

with only an inductive coding scheme. Nonetheless, other quantitative approaches to the study of identity fail to capture the meaning and salience of identity in any sense, and a small sacrifice in reliability is necessary to achieve a major increase in validity.

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Interpretation, Causality, and Family Resemblances

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How might we link interpretation and causal inference? Our symposium contributors all agree that interpretivist and causality-oriented approaches can be selectively and productively combined. Yet if we are generally bullish on these prospects, our optimism is built on a strong desire to steer clear of what we see in many quarters—namely, the deep subordination of interpretivist approaches to causality-oriented ones. We do not view interpretive methods as the “summer intern” to the supposedly more serious work performed by non-interpretivist tools of inquiry.¹

We all agree that, because meaning making is not a marginal part of the human experience, any account that assumes that humans’ interpretations are irrelevant, epiphenomenal, or just so much “noise” will not convince. Likewise, accounts that “pretheorize” (to use Allan and Hopf’s apt term) by unthinkingly accepting categories contained in readily available datasets, will not make for persuasive research. Thus, we are convinced that causality-oriented work in the social sciences simply must attend to meaning-making processes if it is to be credible.

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¹ Nor do we suffer from a numbers allergy. To the contrary, as Allan and Hopf underscore in this symposium, quantitative data suitable for statistical analysis can be built from well-grounded, carefully considered, and inductively produced interpretations of texts. On the “summer intern” problem in a slightly different context, see Hopf (2006).

But how far does this basic intuition take us? In this concluding commentary, I contend that if we want to pursue this bridge-building effort further, we should pay close attention to the multiple meanings that undergird our efforts.

Three Family Resemblances

Given our discipline's great variety of intellectual interests, epistemological inclinations, and ontological starting points, are scholars even speaking the same language? Certainly, it can seem that positivists are from Mars and interpretivists from Venus, but what about us interplanetary travelers? Do we mean even roughly similar things when we invoke "interpretation," "causation," and the like?

Interpretation

Consider "interpretation." In a sense, all scholars interpret—whether p-values or speeches from the President of Sri Lanka or testimonies and behaviors of villagers in rural Bolivia. In each case, the researcher demonstrates her/his understanding of the data's significance and conveys that understanding to others. Like a professional interpreter working in multiple languages, she re-renders the original information in a form that will be appreciated by the intended audience.

But these forms of interpretation are also dissimilar, and so the meaning of "interpretation" must be understood as polyvalent. Consider the kinds of interpretation surrounding p-values in statistical research on causation. The front-end of such research proceeds on a series of core assumptions (that are themselves interpretations), including that causality is additive and causal effects homogeneous.² Further, those working with p-values agree that a value of, say, 0.05 has a uniform statistical meaning. In these senses, interpretive judgments are basically outsourced to community standards and applied mechanically. Interpretation by prior consensus is still an interpretation. The back-end of such research is more explicit about interpretation. For instance, should we take seriously only results that reach the $p < 0.05$ threshold? What about $p < 0.1$? But, even here there are conventions that tend to constrain claims about which significance levels "matter."³ The contrast with ethnographic and discourse-analytic work could not be starker. In the latter cases, interpretation is improvisational, in the sense that Dvora Yanow uses the term. It entails a systematic-ness and requires training, but it is not reducible to decision rules invariably applied.⁴ Instead, interpretation relies heavily on the credibility and training of the interpreter herself.

Even in this collection of symposium essays, a smaller slice of the epistemological spectrum, there is still notable variety in the meaning and form of interpretation. Allan and Hopf engage in the interpretation of texts produced at a great temporal and geographic distance from the researcher. This dis-

² All research proceeds on ontological assumptions. The difference is that scholars working in the mainstream often need not defend their assumptions.

³ Thanks to Alan Jacobs for helping to clarify this point.

⁴ See Yanow 2006, 70.

tance implies a particular kind of interpretation. First, it is an interpretation that lends itself to quasi-replication. After all, their texts (whose content does not change) and their interpretations can be easily shared with a broader community of scholars. While much depends on the interpreter and her/his training, background, and sensibilities (hence, the "quasi" in "quasi-replication"), in theory this opens up the process at multiple points for constructive scholarly scrutiny. Second, this kind of interpretation rarely involves the thicket of ethical considerations more typical of research involving live interlocutors. In a sense, the researcher enjoys a freer hand since the actors involved cannot "talk back," but there is also a downside: she cannot ask questions, follow up on leads, generate new topics, and so on.⁵

Ethnographic forms of interpretation speak with a different accent. For Simmons and Smith, interpretation occurs during and after encounters conducted at close range with interlocutors in the field. In this sense, interpretation is the product of iterated interactions, and it is not only potentially much richer than other forms of interpretive work; it is also more likely—because of complex connections in changing situations—to generate surprising research findings. But Simmons and Smith are not content to let the surprise of a case amount to the totality of their intellectual contribution; they seek to refine and improve interpretations via comparison with additional cases. A hallmark of their approach is in fact that what constitutes a "case" is inductively derived and therefore itself changeable.⁶

Samford's ethnographic interpretation broadly shares this intellectual instinct. Also taking a participant-observation approach, he considers ethnography to be particularly suited to interpreting a possible link between variables that co-vary in statistical work. Thus, one of the differences in his approach is that it is oriented toward a larger array of cases, even if only to problematize (as he does) the apparent link among variables. If his form of interpretation is in principle broadly consistent with Laitin's tripartite approach,⁷ the relative inclination of each to problematize (rather than validate) apparent causal links emerging through statistical analysis likely differs.

Norman's approach adds yet another dimension. His version of interpretation requires attention to how meanings crystallize and shift over time and how these meanings enable certain kinds of political possibilities while foreclosing others. If, for all of the symposium contributors, meaning-making work is the stuff of human (including political) communities, Norman is unique in claiming that the role it plays is best viewed diachronically.

In "interpretation" we thus see Wittgenstein's famous "family resemblance," in which family members may not share a single, common feature (except, and this is debatable, at a very high level of abstraction); instead, they display a series of overlapping qualities. When we invoke "interpretation," such overlapping connotations put us into conversation with one

⁵ This is the takeaway in Pachirat (2009).

⁶ See also Ragin and Becker 1992.

⁷ Laitin 2003.

another, but they do not prevent us from speaking past each other.

Causation

Scratch the surface of “cause” and “causation” and a second family resemblance reveals itself. Again, on an abstract dimension, this family has common traits. It would be hard, for example, to use the term “cause” without some notion of temporal antecedence and some (whether strong or weak) notion of the counterfactual.

Yet, there is variety. Can causes rise to a standard of necessity and sufficiency, or are they better viewed via Mackie’s INUS conditions and Ragin’s causal heterogeneity and limited diversity?⁸ Is it more useful to view causes as probabilistic or as deterministic? Alternatively, is it preferable to posit causality in a diffuse sense, as producing “conditions of possibility” (Norman, this symposium), for particular outcomes to emerge?

For some, establishing causality and generalizing go hand in hand. This is especially true for Allan and Hopf who equate quantitative (and therefore large-N) with “good causal inference.” Although much will depend on how scholars ultimately use the dataset that Allan and Hopf produce, this type of data lends itself to work that views causality in Humean “constant conjunction” fashion. Something can be established as a cause only if it repeats itself across many cases.

Not all of the contributors are interested in establishing causal relationships across a broad range of cases or events; some prefer to focus on causes in heavily time- and space-delimited contexts. Norman’s emphasis on mechanisms, consistent with other work invoking that term, implies that he eschews general claims about causal effects that would exceed the boundaries of a single place or time. He proposes that insights can be portable to other contexts, since causal patterns or elements of causation can and do reappear in other places and times. But, he insists that contextual meanings weigh heavily on how these patterns or elements play out in social and political processes.

Thus, a sometimes unstated but important dimension along which notions of “cause” vary is from abstract to context-dependent. Does one’s ontology allow factors to have an independent causal valence in the abstract, or does it require that context weigh heavily on how and whether any given factor in fact becomes causal? How complex do we assume context to be, and what weight do we assign it for shaping causal processes or effects?

As with the symposium contributions, our understandings of these matters are often implicit. But they nonetheless structure the conversation we are having (or not, on those occasions when we talk past each other).

Generalization

In “generalization,” we see a third family with a series of overlapping qualities. Some researchers harbor an ontology of cau-

⁸ INUS conditions are “insufficient but necessary parts of a condition which is itself unnecessary but sufficient” (Mackie 1965, 245). See also Ragin (2000).

sation in which a factor becomes causal only if it plays the same role repeatedly across a variety of circumstances. Strong versions include Hempel’s famous “covering law” model.⁹ We lack among our symposium participants a proponent of such versions, but Allan and Hopf’s approach—which has no *a priori* scope conditions and could apply to all cases where the nation is a meaningful socio-political category—does nothing to preclude it.

For others, to generalize means simply to make more general, even if modestly so. In practice, this could mean extending outward—even by adding a single case—the validity of a claim. Simmons and Smith’s proposal to fold additional cases for comparison into the analysis runs along these lines; they strongly imply a preference for inductively derived, mid-level theory building and view their comparative ethnography as taking steps in that direction. While ethnography has come to be known for its context-sensitivity more than its generalizing ambitions, Simmons and Smith make clear that they are open to the inductive discovery that similar processes may unfold across cases.¹⁰

If generalization simply meant to make claims about causal effects across cases, then Norman’s interpretive process tracing and Samford’s ethnographic approach to network analysis would not qualify as engaging in it. After all, both promise faithful and theoretically grounded accounts of particular time- and space-bound cases, but they do not offer strong claims about the applicability of their empirical findings to external cases.

But there are different types of generalization. For example, we might distinguish the empirical from the analytic variety. Empirical generalization implies that all cases under given scope conditions display the same dynamics and generate the same causal effects. By contrast, analytic generalization¹¹ implies that versions of the same process or the same factor are *at play* across a variety of cases. How precisely they play out, however, may easily be a function of contextual factors. If empirical generalization implies a clear prediction about dependent-variable values across cases, analytic generalization gives strong guidance about factors and processes to consider as one moves from the original set of cases to additional ones.

Norman makes this point in a way that Samford would likely find congenial: one might reasonably seek to have her study’s findings “speak to” other cases outside the study’s original empirical scope without claiming that empirical patterns in other cases will mirror those found in the original ones. In this sense, all of our authors seek to generalize *analytically* by developing insights that are, in essence, *portable*—i.e., that can be brought to bear in efforts to refine our understanding of a broader range of cases. Norman does so by considering mechanisms, which in some proportion and in some fash-

⁹ Hempel 1965.

¹⁰ Thanks to Alan Jacobs for suggesting this point. On ethnographic approaches to the study of politics, see Schatz (2009).

¹¹ Becker’s (1990) “theoretical generalization” comes close to what I have in mind.

ion may be operating in a variety of times and places.

There are still other forms of generalization. One might—as in versions of rational-actor theory—generalize by assuming that human beings make choices in essentially the same way. Or one might—as in much large-N statistical work in our discipline—generalize by assuming causal homogeneity (rather than multiple causal paths). The list could go on, but I do not have to. The key point is that we may mean different things when we invoke the term “generalization” or its cognates.

Finally, our words do not just have polyvalent meanings; their meanings may have normative valences. In some interpretivist circles, “to generalize” carries a negative connotation roughly equivalent to: *to seek to elide important nuances and distinctions that constitute the human experience*. Likewise, in some positivist circles, “interpretivism” carries a negative connotation, implying something like: *work that properly belongs in the humanities because it cannot be adequately scientific*. Being aware of such valences strikes me as prior to having productive conversations across traditions.

Write out, Write Down, or Write in?

My invitation to be explicit about the meanings that lie behind our words extends to one final area—an area largely absent from our symposium contributions: the interchangeability of researchers. One of the principal premises of interpretivist work is that research scholars are not identical. Yes, it is true that we tend to have roughly similar disciplinary training, but that is not enough to stamp out the individuality that characterizes our scholarly choices. As a result, in some significant part the insights generated during the research process are a function of the person doing the research. By degree, all essays above downplay this point. Allan and Hopf hint at it via Kratochwil’s dialogic notion of reliability in which coders should discuss their different interpretations and come to a consensus position.¹² But the non-interchangeability of scholars is a much larger matter. How do the initial positionality, learning strategies, mid-course corrections, and forms of improvisation engaged in by researcher and researched alike produce particular forms of understanding?

I see three major options regarding what are sometimes called “researcher effects.” They might be called *write out*, *write down*, and *write in*.

In *writing out*, a researcher constructs her account with little or no reference to her choices, instead proceeding on the assumption that the research would have unfolded in essentially the same way had another scholar conducted it. This can leave traces in our written accounts. For example, when we use the passive voice or transform verbs into nouns we write *out* the choices that individuals, including us as scholars, make.¹³ While such an approach is unlikely to find broad resonance among those whose ontology is based on humans as meaning-making creatures, writing the first-person “I” *out* of the research account is common in our discipline.

In *writing down*, a researcher lays bare—to whatever degree she views as appropriate—her research strategies and choices. While there is considerable variety, in recent years this often has taken the form of an appendix on methods. The idea is that by writing down her choices, justifying them as defensible, she nods to transparency while keeping these choices essentially segregated from the claims to knowledge that she advances.

In *writing in*, a researcher integrates the choices made in the research process into the research account itself, charting out how her evolving vantage point conditions the claims that she advances. She does so in order to persuade, believing that analytic leverage is produced precisely when specific individuals make very particular choices in specific situations. Based on a notion of transparency different from what we have seen in recent disciplinary initiatives,¹⁴ she hopes to increase her credibility as an interpreter of the social and political by revealing (rather than concealing) how her interactions developed and observations occurred.

Writing in is sometimes assumed to be a move that is fundamentally opposed to building knowledge. I do not share this assumption. As Pachirat reminds us, rather than seeing “researcher effects” as evidence that an account is partial in the sense of being biased, we might view them as an indication that an account is partial in the sense of being incomplete.¹⁵ All accounts are incomplete, and it takes a cumulative and collective effort of scholars to make it less so.

This final matter—different perspectives on the interchangeability of research scholars—has long been viewed as creating a firewall between interpretivists and those who would make causal inferences. I suspect that in some quarters it may continue to be so viewed. After all, if the knowledge we claim rests not only on contextual complexity but also on very specific interactions between the researcher and the researched, then very little can be said that might transcend particulars. In such a view, while other elements of interpretivism can be integrated into causal inference, the principle of non-interchangeability (a core interpretivist intervention) cannot.

Yet, in our everyday lives, we regularly advance claims designed to transcend the particulars of place and time. Put differently, we leverage our own selves—our background, our training, our proclivities, our relationships, and our knowledge—to offer a sense of the world around us and share that sense with others. In our scholarship, we of course have higher standards for creating, sharing, and evaluating claims, but the essential elements—individuals doing their best to make judgments about the world, sharing those judgments with others, and refining their judgments in the course of things—remain the same. If research is an intersubjective, social exercise, then there is no compelling reason why researcher singularity would be an obstacle to the collective construction of knowledge claims, including those about causality.

¹⁴ See the various contributions to *Qualitative and Multi-Method Research* (2015).

¹⁵ Pachirat 2009.

¹² Kratochwil 2007, as cited in Allan and Hopf, this symposium.

¹³ On how turning verbs into nouns obfuscates, see Billig (2013, 95–114).

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2015 QMMR Section Awards

Giovanni Sartori QMMR Book Award

This award recognizes the best book, published in the calendar prior to the year in which the award is presented, that makes an original contribution to qualitative or multi-method methodology per se, synthesizes or integrates methodological ideas in a way that is itself a methodological contribution, or provides an exemplary application of qualitative methods to a substantive issue. The selection committee consisted of Audie Klotz (Syracuse), chair; Claudio Radaelli (University of Exeter, UK); and Guillermo Trejo (Notre Dame).

Winner of the 2015 Award: Melanie Cammett, *Compassionate Communalism: Welfare and Sectarianism in Lebanon*. Ithaca, NY: Cornell University Press, 2014.

Prize citation, written for the committee by Guillermo Trejo:

The committee received 43 nominations for the Sartori Book Award. We were impressed by the quality of several books, but the committee unanimously agreed that Melani Cammett's book, *Compassionate Communalism*, truly stood out. The book deals with a new and compelling substantive topic (the distribution of social services by sectarian political parties in Lebanon) that has important theoretical and practical implications for the developing world. The outline and execution of the book's research design are exemplary, as is the use of multiple units of analysis (districts and individuals), multiple sources of evidence (qualitative and quantitative) and multiple methods.

Very much in Sartori fashion, Cammett is remarkably clear in defining every concept she uses throughout the book and the two dimensions she uses to derive behavioral expectations about the different modes of social service provision—political mobilization strategies and intrasect competition—yield insightful theoretical propositions. Cammett's discussion of the empirical implications of her theoretical arguments pro-

vides a nice transition to the book's empirical section. The spatial analysis of welfare distribution centers by district is quite effective and is carefully complemented with the unique use of three types of individual-level data on access to welfare: 183 interviews with elites; extensive interviews with non-elites conducted by "proxies" (Lebanese graduate students); and a national survey. In analyzing individual-level data, the book carefully draws on a wide variety of quantitative and qualitative methods to test its main propositions. At the end, the book's explicit discussion of external validity by looking at cases beyond Lebanon adds much value to Cammett's central and important findings.

Alexander George Article/Chapter Award

This award recognizes the journal article or book chapter, published in the calendar prior to the year in which the award is presented, that—on its own—makes the greatest methodological contribution to qualitative research and/or provides the most exemplary application of qualitative research methods. The selection committee consisted of Taylor Boas (Boston University), chair; Jeffrey Checkel (Simon Fraser); and Candelaria Garay (Harvard).

Winner of the 2015 Award: Noam Lupu, "Brand Dilution and the Breakdown of Political Parties in Latin America." *World Politics* vol.66 no.4 (October 2014): 561–602.

Prize citation, written for the committee by Taylor Boas:

Noam Lupu's article addresses the important question of why some longstanding political parties in Latin America experience sudden breakdown, or a dramatic loss of electoral support. Lupu rejects existing arguments that focus entirely on poor governing performance or structural challenges to parties, arguing that they cannot explain anomalies or variation within countries. Instead, he develops an individual-level ex-