What Are the Boundaries of this Potential Revolution? Exploring the Shape of Mahoney's Scientific-Constructivist Social Science

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Jim Mahoney has written an opus magnum. The breadth, scope, and potential implications of the system of thought proposed by Mahoney are such that any attempt—or at least any attempt by me—to do justice to all important aspects of this book is, unavoidably, doomed to fail. I will necessarily have to be very selective and focus on those aspects that I feel reasonably competent. And, even more, rather than commenting or responding to some of Mahoney's main arguments, I will mostly ask questions of clarification. These will be genuine questions, not rhetorical ones. I am curious to hear Mahoney's answers because—so my hope—those answers will further sharpen the boundaries of the revolution of the social science (singular!) that Mahoney is arguing for. I shall also disclose that I not only find this book mind-blowing and horizon-widening, but also largely agree with its main gist.

My questions (and, occasionally, some tentative answers) are structured in four groups. The first set aims at probing the difference in practice between, on the one hand, "scientific constructivism," Mahoney's core concept and favorite logic of the social science and, on the other hand, what he identifies as the current predominant logic of "essentialist" approaches. The second set of questions focuses on Mahoney's version of a regularity theory of causation. In the third set, I address the difference between fuzzy sets (an established term) and continuous sets (the term preferred by Mahoney). And the fourth set is a mixed bag of comments on issues that are less central to Mahoney's overall argument.

What is the Difference between Scientific Constructivist and Essentialist Approaches in Practice?

In Part I of his book, Mahoney makes the, I think, very convincing case that because social science is fundamentally different from (most) natural sciences, their methods must also differ. This difference stems from the fact that social scientists do research on social kinds. Unlike natural kinds, social kinds only exist in the researchers' (collective) minds. From this ontological position, Mahoney argues, radical consequences follow for the practice of how social science should be done.

For Mahoney, social science must be scientific constructivist. It is constructivist because social science concepts are mind constructed and need to be captured by assigning membership scores of cases in sets. This is an inherently interpretive act. The scientific part largely rests in the use of formal logic for analyzing relations between sets. With the following questions, I try to understand better what the practical implications for social science research are of Mahoney's position, according to which there is a sharp ontological division between scientific constructivist and essentialist research.

First, Mahoney explains in detail how the practice of calibrating sets —that is, establishing the membership of cases in mind-dependent social science concepts—is fully in line with the constructivist element of scientific constructivism. What I am wondering is whether other elements in the research process are equally constructed or whether they fall into the "scientific" domain of scientific constructivism. In particular, I am curious about the status of set relations. Arguably, identifying set relations of necessity and sufficiency (and some more complicated derivatives) is the goal of scientific-constructivist research. But are those set relations socially constructed or are they merely the result of applying the cold rules of formal logic? The book seems to allow for both answers. On the one hand, if set membership scores are constructed, set relations also ought to be constructed. On the other hand, formal logic and mathematical rules not only represent an important element of the scientific component of scientific constructivism, but, following philosophers like Leibniz, for Mahoney they also enjoy the elevated ontological status of absolute truth. This status of logic is remarkable, for truth is a scarce resource in a social (science) world in which things are made up by humans and therefore are contested and subject to change over time and space.

A second question probing the practical implications of the scientific constructivist revolution is this: Should scientific-constructivist researchers pay less attention to things that are currently associated with essentialist research, but which also feature high on the agenda of set-theoretic methods? Here I have in mind discussions on appropriate robustness tests for QCA results or the properties of different (minimization) algorithms for analyzing set membership data. My take on this

would be that these more technical and computational problems pertain to the scientific aspect of scientific constructivism and should therefore continue to play an important role in refining and improving scientific-constructivist methods. In the book, however, there is little to no mention of such topics of applied empirical research and I am not sure if this is done intentionally or is simply caused by lack of space.

Third, Mahoney convincingly argues that it is wrong to take an essentialist perspective on social categories. How wrong, though? Mahoney himself writes (2021, 66) that there are two feasible ways of interpreting set membership scores: as facts (essentialist approach) or as societally agreed facts (constructivist approach). Whether one or the other approach is chosen does not seem to make any (important) difference in applied research. Mahoney even concedes that essentialist research can be (and often is) very successful in predicting social events—even if, according to Mahoney, by definition and default, it cannot establish causality. If my reading is correct, the question becomes: Does it matter in practical terms whether we assume essentialism or constructivism when analyzing sets?

Fourth, by design, scientific-constructivist research is about discrete categories of social phenomena and their set relations. My question is: Where, if anywhere, is there room for all those relevant questions that have at their core non-discrete phenomena and that are focusing on forms of associations other than set relations? For instance, in scientific-constructivist social science, can we continue to ask questions such as: Is economic performance related to political participation? or Does the amount of exposure to hate speech on social media increase the risk of political radicalization? Currently, such questions seem to dominate in essentialist empirical social research. Declaring (causal) research on them impossible would be quite a revolutionary step that might need some more explicit treatment and justification.

Fifth, and somewhat related to the last question: Can one imagine and design experiments that stay true to the principles and practices of the scientific-constructivist approach or would that amount to a contradiction in terms? If yes, what would such experiments have to look like? If no, what drives the incompatibility between scientific constructivism and experiments? Is it that the former is largely Y-oriented, whereas the latter largely X-oriented? Or is the incompatibility rooted at a deeper, ontological level?

Scientific-Constructivism and the Regularity Theory of Causation

Mahoney discusses three different theories of causation: causal power, counterfactual, and regularity (for details, see the very informative Table 3.1 on page 91). He identifies the latter as the most fitting for the scientific-constructivist approach. Mahoney's version of regularity theory of causality stipulates that cause X must (a) precede outcome Y in time; (b) make direct or indirect spatial contact with Y; and (c) be part of a minimized solution that is constantly conjoined with Y (2021, 91). This raises several questions of clarification for me.

First, the last criterion – that the cause is part of a minimized solution set – takes care of the question of causal relevance: Are all sets in a solution difference-maker causes? It leaves out, though, the question of causal completeness: Are all difference-making causes for the outcome included in the solution? This makes me wonder how in Mahoney's regularity theory of causation and, by extension, in applied scientific-constructivist research, the issue of model under-specification is dealt with.

Second, according to Mahoney, regularity models of token causality are best fitting for scientific constructivism. One of the most developed scientific-constructivist methods is the set-theory based method of Qualitative Comparative Analysis (QCA). My understanding of QCA is that it reveals type causality. If this is correct, I am asking myself: Does this make QCA incompatible with scientific-constructivist research? Does it prevent QCA from being able to reveal causality? And, in which way, if any, would either QCA and/or Mahoney's vision of social science need to be adapted to be fully compatible? Perhaps my next question provides a partial answer to this set of questions.

Third, I like Mahoney's interpretation of regularity theory of causation requiring spatiotemporal contact between X and Y. I read this the following way: For complete causal inference based on a regularity theory of causation one must include an analysis of the causal mechanism between X and Y that underpins a cross-case effect of X on Y. I am sure, many case-based researchers could not agree more. This reading would also solve partially my previous question on the causal status of cross-case patterns identified with QCA. To be causally interpretable, such cross-case pattern also need to be based on some evidence on within-case mechanisms. This is precisely what the literature on set-theoretic multi-method research is mostly about (e.g. Schneider forthcoming). My only question would then be this: Why do other contemporary proponents of regularity theories of causation not seem to attribute any importance or relevance to causal mechanisms (e.g., Baumgartner 2008)? In fact, most of them would probably explicitly deny any role for mechanisms in causal inference within a regularity theory framework. If the addition of mechanisms to this framework is an innovation by Mahoney, then it is probably worthwhile to point this out

more clearly. Criticisms from other regularity theorists on Mahoney's requirement for a causal mechanism is likely to come his way and defending this addition is, I believe, a worthwhile effort.

Continuous vs. Fuzzy Sets

Mahoney replaces the established term "fuzzy sets" with the term "continuous sets." This is consistent with his earlier writings, in particular that with Gary Goertz in their seminal "Two Cultures" project (Goertz and Mahoney 2012). I have already expressed my uneasiness in a previous QMMR newsletter (Schneider and Wagemann 2013). The disagreement is not about which term to use or whether changing the term unsettles the semantic field and creates more confusion than necessary. The more important point is that the introduction of a different term seems to come with the introduction of a different meaning: fuzzy sets and continuous sets are probably not meant to mean the same thing. Let me explain what I think the difference is and why the meaning of continuous sets is potentially problematic for scientific constructivist research.

Fuzzy sets are sets. They first and foremost establish qualitative differences between members and non-members of a set. In other words, fuzzy sets categorize cases just like crisp sets do. With fuzzy sets, the distinction between members and non-members is established at the membership score of 0.5, the so-called point of maximum ambiguity (Ragin 2008).

Continuous sets also must establish such a qualitative distinction, else they are not sets. The question is where on the range of membership values between 0 and 1 is this qualitative shift located? The notion of "continuous" seems to rule out that the qualitative shift occurs at the 0.5 membership value. A more likely candidate is the membership value of 0. All cases that hold membership of higher than 0 are not only partial members of the set in question, but also qualitatively different from those that hold zero membership. For instance, in the set of tall person, someone with membership 0.1 would be qualitatively identical to someone with membership 0.9 but qualitatively different from someone with zero membership. As said, this is not how things are normally seen with fuzzy sets, where all cases below 0.5 are qualitatively different from those above 0.5.

Here is what I find problematic about a reinterpretation of where the qualitative anchor rests in continuous sets. First, if my interpretation about the location of the qualitative anchor is correct, it would need to be spelled out clearer than it is in the book. It represents a deviation from the common interpretation of fuzzy sets and triggers a series of (unintended?) consequences that I spell out in the following.

Second, imagine a case with, say, 0.3 membership in the set of "tall person." With continuous sets, it qualitatively counts as a tall person because its membership is higher than zero. The problem with this becomes apparent if we ask: What is this person's membership in the logical complement of "not-tall person"? The 1-x rule for logical negation yields a membership of 0.7 in the set of not-tall person. Hence, that very same person would also qualitatively count as a not-tall person. This is a contradiction in terms: one and the same person cannot count qualitatively as both tall and not-tall. Note that with fuzzy sets, this logical fallacy does not occur. With 0.3 membership in the set of tall person, the person in question qualifies as not-tall because their membership is below the qualitative anchor of 0.5. This classification becomes clearer if we calculate the person's membership in not-tall persons: 1 - 0.3 = 0.7, thus above the qualitative anchor of 0.5.

Third, because of its property to never classify cases as qualitatively belonging both to a set and its negation, fuzzy sets can be used in the analytic apparatus of QCA. At the heart of QCA-based research is the truth table. This table consists exclusively of 1s and 0s. Representing fuzzy sets in "crisp set-looking" truth tables can only work because the qualitative anchor in fuzzy sets is located at 0.5. With continuous sets and their alleged location of the qualitative anchor at 0, the current QCA principles and practices would need to be radically rethought and adapted. In the spirit of this reflection on Mahoney's book, I am turning this observation into a question: Am I right in locating the qualitative anchor in continuous sets at the membership value of 0? If yes, am I right in pointing out some problematic consequences of this redefinition of fuzzy sets? And if yes, how can these intended or unintended consequences be fixed?

Two Miscellaneous Observations

Mahoney states that the empirical importance of set relations can be captured by how close a given condition X comes to being both necessary and sufficient for an outcome Y. I fully agree. There is, however, also a second element of empirical importance that is not mentioned in the book. For necessity claims, importance also hinges on how big condition X is in relation to its logical negation ~X. In other words, if condition X is very big and therefore close to a constant, then ~X is very small. It is potentially trivial to claim that such a very big X is necessary for any given outcome Y, because it is virtually impossible for a very big set to not be a superset of whatever else set Y is. The QCA literature has developed the parameter of Relevance of Necessity (RoN) to capture both sources of empirical importance/relevance of necessity claims (Schneider and Wagemann 2012, chapter 9.2). Since not much attention is paid to this source of set-relational trivialness, I am wondering

if this is because Mahoney does not deem it relevant for scientific-constructivist research or whether it has been de-emphasized due to lack of space or lack of importance.

For sufficiency claims, a similar problem of skewed set membership scores exists, but it is of practical relevance only with fuzzy sets. A condition X can be so small that it passes the sufficiency test for both outcome Y and its negation ~Y. Claiming that this X is sufficient for both outcomes would be logical nonsense and must be avoided. This problem is not fully addressed by Mahoney's conceptualization of empirical importance of set relations either. Charles Ragin has developed the PRI parameter to avoid the pitfall of such simultaneous subset relations (for details, see also Schneider and Wagemann 2012, chapter 9.2). Furthermore, with the traditional interpretation of fuzzy sets and their location of the qualitative anchor at 0.5, most of the dangers of these simultaneous subset relations of X vi-a-vis both Y and ~Y can be kept under control. With the notion of continuous sets and their qualitative anchor at 0, in contrast, this analytic problem seems to increase and strategies for containing it would become even more relevant.

My experience from many years of teaching set-theoretic methods is that a sizable chunk of participants tends to struggle when first exposed to a comparatively modest level of formal logic. Even more advanced students continue to sometimes mix up necessity and sufficiency when looking at set relational patterns in their data. My ad-hoc amateur evolutionary theory explanation of this has long been that (formal) logic does not seem to be hard-wired into the human brain because it was (and still is) not needed for survival. This, however, clashes with Mahoney's view, according to which logic is an essential tool in human reasoning. I would be curious to know how these seemingly opposing views and experiences could be reconciled.

A Concluding Praise

James Mahoney and his book deserve the highest praise. He has mine, not only for the vast knowledge and sophisticated mind that it takes to write such a text. I also admire the courage that is required to call for a revolution and to face some of the reactions that Mahoney's call to arms will (hopefully) trigger.

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Two Views of Within-Case Analysis: Ambiguities about Process Tracing in *The Logic of Social Science* and Beyond

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I am grateful to have been included in this conversation with esteemed colleagues about Jim Mahoney's important new book. Rather than using this opportunity to offer praise of the book (which would be easy to do) or criticism of an already-published work (which would be less than useful for the author, or for readers) I would like to use Mahoney's book as an opportunity to explore a tension that, in my view, underlies much of the contemporary scholarship on qualitative methods, and to suggest that the book itself is a bit at odds with itself on a core element of qualitative research in ways that point to some unresolved issues for