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COMMENT: LUCAS AND SZATROWSKI IN CRITICAL PERSPECTIVE

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The stated goal of Lucas and Szatrowski (this volume, pp. 1–79; hereafter L&S) is to provide a critical evaluation of qualitative comparative analysis (QCA), with the objective of discouraging researchers from using the method. However, they fail to provide a meaningful critique. Close inspection reveals not only that they address a shallow caricature of QCA but also that their analysis has fatal flaws.

This response to L&S opens with a discussion of QCA's essential features as an *approach* to social research, which L&S largely ignore. I next turn to the challenge of identifying patterns in empirical evidence and explicate the distinctiveness of QCA's approach, especially its attention to cases as configurations, its use of the method of elimination, and its allowance for causal asymmetry, aspects L&S fail to properly grasp. I then pinpoint fatal errors in their analysis of the space shuttle data, their single attempt to analyze empirical evidence. I also identify a basic flaw in their application of QCA to "deterministic" data.

MISREPRESENTATION OF QUALITATIVE COMPARATIVE ANALYSIS

L&S limit their discussion to QCA as a method of data analysis. However, it embraces several key elements that together define it as an approach. Any critique that fails to address these interdependent aspects is not a critique of QCA. These key features include the following:

- *Allowance for causal complexity:* QCA privileges causal complexity, allowing for the possibility that causation may be both conjunctural and equifinal. When causation has these qualities, several different combinations of conditions may be linked to the same outcome.
- *Integration of case-based and other forms of substantive knowledge:* QCA works best in a dialogue with cases and/or substantive knowledge. This knowledge sets the stage for the specification of relevant causal conditions, the calibration of crisp-set and fuzzy-set membership scores, and the resolution of "contradictions" (cases with the same causal profile but different outcomes).
- *Use of the method of elimination:* QCA uses truth tables and controlled case comparisons to eliminate causal conditions in an incremental, context-bound manner. The method of elimination is superior to both Mill's method of agreement and his indirect method of difference because the focus is on eliminating causal conditions, not confirming them. L&S wrongly attribute Mill's methods to QCA (see Ragin 1987:34–52).
- *Focus on set-theoretic and quasi-set-theoretic relations:* QCA seeks to identify sets of cases that share an outcome. If cases with a given configuration of causally relevant conditions share an outcome, they constitute a subset of the cases with the outcome. This set-theoretic pattern supports an argument of sufficiency, assuming this interpretation is corroborated with other evidence, usually at the case level.
- *Case-oriented counterfactual analysis:* An important feature of QCA is its use of theoretical and substantive knowledge to define "easy" counterfactuals—hypothetical cases that resemble empirical cases in all respects except one, with the one difference making the outcome more likely in the hypothetical case than in the empirical case. The use of easy counterfactuals makes it possible for QCA users to emulate the practice of conventional, small-*n* comparative research.

Although the application of QCA does entail data analysis, this analysis occurs in close dialogue with cases and with theoretical and substantive knowledge, features L&S ignore.

"SIGNAL VERSUS NOISE" IN SOCIAL RESEARCH

It is useful to think about the challenge of identifying patterns in empirical phenomena as a *signal-versus-noise* problem, which occurs in virtually all scientific investigations. The phenomena studied by scientists are often very "noisy," and the "signal" (i.e., the underlying pattern) is difficult to discern. Whether the noise is stochastic or deterministic in origin is very much a side issue and in fact may be unknowable. The problem is noise, and the researcher's key task is to filter out noise in ways that unveil different aspects of the underlying signal. In the physical sciences, there are countless forms of noise and countless filters. At issue is the problem of determining which filters to use and for what purposes.

Although social scientists are less sophisticated in their tools for dealing with signal-versus-noise problems, they also use different methods of filtering noise in their efforts to identify and explain patterns. Consider the noise filter that is central to conventional applications of quantitative methods (hereafter CQNR for "conventional quantitative research"), the data analytic techniques championed by L&S. Almost all applications of CQNR define noise as the result of stochastic processes (i.e., random interference) and use probabilistic methods to filter out specific types of noise. The CQNR filter assumes further that an additive linear model, in which independent variables compete to explain variation in a dependent variable, provides a useful baseline representation of the main pattern of interest. Thus, CQNR favors a filtering device that embodies a very specific understanding of both noise and signal.

Other social scientists use different filters to identify patterns. The filters used by network analysts, for example, allow the identification of ties between nodes and produce graphical representations of those ties. Most network models have little use for the CQNR filter. In fact, many network applications could be described as fully "deterministic" because they lack explicit error vectors. Next, consider research that examines macrosocial formations over centuries (e.g., Mahoney 2010). In research of this type, the signal may seem very clear relative to the noise because the focus is on broad changes observed over the *longue durée*. Furthermore, when *n*'s are small, researchers are able to address the fate of each case, sometimes with particularizing explanations citing case-specific features or events. (Viewed through the lens of CQNR, the goal of addressing every case is unrealistic.) Finally, consider ethnographic research. Although a devotee of CQNR would describe this research as deterministic because of its lack of an explicit error vector, the *deterministic* label would seem completely nonsensical to ethnographers. Ethnographers distinguish between noise and signal by knowing their cases well and by linking together different aspects of their cases via within-case analysis. In short, qualitative researchers try to maximize the validity of what they have learned, thereby enhancing the clarity of the signal.

It is inappropriate to apply the standards of a filter designed for one purpose to a filter designed for a different purpose. Imagine criticizing regression analysis for its inability to portray cases in terms of network nodes and ties. Imagine criticizing a small-*n* comparative-historical researcher for providing an account of each case. (In fact, far from condemning such an effort, social scientists usually expect it.) Qualitative and quantitative researchers often talk past each other because of their adherence to different noise filters. In effect, they each say to the other camp, "According to the standards of the methods I favor, I find your methods deficient."[1] Not much is gained from this exchange.

Consider the researcher with 20 cases, an outcome that varies, and six causally relevant conditions that also vary. The researcher's case-based substantive knowledge indicates that causation is complexly combinatorial and equifinal. Would it be reasonable to judge CQNR on how well it filters noise from signal in this situation? Of course not. CQNR's home turf is random noise in large-*n* data sets, along with the embedded assumption that an additive linear model offers a useful baseline representation of the underlying signal. If a data set permits (i.e., if it combines a great abundance and a great diversity of cases), it is sometimes possible to test simple interaction models with CQNR, but nothing like the examination of complex combinations of conditions that might be needed for research that posits that causation is both conjunctural and equifinal.

Next consider the same researcher again, with 20 cases and six causally relevant conditions and a concern, again based in case knowledge, for causal complexity. This time, however, the 20 cases all exhibit the same outcome (e.g., ethnic conflict leading to a breakdown of political authority). Viewed through the lens of CQNR, there is no variation in the outcome and therefore nothing to explain. But qualitative researchers routinely examine sets of cases with the same outcome and are not troubled by the lack of variation in the outcome. The key is that they study positive cases so that they can discern patterns in "how the outcome comes about." Did the 20 cases share any causally relevant antecedent conditions? Were there multiple pathways to the same outcome? Shared antecedent conditions are especially important because they may indicate possible necessary conditions. Of course, from the perspective of CQNR, explaining a constant (the shared outcome) with another constant (the shared antecedent condition) is nonsensical. Despite this indictment, qualitative researchers find this strategy very useful. Of course, in the end, they may differentiate types of cases. Still, the search for shared antecedent conditions for cases with the same outcome is a well-established trailhead.

QCA was developed to address the concerns of researchers interested in causal complexity, defined as conjunctural causation plus equifinality. Researchers who know their cases well typically understand causation conjuncturally. In fact, as a general rule, the closer analysts are to their cases, the greater the visibility and transparency of social causation's complexity.[2] For this reason, the practice of QCA is predicated on an energetic interplay between case-based knowledge and cross-case analysis. This "best practice" is completely ignored by L&S, which is not surprising given their focus on simulated data sets.

Limited Diversity and the Distinctiveness of QCA's Noise Filter

Many of the objections to QCA leveled by L&S stem from a single feature of the approach, namely, its attention to the *configurational* character of social phenomena and its allied focus on causal complexity. This central feature of QCA is apparent in the rigorous standard it sets for the *elimination* of causal conditions. For practitioners of CQNR, eliminating an independent variable is a simple matter of demonstrating that the variable's unique increment to explained variation in the dependent variable is trivial (i.e., not distinguishable from random noise). This assessment, in turn, is usually based on a parsing of a matrix of bivariate correlations or its analytic equivalent. The important point here is that CQNR has little or no direct interest in the combinatorial diversity of the cases included in an analysis. By contrast, combinatorial diversity is the key focus of QCA.

To set the stage for this important contrast between CQNR and QCA, it is useful first to consider the gold standard for assessing causation in

Table 1. Experimental Design: The Gold Standard

Row	X_1	X_2	Correct Number of Subjects Assigned	Actual Number of Subjects Assigned
1	Low	Low	20	20
2	Low	High	20	0
3	High	Low	20	20
4	High	High	20	20

analytic social science, the experiment. Two of the defining features of experimental design are (1) its focus on combinations of experimental conditions and (2) the practice of random assignment of cases to conditions. For example, with two experimental variables (high vs low values of X_1 and X_2), an experimenter would assign subjects randomly to the four combinations of the two experimental conditions, as shown in Table 1. Assume that the experimenter decided not to assign subjects to combination 2, as shown in the last column of Table 1, but still conducted an analysis of the main effects of the two conditions. The experimenter's justification would be that the contrasts available (e.g., 4 vs 3 and 3 vs 1) provide adequate information about the effects of the two experimental variables, and thus there is no need to assign subjects to condition 2. Assume further that the results of this analysis reveal that the effect of X_2 is not significant. Can this researcher validly argue that X_2 is irrelevant?

Although incomplete experimental designs play a role in some investigations, it is clear in the example just given that the experimenter has made an error. More than likely, this study would be quickly rejected as a candidate for publication. It would be wrong to draw conclusions about the effects of X_1 or X_2 without knowing the outcome (i.e., the mean and standard deviation of the dependent variable) for all four combinations of X_1 and X_2. In the language of QCA, the experimenter has manufactured a situation of *limited diversity*, which occurs whenever sectors of the vector space defined by the causal conditions are void of cases.

Limited diversity is the rule in nonexperimental research, even large-*n* research (Ragin 2008:196–99). CQNR does not address limited diversity directly; instead, it invokes assumptions (e.g., additivity and linearity) that bypass the issue altogether. Given a survey data set comparable with the

Table 2. Limited Diversity: A Key Feature of Nonexperimental Data

Number	Strong Unions	Strong Left Parties	Number of Cases	Generous Welfare State
1	No	No	9	No
2	No	Yes	0	?
3	Yes	No	8	No
4	Yes	Yes	7	Yes

experimenter's data set just described (i.e., with one of four sectors void of cases), a practitioner of CQNR would not hesitate to estimate the net effects of X_1 and X_2. Nor would journal referees object to this practice. If the researcher found the effect of X_2 to be nonsignificant, X_2 could be justifiably relegated to the dustbin of the investigation.

QCA has a very different approach to the elimination of variables, one that is much closer to the experimental design template.[3] Consider Table 2, which shows an analysis that parallels that shown in Table 1. There are two causal conditions and an outcome. There is also limited diversity. A practitioner of CQNR would likely focus on the perfect correlation between strong left parties and a generous welfare state and conclude that having strong unions does not matter. But it is important to understand that this conclusion assumes that if cases combining strong left parties and weak unions could be identified, they would have generous welfare states. Using QCA, by contrast, the issue of limited diversity is highlighted, and the researcher confronting Table 2 must decide: Given that all known instances of generous welfare states occur in countries that combine strong unions and strong left parties, is it reasonable to conclude that having strong unions does not matter? The prudent answer is, Of course not, especially given all the existing case evidence on union collaboration with left parties in struggles to establish generous welfare states. In fact, it would be a serious error to eliminate strong unions from the explanation of generous welfare states. QCA's complex and intermediate solutions to Table 2 would both show that it is the combination of the two causal conditions that matters.

More generally, QCA eliminates causal conditions contextually and incrementally. If two cases are identical in all causally relevant respects save one and display equivalent outcomes, then the causal condition that differs between the two cases is irrelevant. But it is designated as

Table 3. Race and Success in Presidential Campaigns (from L&S)

Outcome	African American	White
Won	1	34
Lost	31	99

irrelevant only in that *specific context*, as defined by the other causally relevant conditions, not in all contexts. Nor does its elimination in this context have any direct implication for other contexts or for any assessment of the condition's average effect. Thus, *average treatment effect* is not a meaningful concept in QCA. For many researchers, this aspect of QCA is a valuable feature of the approach; for researchers interested in estimating the net effects of independent variables, it is obviously an obstacle.

As this example illustrates, QCA's approach to empirical evidence is qualitatively different from that of CQNR. It is clearly more difficult using QCA to completely eliminate causal conditions than it is with CQNR. The bar for elimination is very high and can be met only via multiple controlled case comparisons. Although this bar may seem too high for scholars who apply CQNR to nonexperimental data, the payoff is clear: QCA provides tools specifically designed for analyzing cases as configurations, deciphering causal complexity, and conducting a dialogue between cross-case analysis and both substantive and case knowledge.

ASYMMETRIC CAUSATION

Set-theoretic methods are especially attentive to causal and other forms of relational asymmetry. Consider Table 3, which is drawn directly from L&S's discussion of causal asymmetry. L&S argue, on the basis of standard CQNR reasoning, that the only proper way to analyze this table is to use information from all four cells at once. This analysis shows that the probability of success is much greater for white candidates for president than for African American candidates. From a set-theoretic viewpoint, however, there are other, more striking patterns in the table. First, there is a near perfect set-theoretic relation between being white and being a successful candidate. Successful candidates are almost all white (i.e., these candidates constitute a near perfect subset of the white candidates), indicating that being white is a very widely shared antecedent condition for success (consistency = .971). Second, it is clear that being African American is a near perfect subset of losing candidates, indicating that this outcome is very widely shared by African American candidates (consistency = .969). Bolder verbal formulations of these two patterns would describe being white as "almost always necessary for success" and being African American as "almost always sufficient for failure" when it comes to presidential elections. L&S look past these striking, asymmetric connections, and they focus exclusively on differences in probability levels. However, there is much to be gained from paying attention to data patterns that exhibit or approach set-theoretic connections; identifying such connections can help researchers better understand the fabric of social life.

In their discussion of causal asymmetry, L&S devote considerable effort to arrive at a conclusion that almost every QCA user with a modicum of experience knows well: The more a truth table approaches full and consistent specification, the more likely it is that the solution for $Y = 0$ will be the *logical negation* of the solution for $Y = 1$.[4] They contrive an example of an almost fully specified truth table with three contradictory rows and argue that asymmetric results for $Y = 1$ versus $Y = 0$ follow from having rows that fail to meet the consistency threshold value for either $Y = 1$ or $Y = 0$. Such rows are analytically indeterminate and thus are coded "false" for both the analysis of the outcome and the analysis of its negation. They then argue that by manipulating the consistency threshold—for example, dropping it to .25—it is possible to make the indeterminate rows determinate and thereby produce results that at least have the appearance of symmetry (as illustrated in L&S's Tables 8–11).

L&S commit many analytic errors in their treatment of causal asymmetry. The most glaring error is that they miss the point of set-theoretic analysis altogether. Set-theoretic techniques focus on connections that are very strong. For crisp sets, this means that the probability of the outcome for a given set of cases is very high (e.g., $\geq$ .80), as in the examples in Table 3, in which there is a very high probability that successful candidates are white and a very high probability that African American candidates are unsuccessful. Thus, the data manipulations that L&S impose to produce symmetrical results entail complete abandonment of meaningful set-theoretic thresholds.

A second error is the fact that asymmetric results do *not* follow simply from having rows with consistency scores that fail to pass either threshold, for $Y = 1$ or $Y = 0$. Much more common is a situation in which there is limited diversity and thus a substantial number of truth table rows lacking empirical instances. For example, Olav Stokke's analysis of successful versus unsuccessful shaming of violators of fishing agreements examines five crisp causal conditions and thus involves a truth table with 32 rows (see Ragin 2008:167–72). Eight of these rows have empirical instances, 24 rows lack empirical instances, and all rows are contradiction free (i.e., all outcome probabilities are 1 or 0). Analysis of this evidence reveals interesting (and telling) patterns of asymmetry. For example, all instances of failed shaming share "inconvenient for the target of shaming to change practices" as an antecedent condition, whereas successful instances vary on this condition. All instances of successful shaming share "shaming supported by scientific advisory body" as an antecedent condition, whereas unsuccessful instances vary on this condition. These shared antecedent conditions could warrant interpretation as necessary (but not sufficient) conditions, assuming such interpretations are consistent with substantive and case-level knowledge. Thus, "support of the scientific advisory body" could be viewed as a necessary condition for successful shaming, while "inconvenient to change practices" could be viewed as a necessary condition for failed shaming. Asymmetries such as these are well beyond the reach of both L&S and CQNR.

THE SPACE SHUTTLE ANALYSIS

L&S offer only one analysis of empirical evidence; all their other analyses use simulated data. Given the importance of the dialogue with cases and substantive knowledge that occurs in applications of QCA, it is reasonable to suppose that the analysis of the space shuttle data offers their most important confrontation with QCA. Even though there is little opportunity for a direct dialogue with cases, at least the analysis is anchored in the empirical world and not in a researcher's imagination.

L&S apply QCA to published data on O-ring erosion in 23 launches of the space shuttle. The raw data they cite are shown in Table 4. To calibrate the two pressure variables as fuzzy sets, they appear to use 50 = .05, 100 = .27, and 200 = .95. To calibrate temperature as a fuzzy

Table 4. Space Shuttle Data

Case	Flight	Field Pressure	Nozzle Pressure	Erosion	Joint Temperature (°F)
1	STS-1	50	50	0	66
2	STS-2	50	50	1	70
3	STS-3	50	50	0	80
4	STS-5	50	50	0	68
5	STS-6	50	50	0	67
6	STS-7	50	50	0	72
7	STS-8	100	50	0	73
8	STS-9	100	100	0	70
9	STS-41-B	200	100	1	57
10	STS-41-C	200	100	1	63
11	STS-41-D	200	100	1	70
12	STS-41-G	200	100	0	67
13	STS-51-A	200	100	0	67
14	STS-51-C	200	100	1	53
15	STS-51-D	200	200	1	67
16	STS-51-B	200	100	1	75
17	STS-51-G	200	200	1	70
18	STS-51-F	200	200	0	81
19	STS-51-I	200	200	1	76
20	STS-51-J	200	200	0	79
21	STS-61-A	200	200	1	75
22	STS-61-B	200	200	1	76
23	STS-61-C	200	200	1	58

set, they appear to use 32°F = .05, 65°F = .50, and 98°F = .95. O-ring erosion is coded dichotomously, even though there is some variation that could have been used to create a fuzzy-set outcome. Some notes on their analysis follow:

- In general, when pairing a crisp-set outcome to fuzzy-set causal conditions, consistency and coverage scores will tend to be low. Although this arrangement is not fatal, it should be avoided, if possible.
- Sets and set labels almost always involve adjectives in some way. For example, it is possible to assess degree of membership in heavy; it is not possible to assess degree of membership in weight. This aspect of sets is fundamental to set-theoretic analysis. L&S violate this principle by using mechanistic calibrations based on ranges. This issue is especially important for the calibration of temperature because the overarching engineering concern was for temperatures that were low,

a unipolar concept. Thus, L&S should have calibrated degree of membership in low temperature, based at least in part on case knowledge. Instead, they tried to calibrate degree of membership in temperature, which is nonsensical.

- Despite repeated efforts to reproduce their results, using different calibrations and different specifications of the analysis, my results differ. My solution consistency scores failed to reach the .75 consistency threshold they claim to have used. This failure is troubling given that I use their stated data source and calibrations. Although the differences were relatively minor, it was necessary to lower the consistency threshold to .70 to reproduce their results.
- A proper analysis of these data using degree of membership in low temperature (calibrated as 65°F = .05, 60°F = .50, and 45°F = .95) shows the following solutions:

  Parsimonious solution: low_temperature → erosion (consistency = .97, coverage = .18)

  Intermediate solution: low_temperature × high_field_pressure → erosion (consistency = .97, coverage = .18)

  In short, the parsimonious QCA solution gives the "correct" answer. The high consistency coupled with low coverage indicates that low temperature consistently leads to O-ring erosion; however, it is not the only path to erosion.

Although L&S might complain that high field pressure is added in the intermediate solution as a contributing cause, it is important to consider what the difference between these two solutions shows. In essence, the intermediate solution is signaling that there are no cases of low temperature combined with low field pressure in this data set. Consequently, there is no basis in these data for ruling out high field pressure as a contributing cause in the intermediate solution. Although L&S might want to label this result a diversion from the central finding, an engineer working on the O-ring problem would find this additional information very useful. If high field pressure is ruled out as a contributing cause, this elimination cannot be made on the basis of the given empirical evidence because the cases are limited in their diversity.

Finally, it is important to have full disclosure and avoid giving a one-sided presentation. What I am referring to is the simple fact that L&S failed to present the logistic regression analysis of this data set, which is

Table 5. Logistic Regression Analysis of Space Shuttle Data, with O-ring Erosion Regressed on Joint Temperature, Field Pressure, and Nozzle Pressure

Variable	Coefficient	Standard Error	z	$p > \|z\|$
Constant	9.344	8.443	1.11	.268
Temperature	–.183	.125	–1.46	.144
Field pressure	.010	.013	.75	.452
Nozzle pressure	.017	.016	1.05	.294

surprising given that they often use this technique in their critique. I present the missing analysis, using the original uncalibrated data, in Table 5. This straightforward application of logistic regression to the data reveals a startling contrast with the QCA results. QCA correctly identifies the culprit—low temperature—and adds a proviso regarding a limitation of the evidence. The logistic regression, by contrast, reports that none of the hypothesized causal variables has a significant effect. The failure of temperature to reach significance holds even with the two pressure variables removed from the equation. Thus, the type of analysis favored by L&S, drawn directly from the CQNR toolkit, leaves engineers in the dark (and out in the cold) about the impact of low temperatures on O-ring erosion.

FATAL FLAWS IN LUCAS AND SZATROWSKI'S SIMULATION 1

L&S create a data set of 40 cases with four variables (X_1, X_2, X_3, and X_4) randomly coded 1 or 0. They then code an outcome variable (Y) equal to 1 whenever X_1 and X_2 both equal 1 and also whenever X_3 and X_4 both equal 1; otherwise, Y is set to 0. They then claim to apply QCA to this data set with the expectation that they will find the following (correct) solutions:

$$X_1 * X_2 + X_3 * X_4 \rightarrow Y,$$

$$\sim X_1 * \sim X_3 + \sim X_1 * \sim X_4 + \sim X_2 * \sim X_3 + \sim X_2 * \sim X_4 \rightarrow \sim Y.$$

Any regular user of QCA knows that this result is likely but not guaranteed only for the parsimonious solutions for Y and $\sim Y$. The issue is that the truth table (which shows all 16 combinations of values for X_1 to X_4)

must be fully populated for the complex solution to show the anticipated results. Forty cases of random ones and zeros for X_1 to X_4 may not be enough to fully populate the 16 rows of the truth table. The table must be fully populated because this data set is completely without any empirical basis, and thus there can be no meaningful recourse to cases or to counterfactual analysis or even to substantive knowledge (which could be used to address the rows without cases—the empty sectors of the vector space defined by the causal conditions). L&S should have focused only on the parsimonious solutions, and in fact, the parsimonious solution they report for the analysis of ~Y is the correct solution.

They report the following solution for the analysis of Y and indicate the same result for the parsimonious, intermediate, and complex versions:

$$X_1 * X_2 * \sim X_4 + X_3 * X_4 \rightarrow Y.$$

They note that the first combination should not include ~X_4. They are correct; it should not. And, in fact, *it does not*. A reanalysis of their data for this exercise shows clearly the following solution for Y:

$$X_1 * X_2 + X_3 * X_4 \rightarrow Y,$$

which is the correct solution, and it is the same for the parsimonious, intermediate, and complex versions. There is no way to produce the solution for Y that they report with the data they present. This fatal error in one of the simplest possible applications of QCA casts a long shadow over the entire article.

SUMMARY

L&S grossly misrepresent QCA. They ignore several key aspects, especially QCA's articulation with case and substantive knowledge. They also fail to grasp the distinctiveness of the method of elimination and its centrality to configurational analysis, and they completely miss the point of set-theoretic analysis. They misconstrue both limited diversity and causal asymmetry. They botch their only application of QCA to empirical evidence and fail to disclose the results of a logistic regression analysis of the same data. In their analysis of simulated data, L&S present incorrect results that cannot be duplicated with the data they present. In the end, L&S are further from their objective of discouraging the use of QCA than they were before they began.

Notes

1. I thank Howard S. Becker for emphasizing this point in various exchanges over several decades of colleagueship.
2. This inconvenient truth is at the root of the disjunction between quantitative and qualitative analysis.
3. Of course, the defining feature of experimental design is the random assignment of cases to conditions, which is not possible in most social scientific investigations. Although the case pairings of *controlled case comparison* are sometimes described as quasi-experimental, a more truthful labeling would be pseudo-experimental.
4. It is important to understand that there is a world of difference between logical negation (which usually involves the application of a law that is apparently unknown to L&S—De Morgan's) on one hand and reverse coding a dependent variable in a linear model (the focus of L&S) on the other. Explicating this difference for the benefit of the uninitiated is beyond the scope of this comment.

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