

# Why and How to Select a Pathway Case of Intervention in Civil War<sup>1</sup>

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## **Abstract**

Methodological advice suggests that a “pathway case” nested analysis is appropriate for maximizing external validity when developing a mechanisms-based account of interventions in civil war. Yet only the most general guidelines are given with regard to the specific issues that arise when trying to apply the pathway case design. This paper discusses four such issues—model specification, measurement validity, the indeterminacy of case selection, and the question of generalizability—and proposes strategies to deal with them. If properly addressed, the pathway case remains a fruitful research design. By incorporating a mechanisms-based causal logic in a nested analysis, the pathway case mixes methods in terms of both data and epistemology. As such it is an appropriate response to the large-N research programme on civil war.

## **Introduction**

By which mechanisms are third-party military interventions in “ethnic” civil wars linked with transnational ethnic affinities? That—in one question—is the central puzzle for this research project. The starting point for analysis is the following scenario. A country experiences organized civil violence, framed by perpetrators and victims as being “ethnic”, and the violence involves parties that have putative ethnic kin in a foreign state. Given that kin states sometimes intervene militarily in civil wars, and given that transnational ethnic affinities often are included in the explanation of such interventions, the objective here is to empirically examine and theorize the mechanisms at play. The co-variation of transnational ethnic affinities and interventions is taken as a given. I ask how they connect.

Thus is the central research objective of my PhD project introduced. A political outcome to investigate is identified—third-party military interventions in “ethnic” civil wars; an explanatory variable of interest and assumed co-variate is named—transnational ethnic affinities; and a particular type of theory-building is signalled—mechanisms-based causal explanation.

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Accept for now the following postulates. Mechanisms-based causal explanation is fundamentally different from effects-based causality. The appropriate method for uncovering causal mechanisms is not comparison but process tracing. Process tracing may be applied to only one case of intervention, although it can—and must—include multiple observations. To maximize the potential for inferring beyond the case, its selection must be purposive not random.

Enter this paper. Its primary purpose is to discuss problems associated with one particular method of case selection—the pathway case (Gerring & Seawright, 2007)—and their possible solutions. The attention is justified given that a commitment to mechanisms-based explanation is coupled with a wish to create opportunities for generalization. The pathway case would seem to offer such opportunities because of the particular way it is “nested” (Lieberman, 2005) in its own universe. The paper also argues that the pathway case thus constitutes a mixed-method strategy—not only because it combines quantified, standardized dataset observations in its selection technique with qualitative, non-standardized, non-comparable data in the case study; but also because it embeds a mechanisms-based causal logic within a sample viewed through an effects-based causal lens. Adjunct to these discussions, the paper positions the project on mechanisms connecting transnational ethnic affinities to third-party military interventions in “ethnic” conflict as a response to the large-N research programme on civil war.

By way of a roadmap for the paper, the first section raises the issue of the large-N research programme on civil war, suggesting that mechanisms are a timely alternative mode of explanation. The second section considers why process tracing is the appropriate method, while the third section argues that incorporating process tracing in a nested design could combine the strengths of large-N research with the advantages of qualitative methods. The final sections discuss the pathway case nesting strategy—its selection technique, its eclecticism, issues arising with the pathway case, and ways of addressing them.

### **The Need for Mechanisms**

The large-N research programme on civil war has evolved to maturity—or as some would have it—is in need of reinvention. By “large-N” I mean the well-established, well-known research programme whose unit of observation is on the country level, and whose method is the statistical comparison of large numbers of cases. Indicative of this state of affairs is the publication of meta-studies. These are not review essays of the civil war literature in the traditional sense, but empirical research where the *field* of civil war studies is observed, not civil wars as such. Take Sambanis (2004) for example. Sambanis compares 12 different operational definitions of civil war to show that there are notable differences in the coding of onset and duration. Such differences have substantial implications. By regressing the same independent variables on the 12 alternative civil war codings, he finds that parameter estimates vary both with regard to size and significance. Notable examples include ethnic fractionalization and oil exports, which sometimes do and sometimes do not significantly raise the propensity for civil war—depending on the operational definition of conflict (Sambanis, 2004). A more recent meta-study is Hegre & Sambanis’ (2006) sensitivity analysis of 88 independent variables used to explain civil war in the literature. The robustness

of each explanatory variable is assessed by regressing every operational alternative on civil war onset while systematically varying the combination of control variables. Generating a sample of 4.7 million logistic regressions, Hegre & Sambanis find that, amongst others, large populations, low income levels, and low rates of economic growth are particularly robust predictors of civil war. A variety of measures of ethnic difference, in contrast, are more sensitive to model specification (Hegre & Sambanis, 2006). At the same time as such meta-studies are assessing the large-N state of the art, the reinvention of the field is well under way.

Changes in civil war research are evident in moves to “disaggregate” and in moves to uncover “micro-foundations”—two approaches with considerable methodological overlay. The two moves have in common the ontological assumption that civil violence unfolds at a much lower level of aggregation than the country level. Theory and observation are accordingly adapted. Disaggregation may be understood as the collection and analysis of data that is more fine-grained by time and location than country-level data such as average income and population size. One early, if not the earliest effort is Buhaug & Gates’ (2002) exploration of determinants of the scope and location of conflict zones, using disaggregated data on the area and distance from the capital of fighting locations. Disaggregation has since advanced operationally. Witness Raleigh & Hegre’s (2005) effort to geo-reference by location and date all battle events in armed civil conflicts, Cederman, Buhaug & Rød’s (2006) application of geo-referenced data on the location and size of ethno-linguistic groups to model centre-periphery violent conflict, and Lyall’s (2007) use of source- and strike-location data on random artillery strikes by Russian forces in Chechnya to challenge the conventional wisdom that insurgent reprisals increase in their wake. Regardless of such operational advances, disaggregation is still premised on the expectation that real explanatory leverage is to be found below the country level.

In this sense, the micro-foundations community also uses disaggregated data. Yet its banner gives away a theoretical-operational commitment that sets it apart. Its objective is to explain behaviour in civil war on the individual level, and its operational response is to facilitate that by collecting micro-level data, be it on demobilized combatants in Colombia (Arjona & Kalyvas, 2007), perpetrators and victims of war-time homicides in the Argolid, Greece (Kalyvas, 2006), or small fighter units in Sierra Leone (Humphreys & Weinstein, 2006). Where disaggregationists are satisfied with sub-national yet still-aggregated data in cross-national samples, micro-foundationists insist on using data collection instruments such as surveys and interviews to gain leverage on the individuals involved. Their endeavour has some important commonalities, however. That is to address the ecological fallacies of the large-N research programme, and to get closer to the causal mechanisms involved.

Causal mechanisms is precisely what the large-N research programme on civil war is ripe for, and so my own prospective research on the mechanisms connecting transnational ethnic affinities to third-party military interventions in “ethnic” conflict is one response to this perceived need. Yet if one is to take mechanisms seriously, it requires a shift away from an effects-based understanding of causality. Mechanisms are at risk of attaining buzz-word status. At worst they are invoked simply to signal better theory while neither specifying what the term means, nor taking the operational consequences of aiming to uncover them. As Hovi

(2004) vividly illustrates, mechanisms is a diffuse concept. Heeding his advice to specify how one uses the term (Hovi, 2004, p. 85), mechanisms is here understood as a generalized and empirically verifiable formulation of the processes that connect cause (transnational ethnic affinities) and effect (foreign intervention in “ethnic” conflict). Mechanisms operate at an analytical level below that of a more encompassing theory, increasing the theory’s credibility by rendering more fine-grained explanations (Johnson, 2002, p. 230). This is an understanding of mechanisms much in line with Checkel (forthcoming). Citing Mayntz (2004, p. 241), he adopts the view that mechanisms are “*recurrent processes* linking specified initial conditions and a specific outcome” (Checkel, forthcoming, p. 2). In a footnote, Mayntz (2004, p. 241) provides the important clarification that “the notion of a recurrent process presupposes epistemologically that generalizable properties can be abstracted from concrete (historical) processes; it presupposes ontologically that (some) observable sequences of real events have similar properties”. In other words, mechanisms have a certain level of generality *and* they have observable implications.

Mechanisms make correlational theories—“transnational ethnic affinities raise the likelihood of intervention”—more plausible. They provide explanation. Mechanisms are causal, but they need not manifest themselves in correlations. Thus mechanisms constitute a different understanding of causality than the effects-based one. The latter understanding is adopted by King, Keohane & Verba (1994) who give succinct formulation to their position. “We define causality in terms of a causal effect: the mean causal effect is the difference between the systematic component of a dependent variable when the causal variable takes on two different values” (King, Keohane, & Verba, 1994, p. 85). According to this understanding, transnational ethnic affinities is a cause to the extent that its presence has an effect on the probability of intervention. Applying a mechanisms-based understanding of causality requires not only that one conceptualizes the more or less particular mechanisms that sustain the causal effect of transnational ethnic affinities, but also that one identifies the range of conditions under which one might reasonably expect those causal mechanisms to generate effects (Johnson, 2006, p. 247). This cannot be reduced to identifying appropriate intervening variables and interaction terms. As opposed to variables, whose values are observable *and* observed, mechanisms are *theoretical* microfoundations; explanation involves constructing models of *unobservable* causal mechanisms (Johnson, 2006, p. 248). They may however have observable consequences, and so it is natural to turn to operationalization.

### **Getting Operational**

Being operational about mechanisms requires a different methodological toolkit from the correlational template applied by disaggregationists and micro-foundationists alike.

Methodologists suggest that the appropriate method for uncovering the mechanisms that connect cause and effect is careful process tracing of one or a few cases (Checkel, forthcoming; George & Bennett, 2004, pp. 206-207) Beyond identifying causal mechanisms, process tracing helps narrow the list of potential causes, and it forces the investigator to take equifinality into account (George & Bennett, 2004, pp. 206-207). Process tracing of a case of intervention associated with transnational ethnic affinities should therefore be highly appropriate for the objective of uncovering causal mechanisms. My understanding of process

tracing is informed by Gerring (2007) and Checkel (forthcoming) rather than George & Bennett (2004), who seem to equate it with any investigation into causal mechanisms. “The hallmark of process tracing...”, Gerring (2007, p. 173) argues, “...is that multiple types of evidence are employed for the verification of a single inference—bits and pieces of evidence that embody different units of analysis (they are each drawn from unique populations). Individual observations are therefore noncomparable”. Process tracing thus involves a variety of evidence that in its totality does not fit into a neat, row-and-column, dataset structure—be it qualitative or quantitative. The need for different sorts of evidence is echoed by Checkel (forthcoming, p. 7), whose method is to employ “multiple data streams” to allow for extensive cross-checking. Gerring (2007) and Checkel (forthcoming) thus both emphasize that process tracing involves collecting a variety of juxtaposable but not necessarily comparable evidence, and that data needs are carefully derived from a pre-theorized set of mechanisms.

The suggestion, then, is to apply process tracing to a single case of intervention in “ethnic” conflict associated with transnational ethnic affinities in order to develop a mechanisms-based explanation. It would be unfortunate to do so in total isolation from the cross-national large-N sample of interventions of which we know individual interventions to be a part. More generally, potential for mutual learning would go untapped if single cases of conflict were studied without relating them to findings emanating from the vigorous large-N research programme on civil war—be they more or less robust. But how to? In this case—how to relate a single case of intervention to large-N findings on the co-variates of interventions in civil war (for example Davis & Moore, 1997; Nome, 2007; Regan, 2000; Saideman, 2002)?

### **The Allure of Nesting**

One solution is to “nest” the case study within a large-N sample of interventions. Lieberman’s (2005) article on nested analysis as a mixed-method strategy for comparative research is cited as one of the few methodological contributions on how to do this (Bennett, 2007, p. 9). Lieberman (2005, pp. 435-436) portrays nested analysis as a unified mixed-method approach to comparative research that combines statistical analysis of large-N samples with in-depth investigation of one or more cases taken from that sample. Applying his vision to this project, it would be to combine the statistical analysis of a large cross-national sample of interventions with the case study of one or a few interventions. Lieberman’s contribution is to show how particular cases can be explicitly embedded and positioned within their large-N samples, thus offering an operational answer to the call from case study methodologists who insist that cases should be selected with prior knowledge of the universe. This is a lesson to retain. Yet if the objective is to use the case study to uncover causal mechanisms, then this is as far as Lieberman takes us. His suggestions on how to connect large-N and case analysis are based on an effects-based understanding of causality. This is apparent in claims such as “the elaboration of concepts and mechanisms can best be accomplished through *comparison*” and “comparison provides an empirical basis for making narrative assessments of counter-factual claims—that is, an event would have happened a different way had the score on a key variable or set of variables been different” (Lieberman, 2005, p. 441, italics in original). The emphasis on comparison is incompatible with the nature of process tracing—the methodological foundation of mechanisms-based explanation—and the counter-factual logic indicates a view

of cause as co-variate. Lieberman's use of examples reinforces this impression. When questions of causality arose, cases were selected based on different scores on the independent variables, tracing their varying "impact" on the dependent variable (Lieberman, 2005, p. 444). Finally, theory building is viewed solely as finding new explanations for deviant cases—new causal factors, or new co-variates—not as uncovering and theorising new causal mechanisms (Lieberman, 2005, p. 445). Using the image of cases' position relative to the regression line, Lieberman (2005, p. 445) argues that "only when the scholar has good reason to believe that a particular case is "on-the-line" for entirely spurious reasons would it be useful to select such a case for [model-building small-N analysis]". If one moves from an effects-based to a mechanisms-based view of causality, however, cases on the regression line that are well and *non-spuriously* explained by our multi-variate models may be very fruitful indeed for theory building (Ragin, 2006).

In an offering that is more open to different causal logics, Gerring & Seawright (2007) specify a range of nesting strategies where the techniques for choosing cases vary according to theoretical objectives. The cross-case selection of single cases holds great allure for the multi-method-minded researcher. It promises to give cases particular analytical—and inferential—value by explicitly positioning them with regard to their universe, it should give both quantitative and qualitative research a broader audience, and it allows for the combination of different understandings of causality. No small feat. The synergistic potential of combining large-N and small-N research is at the heart of Gerring & Seawright's approach to nested analysis: the goal of case selection is the same regardless of sample size—to identify the cases that reproduce the relevant causal features of their universe (Gerring & Seawright, 2007, pp. 87-88). The central perspective is yet again evident when they state that "case study analysis does not exist, and is impossible to conceptualize, in isolation from cross-case analysis" (p. 90).

Smitten by the promise of nested analysis Gerring & Seawright-style, it is important to note several conditions on doing it. Yet the advice here is rather general. One learns from a combined reading of Gerring & Seawright (2007) and Gerring (2006) that cross-case selection of cases only is fruitful when a) inference pertains to more than several cases, b) one's multi-variate model of cross-case variation is a good depiction of reality, c) the measures are valid and reliable, d) the statistical technique is appropriate to the phenomenon being studied, and e) the population of outcomes is reasonably homogenous—the units of observation are in general cases of the same thing (Gerring, 2006, p. 719; Gerring & Seawright, 2007, pp. 90-91). Yet when trying to actually apply their cross-case selection techniques, foundational issues arise that warrant more discussion.

### **The Pathway Case**

The following section discusses some of these issues as they pertain to one particular nesting strategy—the pathway case. I outline the pathway case selection technique, argue that it constitutes multi-method research on two dimensions, and discuss possible solutions to some of the problems that arise. Let me first justify my focus on the pathway case.

Of the nine different case selection techniques that Gerring & Seawright sketch, the pathway case warrants particular attention given the objective of uncovering causal mechanisms (Gerring & Seawright, 2007, pp. 122-131). A pathway case can provide uniquely penetrating insights into mechanisms when causal hypotheses are clear—“transnational ethnic affinities cause interventions”—and when hypotheses are confirmed by cross-case analysis—as several contributions (Davis & Moore, 1997; Nome, 2007b; Saideman, 2002) suggest they are (Gerring & Seawright, 2007, p. 122). The pathway case is a case where the causal effect of the factor of interest can be isolated from other potentially confounding factors, the logic being that a case in which one knows a particular causal factor to be dominant should be ideally situated to shed light on the mechanisms involved. The pathway case builds on prior cross-case analysis, and case selection has to be situated within the sample (Gerring & Seawright, 2007, p. 122).

Situating case selection within the sample is meant literally. Based on Gerring & Seawright’s (2007, pp. 126-127) description of the procedure for identifying potential pathway cases, I use as an example a multinomial logit regression model of interventions in civil wars applied to a dataset of 1721 country dyads, as described in Nome (2007b). The model indicates that, controlled for possible confounding factors, transnational ethnic affinities (TEA) are a significant determinant of interventions. The model suggests that states are likely to intervene in favour of the side in civil wars that their dominant ethnic group has ties with, be it government or rebels (Nome, 2007b).

The pathway case has to satisfy two criteria with regard to the distribution of cases in a fitted model. First, it should not be an outlier in the general model (Gerring & Seawright, 2007, p. 126). That is, the estimated value of the dependent variable for the case should be close to its actual outcome. Second, the estimated outcome should be strongly influenced by the variable of interest, TEA, taking all other factors into account (Gerring & Seawright, 2007, p. 126). The image is of a case that lies relatively far from the regression line in a reduced model, but is “pulled” close to the regression line by including TEA in the model. Meeting these criteria requires a careful look at the residuals for each case. This involves comparing the size of the residuals for each case in a reduced model to the size of the residuals for each case in a full model (Gerring & Seawright, 2007, p. 127). In the set of cases with smaller residuals in the full model than in the reduced model, the possible pathway cases are those for which the residuals display the greatest change.

Consider the example of intervention in civil war. Expressing the logic more formally, I first estimate the reduced model

$$Z = \alpha + \beta_{\mathbf{X}}\mathbf{X},$$

where  $Z$  is the log odds of the intervention outcome of interest and  $\mathbf{X}$  is the set of control variables. I then estimate the full model

$$Z = \alpha + \beta_{TEA}TEA + \beta_X X,$$

in which I add TEA—the variables associated with transnational ethnic affinities. Converting the logits to estimated probabilities,  $\Pr(Y=1)$ , for each case of intervention where transnational ethnic affinities were present ( $TEA=1$ ), I then calculate the residuals for both the reduced and the full model,  $Y(0 \text{ or } 1) - \Pr(Y=1)$ , and determine the change,  $Res_{full} - Res_{reduced}$ . Table 1 reports the results of this exercise, and includes the cases of intervention where transnational ethnic affinities were present such that the ethnic group in power in the intervener had ties with either government or rebels in the target state. The underlying models are provided in the appendix.

*Table 1: Possible Pathway Cases*

<b>Intervener</b>	<b>Civil war</b>	<b>Intervention in favour of...</b>	<b>Res<sub>reduced</sub></b>	<b>Res<sub>full</sub></b>	<b>ΔResidual</b>
Syria	Lebanon 1958	Rebels	0.56	0.11	-0.45
Greece	Cyprus 1963-1964	Government	0.98	0.74	-0.24
Turkey	Cyprus 1963-1964	Rebels	0.82	0.18	-0.64
Syria	Lebanon 1975-1988	Rebels	0.55	0.10	-0.45
Greece	Cyprus 1974	Government	0.98	0.84	-0.14
Turkey	Cyprus 1974	Rebels	0.79	0.15	-0.64
Iran	Iraq, Shi'i insurgency 1984-2003	Rebels	0.85	0.28	-0.57
Pakistan	India, Sikh insurgency 1985-	Rebels	0.91	0.34	-0.57
Yugoslavia	Bosnia 1992-1995	Rebels	0.49	0.05	-0.44
Russia	Moldova 1992-	Rebels	0.91	0.41	-0.50

The table indicates that the greatest change in residuals is associated with the two cases of Turkish intervention in Cyprus. Iran's intervention in Iraq, and Pakistan's intervention on Indian territory also display relatively large changes in the residuals. Judging by the criterion that a pathway case ought to lie as close as possible to the regression line in the full model,

the two cases of Turkish intervention have the lowest residuals and may therefore be good pathway case candidates.

### **A Two-Way Mix**

The pathway case is a mixed-method strategy in two ways. First, it mixes quantitative and qualitative data in a sequential design. Quantitative data is first used in the cross-case large-N analysis to identify possible pathway cases. Then qualitative data is collected during the case study to shed light on the mechanisms involved. Statistical analysis thus precedes process tracing. Yet by being open to alternative causal logics, the pathway case also mixes epistemologies. Statistical analysis is often associated with a positivist epistemology, involving a “preoccupation with verification and observation, skepticism toward causality and explanation, and distrust of theory insofar as it invokes unobservable factors” (Johnson, 2006, p. 231). By nesting a mechanisms-seeking case study in a large-N sample, the pathway case embeds a pragmatist epistemology that is quite comfortable with unobservable theoretical entities such as causal mechanisms within a large-N sample typically viewed through positivist lenses. There is nothing intrinsically positivist about quantitative research, however (Johnson, 2006, p. 240). Statistical analysis and mechanisms-based explanation are quite compatible in a nested design, where research strategies are employed to identify the mechanisms that can explain the robust regularities uncovered by quantitative research. As a mixed-method strategy, the pathway case should be well placed to do this. Yet selecting a pathway case involves some difficulties that must be addressed and resolved.

### **The Trouble With the Pathway Case and How to Solve It**

I here address four issues: the question of parameter estimates, the problem of measurement validity, the indeterminacy of case selection, and the conundrum of generalizability. The bottom line, I suggest, is to go ahead with the pathway case but not to claim too much.

#### *The Right Effects?*

Let us for the moment assume that we have highly valid measures for all the variables needed in the cross-case model. How can we be confident that the parameter estimates are right, or at the least precise enough? Selecting a pathway case requires that the effect of the variable of interest is known—at least as it relates to other explanatory variables. Yet the magnitude of estimated effects varies not only as a function of variation in the systematized concepts (Adcock & Collier, 2001) that are measured, but also as a function of the variables that are included in the models, as well as the statistical assumption that are applied. In order to build confidence about case selection, both questions of model specification and statistical assumptions must be considered and addressed.

Model specification concerns in this context which variables to include when estimating the models to base case selection on. The first priority should be to include the necessary control variables in order to isolate the effect of the variable of interest as much as possible. The next step should be to assemble a model that gives a plausible account of variation in the dependent variable. Either task can only be solved by thinking carefully about which variables to include with reference to existing theory and empirical research. The key here is not to

assemble the ultimate model—no such model exists—but to build confidence that both the reduced and the full model make sense.

Beyond model specification one should also question whether the statistical assumptions on which we base our model estimates are reasonable in light of the phenomena we study. Looming particularly large here are questions of the independence between units of observation—and time.

Any introductory text on statistics will emphasize that the sampling procedures on which the estimation of regression coefficients and standard errors are based assume that the units of observation are independent of each other. Thus, the estimation of the multinomial logit models of intervention used in the example of case selection are based on the assumption that countries decide to intervene or not to intervene regardless of how they observe or expect other countries to behave. This is a dubious if not absurd assumption with unknown consequences for coefficient estimates and standard errors. The appropriate response—short of abandoning the pathway case altogether—is to use statistical techniques that better approximate the phenomenon they model. Raknerud & Hegre (1997), for example, have shown how dependence between units of observation can be modelled directly by including variables that account for inter-country dependence in a continuous time Cox regression model.

Raknerud & Hegre (1997) also argue that the country-dyad units of observation included in logit models such as the example here are assumed to have a stationary propensity for conflict. In other words, the probability of intervention is implausibly assumed to be independent of time. As one possible response, the statistical technique they propose as an alternative allows the baseline risk of intervention to vary with time.

This is only to show that there are ways of improving on the foundational shortcomings of certain statistical techniques in order to estimate models that conform more closely to reality. In sum, the quality of the models used to select pathway cases can be maximized by paying careful attention to the choice of variables and the assumptions behind statistical analyses.

### *Valid Measures?*

Let us assume then that we have satisfactory estimates of the effect of the variable of interest, as well as a clear idea about the prediction of which cases is best improved by including the variable in the model. What if there is reason to be skeptical of the measurement validity of key variables? What if doubt is being heaped on whether the proxies sufficiently represent their systematized concepts? In less abstract terms, what if the outcome of interest is not caused by what our measures and their effects lead us to believe? This is no trivial question. Cross-case selection is not useful unless the researcher feels reasonably confident about the accuracy and conceptual validity of the central measures (Gerring & Seawright, 2007, p. 91).

The answer would seem to be two-fold. First, there is no way getting around that selecting a pathway case requires a minimum of confidence that the central proxies—variable of interest, control variables, and dependent variable—measure what they are supposed to measure. If it is to be meaningful to uncover the causal mechanisms that explain a known causal effect, then

the social phenomena captured by the systematized concepts must co-vary, not only the indicators purported to measure them. This is an obvious point, and the way to go about it is to apply standard procedures for content validation (see for example Adcock & Collier, 2001).

Second, there should be a minimum of consensus in the research community that the central measures are reasonable proxies, particularly if one cannot make a water-tight case for the validity of a novel measure. By a minimum of consensus I mean that the measures are applied by other researchers, at best in published material. I am by no means an adherent of truth by consensus, that is I do not argue that this is a guarantee for measurement validity. The logic here is that if one can establish a minimum of content validity for the key variables, and if there is a minimum of community confidence in the sensibility of the measures, then one can be reasonably sure that—in a pathway case—one is exploring causal mechanisms that generate co-variation of interest to the community.

### *One Pathway Case?*

The archetypical pathway case is based on the idea of causal sufficiency (Gerring & Seawright, 2007, p. 122). In those terms, the pathway case is that for which all known independent variables suggest that no positive outcome is observed, and where the variable of interest is sufficient to explain the positive outcome that actually is observed. In other words, the pathway case would be an intervention caused solely by transnational ethnic affinities. Given the complexity of international relations and the multiplicity of processes leading to particular interventions, causal sufficiency for transnational ethnic affinities seems a bit much to ask. Does that mean that the idea of the pathway case is no longer valid? I think not. The solution is to moderate the image of the archetypical pathway case by thinking of it as the extreme on a continuum, and by thinking carefully about generalization.

First, one should give up on trying to find *the one* pathway case where the variable of interest is a sufficient explanatory factor. Rather, one should try to identify cases that are closer to the ideal than others. Depending on the state of one's research programme, mechanisms-based explanation may be highly appropriate—it certainly is here. If that is so, then selecting a case for process-tracing that is close rather than far from the ideal pathway case is better than selecting a case with no sense of its position in the universe at all.

However, as one moderates one's demands on the pathway case, one must also revise its potential for generalization. Consider the ideal pathway case where—in this project—transnational ethnic affinities is a sufficient cause of intervention. Assuming that transnational ethnic affinities cause interventions by similar mechanisms in most instances, then the pathway case should be the ideal foundation for generalizing about those mechanisms. When a case deviates somewhat from the ideal—as most cases do—then it becomes more difficult to isolate the mechanisms associated with transnational ethnic affinities, and as a consequence more difficult to infer about them to a broader population. Even so, all hope should not be lost. Process tracing a less than ideal pathway case may circumscribe generalization, but by carefully looking to distinguish the general from the unique properties of an historical instance, generalization can be maximized within the limits imposed by one's case.

### *How Generalizable?*

As much as the question of how pathway a pathway case is determines its external validity, there is the more basic question of whether one can infer about mechanisms at all. The question is pertinent because the very premise for the pathway case nesting strategy is to position a case so as to make its mechanisms generalizable. There would be little point otherwise. This is what sets the pathway case apart from Lieberman's (2005) small-N theory-building design, where the sole purpose is to uncover new co-variables that may be fed back into the underlying cross-case model.

One writer on mechanisms focuses on their inherent indeterminacy (Elster, 2007, p. 36), which does not bode well for generalization. Others are more optimistic. Seeing possibilities for theorising mechanisms out of their indeterminacy, they suggest that the key is to specify scope conditions: "the theoretical challenge consists in not merely conceptualizing the more or less particular mechanisms that sustain causal explanations but also *identifying the range of conditions* under which we might reasonably expect those causal mechanisms to generate effects" (Johnson, 2006, p. 247, emphasis added. See also Zürn & Checkel, 2005). Devising a research strategy that can specify scope conditions should brighten the prospects for generalizing about mechanisms.

### **The Bottom Line**

The above discussion suggests that it is fruitful to go ahead selecting a pathway case, but that claims to inferential power must be moderated in light of the indeterminacies of the nesting process. Mechanisms may not be readily generalizable, requiring the specification of scope conditions; no one ideal pathway case may exist, so the external validity of candidate cases must be circumscribed accordingly; the measurement validity of central variables may be questioned; and statistical techniques depend on assumptions about their units of analysis that may deviate more or less from the phenomenon at hand. There are ways to address all of these issues, but the fact remains that using the pathway case nesting strategy requires a carefully portioned amount of humility—neither claiming too much nor too little.

In other words, do it! The pathway case nesting strategy exemplifies how methods can be mixed by sequentially employing quantitative and qualitative data, statistical analysis and process tracing, and by embedding a mechanisms-based causal logic within an effects-based causal framework, in order to develop better explanations. Such a strategy is an appropriate response to the large-N research programme on civil war in which this research project has its roots. Statistical analyses suggest that transnational ethnic affinities is a co-variate of third-party interventions in "ethnic" conflicts. Time to ask—which mechanisms?

## Appendix

*Table 2: Underlying Models for Comparison of Residuals. Multinomial Logistic Regression Estimates, Probability of Interventions in “Ethnic” Civil Wars in Support of Either Government or Rebels<sup>2</sup>*

	Reduced model			Full model		
	<b>G</b> <i>Intervention in favour of government</i>	<b>R</b> <i>Intervention in favour of rebels</i>	Test for $\beta_i^G = \beta_i^R$	<b>G</b> <i>Intervention in favour of government</i>	<b>R</b> <i>Intervention in favour of rebels</i>	Test for $\beta_i^G = \beta_i^R$
	$\hat{\beta}$ (SE)	$\hat{\beta}$ (SE)	$\chi^2$ (sig.)	$\hat{\beta}$ (SE)	$\hat{\beta}$ (SE)	$\chi^2$ (sig.)
<i>Dominant-Marginal dyad</i>				-3.206*** (0.713)	3.206*** (0.713)	20.19*** (<0.00005)
<i>Marginal-Dominant dyad</i>				dropped	-0.434 (1.038)	0.17 (0.676)
<i>Dominant-Dominant dyad</i>				3.805*** (1.479)	-0.877 (1.185)	7.23*** (0.007)
<i>Marginal-Marginal dyad</i>				3.805*** (1.479)	-0.877 (1.185)	7.23*** (0.007)
<i>Joint borders</i>	1.947*** (0.510)	1.947*** (0.510)		1.619** (0.719)	1.619** (0.719)	
<i>ln Distance</i>	-1.235*** (0.316)	-1.235*** (0.316)		-0.945*** (0.366)	-0.945*** (0.366)	
<i>ln Power ratio</i>	.496*** (0.170)	0.040 (0.180)	4.16** (0.041)	0.946*** (0.299)	-0.103 (0.177)	11.56*** (0.001)
<i>ln Capabilities<sub>t</sub></i>	0.311 (0.205)	0.311 (0.205)		0.308 (0.204)	0.308 (0.204)	
<i>Colonial history</i>	2.338*** (0.540)	2.338*** (0.540)		2.486*** (0.818)	2.486*** (0.818)	
<i>Cold War</i>	1.072*** (0.411)	1.072*** (0.411)		1.147** (0.470)	1.147** (0.470)	
Constant	3.508 (3.380)	4.791 (3.321)		-1.196 (4.547)	2.564 (3.575)	
N	1721			1721		
Log pseudolikelihood	-102.767			-82.978		
Pseudo-R <sup>2</sup>	0.365			0.487		

<sup>2</sup> The full model is described in Nome (2007b). The reduced model is estimated using the same procedure on the same sample.

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