements are met. For example, in their study on biological attributions of mental illness, Matthew Andersson and Sarah Harkness (2018) adopt a multi-method approach that combines QCA and regression analysis (see Box 2.2) and Tobias Ide (2018) uses statistical techniques to derive a sample of cases before the QCA part of his study (see Box 2.1).

As for process tracing, it is equally misleading to frame the debate in either/or terms. As discussed in Chapter 4, the unique strength of process tracing is the identification of causal mechanisms through the intensive study of individual cases. Yet, as a within-case method process tracing lacks a cross-case perspective. This is where the connection with QCA can be especially fruitful. While Munck (2016, 5) sees tension between process tracing and QCA, recent work highlights their common set-theoretic foundations (Beach et al. 2019; Goertz 2017; Rohlfing and Schneider 2018; Schneider and Rohlfing 2013). This also resonates with the perspective expressed by George and Bennett (2005, 163), who suggested that QCA case comparisons should be complemented with process tracing.

Formalization and Algorithms

Another strand of critique has taken issue with QCA's use of algorithms and what is perceived as an increased formalization in the method's analytical protocol (Collier 2014a; 2014b; Munck 2016; Sartori 2014). From the commentators' perspective, it is the methods' evolution and refinement that has distanced QCA from the original goal of case-based comparative research. Moreover, the complexity of QCA solutions and the emphasis placed on formal logic are seen as undermining the method's value as an empirical research approach.

This view is expressed by David Collier (2014a; 2014b), who recommends that QCA "should set aside the algorithms", calling instead for a renewed emphasis on "case knowledge, process tracing, and the use of qualitative data" (2014b, 123-24). Moreover, Collier highlights that many "scholars now seek the most 'simple and intuitive' tools adequate to the task at hand", arguing that "[c]ase knowledge should not be an adjunct to the algorithms but rather the primary method of analysis" (2014b, 124-25). Likewise, Giovanni Sartori (2014), in a brief comment on *A Tale of Two Cultures* (Goertz and Mahoney 2012), cautions against an overreliance on technique:

> Yet the intricate fuzzy set procedures cantilever out from these questions, posing dangers of technique that concern me. In some domains of social science we now see a growing skepticism about complex statistical techniques – and a turn to simpler tools. The elaborate procedures of fuzzy sets merit the same skepticism (Sartori 2014, 15).

Munck (2016) seems to share this skepticism, particularly when it comes to arguments that are rooted exclusively in formal logic, such as the contrasting discussion of regression analysis and set-theoretic methods in Thiem et al. (2016):

> A key problem in Thiem et al.'s discussion is that they posit, as do other advocates of STCM [set-theoretic comparative methods], an analysis of causation *entirely in formal terms*. This is a basic oversight, because *a causal relation is not a logical relation* but, rather, a relation between events or, more precisely, between changes in the properties of things. But Thiem et al. have nothing to say about the semantics of empirical sciences […] (Munck 2016, 3, emphasis added).

These are reasonable points. In my view, Collier, Sartori, and Munck raise general concerns about research methods that many QCA proponents would doubtless agree with. To begin with, it should not be controversial to say that the minimization algorithm and the computation of measures of fit should *guide* the analysis, but they are not supposed to take the driver's seat. This is in line with the recommendation that QCA should never be applied mechanically, deprived of theoretical and substantive considerations – points that have been emphasized throughout this book. Yet, these concerns do not warrant the blanket conclusion to "set aside the algorithms", as Collier (2014b) proposes. Rather, more effort should be placed in clarifying what the computational routines in QCA effectively accomplish and where they depend on researcher input (see Duşa 2019b; Oana et al. 2021).

As introduced in previous chapters, QCA's procedures for calibration and Boolean minimization are based on clear principles and as such they can be manually reproduced. The solution terms are not the result of some impervious procedure but based on the truth table and the treatment of logical remainders. Likewise, fuzzy sets and calibration draw on clear rules of mathematical transformation – there is no "magic ingredient" involved.

Surely, if one is not accustomed to calibrated data, then this requires some getting used to, but that applies even more so to some advanced quantitative methods and formal modelling, both of which can appear impenetrable to the uninitiated. In that light, Chapter 5 provides a guide to the mathematical transformation that is entailed in fuzzy-set calibration, whereas Chapter 6 and Chapter 7 shed light on the calculation of measures of fit and the minimization algorithm that forms the core of the analytical part of QCA (see also Duşa 2019b; Oana et al. 2021; Ragin 2008; Schneider and Wagemann 2012).

Finally, what is important to note is that we need to distinguish between QCA as a *method* and its *application* in empirical studies. As John Gerring (2012) rightly points out, "The *potential utility* of a method should be differentiated from its *actual employment*" (2012, 350, emphasis added). To put it bluntly, the existence of flawed statistical analyses or badly done case studies does not mean that either approach is invalidated. Just as there may be p-hacking in regression analyses and anecdotal evidence in case studies, there are QCA studies that do not fulfill criteria of good research practice. Such criteria have been formulated and will continue to evolve as QCA matures as a method (Oana et al. 2021; Rihoux and Ragin 2009; Schneider and Wagemann 2010; 2012). There have also been a variety of efforts to hold empirical applications against the developed standards concerning research design, calibration, and analytical routines (e.g., Mello 2013; Rohlfing 2020; Thiem 2016; Wagemann et al. 2016).

Causal Analysis and Solution Terms

In recent years, a debate has emerged about causal analysis and the contribution of the different QCA solution terms towards that aim (Baumgartner and Ambühl 2020; Baumgartner and Thiem 2020; Duşa 2019a; Haesebrouck and Thomann forthcoming; Schneider 2018; Thiem 2019). Michael Baumgartner and Alrik Thiem (2020) argue on the basis of simulated data that only the parsimonious solution of QCA should be used for causal inference (see also Thiem 2019), while the conservative and intermediate solutions are both deemed incorrect due to their inclusion of redundant elements. By contrast, Adrian Duşa (2019a, 1) finds that an intermediate solution with directional expectations "emerges as the best hybrid that is suitable for causal analysis", which ties in with the longstanding recommendation to place emphasis on the intermediate solution (Ragin 2008, 175; Schneider and Wagemann 2012, 279). This exchange has sparked exchanges about the accuracy of different solution types and the prerequisites of causal inference with set-theoretic methods (see also Haesebrouck and Thomann forthcoming; Schneider 2018).

It is beyond the scope of this book to develop each side's argument and evidence, but it should be noted that the simulations by Baumgartner and Thiem (2020, 24) were not run on approximations of "real-life" data with limited diversity and imperfect set relations, as the authors themselves acknowledge. There is also no mention of untenable assumptions, which can present a major problem for the parsimonious solution (see Chapter 7).

In essence, the debate about the correctness of the solution types boils down to different understandings of methodological aims in social science research. The argument by Baumgartner and Thiem (2020) favoring the parsimonious solution appears consistent within a *formal perspective* on regularity theory and logic, driven by the impetus to find the last "difference maker" for an identified effect. From this perspective, any condition that is deemed "redundant" must be eliminated, even when this comes at the expense of context and background. This contrasts with a *case-based perspective* that acknowledges complex causation and aims at theory-guided analysis within specified boundaries. In fact, as Duşa (2019a, 19) shows, the intermediate solution *outperforms* the parsimonious solution once its directional expectations are correctly specified.

To put it differently, when *theory* is inserted into the picture, a purely logic-driven perspective becomes less persuasive. I contend that most social scientists who are interested in configurational comparative methods would prefer to inform their analysis with expectations derived from theory and their own knowledge of the field, rather than risk letting the algorithm include untenable assumptions in the computation of solution terms (as may happen with the parsimonious solution). This resonates with a position among methodologists on set-theoretic methods that substantive knowledge and research design should not be outweighed by the mechanical implementation of technical routines (Ragin 1987; Schneider and Wagemann 2010; Wagemann 2017).

Another way to look at the solution term debate is in terms of the *complexity-parsimony* continuum that characterizes the solution terms derived by QCA (see Chapter 7). Due to the way Boolean minimization works, the solution terms stand in subset-superset relationships to each other, beginning with the complex solution, which is a subset of the intermediate solution, which in turn is a subset of the parsimonious solution. From this perspective, it would be nonsensical to claim that a complex solution was "incorrect" when its parsimonious superset was deemed "correct". The parsimonious solution would simply be the *most general claim*, whereas the complex solution constitutes a subset that makes a more *specific claim*.

For instance, the parsimonious solution may yield that smoking was a sufficient condition for cancer (among some observed group of individuals), whereas the complex solution may suggest that the combination of smoking and obesity was sufficient for the outcome. If the parsimonious statement is deemed correct, then the complex statement must also be correct. As this example illustrates, the exchange may be less about the correctness or truth value of the specific solution terms, but rather about the weight assigned to parsimony as opposed to context-sensitivity. But focusing solely on difference-makers may not suffice, as Rani Lill Anjum and Stephen Mumford (2018, 125) remind us: "there are some causes that are not difference-makers; and there are some difference-makers that are not causes". Moreover, John Mackie acknowledged that research typically evolves within a "causal field" of context and background conditions. Distinguishing which of these should be considered a cause rather than a condition rests in no small part on the phrasing of the causal question at hand (Mackie 1980, 34-36). Again, this points us back to the salience of theory and research design.

At this stage, one take-away from this ongoing debate is what has long been considered *good practice* in applied QCA research – namely that, depending on the research aims of a study, any of the three solutions can be emphasized (if untenable assumptions are taken care of), but that all three solution terms should be reported in publications, at least in a supplementary document (Rihoux and Ragin 2009; Schneider and Wagemann 2010).

In line with the discussion on theories of causation in Chapter 4, I encourage a *pluralist* view on the matter of causal analysis, siding with the cautionary note by Anjum and Mumford (2018, 250) that "one should be open to evidence acquired through plural methods because causation has plural symptoms." Clearly, QCA is not tied to a single theory of causation and different meta-theoretical frameworks can be grafted onto set-theoretic research.

Recognizing QCA's Strengths and Limitations

To conclude this chapter, a sober look at QCA's strengths and limitations is warranted. In my view, the core strengths of QCA are threefold. To begin with, as outlined throughout this book, one of the distinctive features of the method is that it takes into account *causal complexity*, or "the combinatorial complexities of social causation", as Ragin puts it (1987, 170).[7] This is a conscious departure from "net effect" thinking on relations between individual variables (Mahoney 2010). Causal complexity acknowledges that social phenomena can often result from various configurations, recipes, or pathways (*equifinality*), each of which may entail several different conditions (*conjunctural causation*). Moreover, the relations between configurations and outcomes can usually not be mirrored symmetrically (*causal asymmetry*), as when the absence of a cause is assumed to also be the cause of the non-outcome. Moreover, the concept of equifinality challenges the assumption that cases with similar outcomes must have common causes at their root (George and Bennett 2005, 161).

How to make the most of causal complexity when using QCA? Empirical applications stand to benefit when they are consciously modeled on set-theoretic relationships. This means that theory should be formulated in the language of necessary and sufficient conditions, including reference to INUS and SUIN conditions, where appropriate. This further entails thinking about specific combinations of conditions, rather than examining and justifying each condition individually. In that context, it can be helpful to *visualize* theoretical expectations, especially when these entail conjunctural causation and equifinality. Alternatively, the QCA solution terms can be visualized, which also helps enhance the understanding of causal complexity. In his discussion of causal mechanisms, Goertz (2017, Ch. 2) covers some helpful examples of graphic illustrations for relationships of causal complexity.

The second core strength of QCA is that the method allows for a *systematic comparison* across empirical cases and the incorporation of *counterfactual reasoning* on logically possible combinations of conditions that are not empirically substantiated. Both elements incorporate the truth table as analytic device. In this sense, QCA has a disciplining effect as it demands from researchers not just selective comparisons, as they often occur in small-*N* comparative studies, but instead requires systematic and exhaustive data generation and a consistent calibration for all cases entailed in the analysis. At the same time, the truth table will indicate the empirical distribution across the attribute space, and it will also show whether there are any empty configurations, information that can then be applied for the assessment of counterfactual cases. To profit from this strength, users should devote sufficient attention to the examination of the truth table, the distribution of empirical cases, the consistency of the respective rows, and also examine the non-substantiated configurations (logical remainders). As outlined in Chapter 7, the latter provide an opportunity for counterfactual reasoning on whether or not cases with certain characteristics would show the outcome of interest if they existed. Such expectations can then be incorporated in the intermediate solution.

Finally, QCA gives researchers generous *flexibility* to adapt the approach to the context of their own research aims. Though QCA is often characterized as a medium-*N* method and the majority of applications work with a range of 20-30 cases (see survey results in Chapter 2), the method works equally well in large-*N* settings and, albeit some restrictions, it is also feasible to work with smaller numbers of cases. With small numbers the main restriction is that the number of conditions should also be kept low, to maintain a ratio of at least four cases per condition (see Table 2.2). Apart from the number of cases and conditions, QCA also allows for flexibility with regards to the kind of data that it used for the analysis. The set-theoretic calibration can draw on all kinds of qualitative and quantitative information and these can also be combined to create meaningful indicators for set-theoretic concepts. Moreover, given the diversity of QCA variants and analytical routines on offer, users can select the approach that best suits their research aims and the concepts they are working with (see Chapter 8). The benefits of flexibility should be consciously utilized following a problem-driven approach. This means that there is no single "orthodox" way of how QCA must be done, but a menu of feasible options that users can choose from, in line with their own research aims. Yet, it must be underlined that established standards of good practice apply to all QCA applications, irrespective of the chosen variant (Rubinson et al. 2019; Schneider and Wagemann 2010).

Despite these strengths, QCA has limitations. There is no silver bullet method in the social sciences. First, as was acknowledged in the section on analytical robustness, the method is *sensitive* to changes in the selection of cases and conditions and the setting of calibration thresholds. On a practical level, this means that users should always provide theoretical and substantive reasons behind choices that informed their research design, while robustness tests can serve to increase the confidence in the analytical results. Having said that, it is important to keep in mind the *qualitative* core of QCA as a case-based method. This means that rather than conducting a battery of quantitative robustness tests, which might eventually mimic statistical approaches, there should be reasonable substantive arguments as to why certain calibration thresholds were chosen, on which grounds the cases were selected, and which options existed for the researcher during the analytical procedure.

Second, as a comparative method, QCA simply requires a certain *number of cases* and *comparable data* to "get off the ground" and run reliably. This poses a research-pragmatic constraint for academic fields that are characterized by small-*N* and comparative case studies (Mello 2017). For applications in these areas, the first hurdle is collecting data on a sufficient number of cases and conditions to allow for a comparison of, say, at least 12 cases. Certainly, the number of observations can be increased by disaggregating cases into subunits, for example when countries are separated into regions or municipalities, governments are examined rather than countries, or individuals are compared rather than groups or cohorts of people.

Finally, as was mentioned in Chapter 8 on QCA variants, there has not yet been a satisfying solution to the problem of *temporality*. At its core, QCA remains a static comparative approach where the conditions are treated in the same way in their relation to the outcome (Marx et al. 2014). Yet, the question of how to take into account time and sequence is pervasive in the social sciences (Büthe 2002), and as such it constitutes a limitation that applies to a large number of social science methods and is not restricted specifically to QCA. Apart from the recent introduction of "trajectory-based QCA" (Pagliarin and Gerrits 2020), which offers a promising approach to address temporality, feasible ways to address the issue through research design are either (a) to indirectly include timing and sequence in the concept formation for the conditions, or (b) to combine QCA with another method that is capable of acknowledging these features, such as process tracing (see Chapter 2). Table 9.3 summarizes the strengths and limitations of QCA and strategies on how to make the most of the former and address the latter.

Table 9.3 Strengths, Limitations, and Strategies

Strengths	*How to make the most of these*
Accounts for causal complexity (conjunctural causation, equifinality and causal asymmetry)	Focus on set-theoretic relationships, formulation of causally complex theoretical expectations, visualization of causal complexity
Allows for systematic comparison across empirical cases and counterfactual reasoning on logical remainders	Emphasis on truth table, conscious treatment of logical remainders, substantive interpretation of solution paths
Flexibility in adapting to specific research aims (cases, conditions, qualitative and quantitative data, analytical routines, QCA variants)	Problem-driven use of most suitable research design, QCA variant, and analytical routine
Limitations	*How to address these*
Sensitive to changes in calibration and case/condition selection	Thorough justification of analytical choices, conducting robustness tests
Requires a certain number of cases and comparable data	Adjustments in the case/condition ratio and calibration, disaggregation of cases
Comparison is inherently static, conditions are treated equally	Integration of time in conceptualization and research design, use of specific QCA variants, combination with other methods

Notes

[1] Rohlfing and Schneider (2014, 32).

[2] See the cross-disciplinary survey results at the end of Chapter 2 for differences in the number of cases typically included in QCA studies in various academic fields.

[3] At the time of writing, the COMPASSS community website lists 19 different software packages related to QCA and configurational methods, see: https://compasss.org/software/.

[4] Journal issues with a focus on QCA and set-theoretic methods include (in chronological order): *Sociological Methods & Research*, 1994, 23(1); *Field Methods*, 2003, 15(4); *Qualitative Methods*, 2004 2(2); *Qualitative Methods*, 2005 3(1); *Studies in Comparative International Development*, 2005 40(1); *International Sociology*, 2006, 21(5); *Political Analysis*, 2006, 14(3); *Political Research Quarterly*, 2013 66(1); *Sociological Methodology*, 2014, 44(1); *Qualitative & Multi-Method Research*, 2014, 12(1); *Qualitative & Multi-Method Research*, 2014, 12(2); *Field Methods*, 2016, 28(3); *Comparative Political Studies*, 2016, 49(6), *Quality & Quantity*, 2017, 51(5), and *Quality & Quantity*, 2021 (forthcoming).

[5] For a concise articulation of the case-based perspective and its take on earlier critiques, see Rihoux (2003).

[6] For an overview on robustness tests, see also Thomann and Maggetti (2020, 13-14).

[7] For a detailed discussion of causal complexity, see Chapter 4.

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