Where the Model Frequently Meets the Road: Combining Statistical, Formal, and Case Study Methods

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Abstract: This paper analyzes the working or default assumptions researchers in the formal, statistical, and case study traditions typically hold regarding the sources of unexplained variance, the meaning of outliers, parameter values, human motivation, functional forms, time, and external validity. We argue that these working assumptions are often not essential to each method, and that these assumptions can be relaxed in ways that allow multimethod work to proceed. We then analyze the comparative advantages of different combinations of formal, statistical, and case study methods for various theory-building and theory-testing research objectives. We illustrate these advantages and offer methodological advice on how to combine different methods, through analysis and critique of prominent examples of multimethod research.
The great potential for synergistic research combining the three leading methodological traditions in political science—formal modeling, statistics, and case studies—is not being fully realized. There is much to be gained by combining alternative research methods, as each method can complement the others. Formal models can provide deductively valid and counter-intuitive insights, but they are not an empirical method and must be combined with either statistical or case studies to become so. Case study methods can use process tracing to look for detailed evidence on causal mechanisms and causal processes within cases, but they face limitations in generalizing these results to cases not studied. Statistical studies can identify patterns and correlations in specified populations, but they lack the guarantee of logical theoretical consistency that formal models can provide, and they can benefit from the rich appreciation of processes and contexts that case studies can provide. Combining methods provides opportunities for the development and testing of theories that no single method can match.²

Yet the promise of multimethod work has been only partly fulfilled because of methodological challenges, institutional barriers, and conceptual limitations. These include the technical difficulties of each method alone and the even greater difficulties of combining them, the disincentives to co-authorship in the discipline, and the relative neglect of some possible combinations of methods and kinds of theory-building. There is also the danger that in published reviews (and even more importantly in job talks) researchers carrying out multi-method work

² For analyses of the contributions different methods have made to specific research programs, see Sambanis (2004) on civil wars, Back and Dumont (2007) on political coalitions, Hafner-Burton and Ron (2009) on human rights, and George and Bennett (2005) on the inter-democratic peace.
will be unfairly held to impossible standards and viewed as falling between three stools: not sufficiently up on the latest models for the formal modelers’ tastes, not on the cutting edge of the very latest statistical methods, and not sufficiently deep into the archives and interviews and historical context to satisfy the qualitative researchers. Perhaps the most pervasive inhibition on multimethod work is conceptual: researchers in each methodological tradition have tended to share methodological assumptions that clash with the assumptions adopted by practitioners of alternative methods.

Barriers such as these have kept multimethod work from growing even as each methodological tradition has become more refined in the last two decades. One survey found that from 1975 to 2000, the proportion of articles using more than one method remained steady, ranging between 15% and 19% of the articles surveyed. Moreover, these multimethod articles rarely used some combinations of methods. Most (between 50% and 90% in different time periods) combined formal models and statistical research, but only 33 to 50 percent combined statistics and case studies, and only 5 to 16 percent combined formal models and case studies (Bennett, Barth, and Rutherford, 2003). Similarly, despite the great volume of literature explicating each methodological tradition, scholars have written very little on how to combine methods.

We explore one explanation for this disjuncture that arose in our analysis of examples of multi-method work: substantial and often implicit differences in the methodological assumptions that researchers from each tradition typically hold regarding the sources of unexplained variation, the meaning of outliers, parameter values, human motivation, functional forms, time, and external validity. We argue that these working assumptions are often not essential to each

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3 There is of course a diversity of opinion on these issues among methodologists and practitioners of each methodological tradition. We hope only to paint an accurate portrait of views commonly held within each tradition,
method, and that there is much to be gained by relaxing them in ways that allow multimethod work to proceed. We then analyze the comparative advantages of different combinations of methods for various theory-building and theory-testing research objectives. We illustrate these advantages, and offer methodological advice on how to combine different methods, through analysis and critique of prominent examples of multimethod research.

**The Clash of Assumptions**

One of the major obstacles to integration across methods is the seeming incompatibility among the assumptions widely held by practitioners of different methodological traditions. While these assumptions are thoroughly embedded in each methodological tradition and are usually treated as unproblematic assertions about the ontology of the social world, they are in fact often merely simplifying working assumptions that, albeit convenient, are not usually necessary for research to proceed. We cannot ultimately know, for example, whether a given phenomenon is produced by a deterministic process or an inherently stochastic process, as in either case our observations will include some unavoidable amount of measurement error and model mis-specification. At times we proceed by making working assumptions, often implicit ones, on such issues as we proceed in our research. The danger lies not in making such assumptions but in reifying them, thereby rendering them both invisible and inviolable. It therefore behooves us to examine the key working assumptions held by researchers in each

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not to imply that these views are universal within each research community. Also, many methods contribute to “multimethod” research, including experiments, surveys, ethnographic studies, simulations, and other approaches. We focus on the three broad methods of case studies, formal models, and regression analysis for reasons of space.
methodological tradition and to explore how they can be relaxed or adapted in ways conducive to multimethod research.⁴

**The Sources of Unexplained Variance**

Whether couched as abstract arguments over the fundamental nature of causation or concrete arguments about the sources of error in the dependent variable, disagreements over assumptions about the sources and magnitude of error in the dependent variable are a persistent feature of methodological discourse.⁵ The most extreme divergence here is between qualitative and quantitative methodologists. For quantitative methodologists, dependent variables in the social world are typically thought to be inherently and unproblematically stochastic: like electrons fired at gates, their behavior can be predicted in the aggregate but not in any particular case. For this reason, the criticism that this or that case does not fit the story captured by the statistical model is not seen as persuasive: it is to be expected that some cases will not fit the model, simply because their outcomes cannot be perfectly predicted. Even “failure” in a case in which the predicted probability of a “success” is 0.9 says little about the quality of the model, as such events should happen one time out of ten. Only the aggregate results are seen to be convincing. In relying on the underlying assumptions of the central limit theorem, statistical researchers assume that a large number of small and relatively non-influential variables will produce a normal error term with occasional large outliers.

At the other end of the spectrum, case study researchers focus to a much greater degree on the systematic components of the theory and are inclined to demand that any apparent

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⁴ For a similar discussion of the differing assumptions of statistical and case study researchers, see Goertz and Mahoney, 2006.
⁵ As Hall (2003) demonstrates, accounts of the empirical implications of causal relations dovetail all too well with descriptions of the empirical patterns that different methods are designed to unearth.
randomness in outcomes be explained in detail. Instead of taking a fundamentally stochastic world for granted, as do many researchers in the statistical tradition, case study researchers problematize this assumption. Absent some convincing rationale, quantum fluctuation is thought to be a property reserved for electrons rather than people, and case study researchers seek strong causal explanations of cases on the assumption that theoretically intelligible social behavior rather than quantum variation accounts for outcomes.

Interestingly, formal modelers are of two minds on this issue. Most equilibrium solutions in formal models are deterministic, but observed social realities clearly do not neatly conform to this expectation. Thus, modelers have found it necessary to devise equilibrium concepts that can accommodate out-of-equilibrium observations. Consequently, one category of formal models, largely outside of the rational choice tradition, describes outcomes in stochastic terms. Agent-based evolutionary modeling is perhaps the best example of this approach (Cederman, 2001, Weibull, 2002). In this kind of model, the agents' “success” in their environment affects the frequency of reproduction, but the process is inherently stochastic. Sometimes rational choice theories predict stochastic outcomes as well: a basic mixed-strategy equilibrium, for example, is based on the idea that each player will randomize in order to keep the other indifferent between one strategy and the other (in the classic example, speeders will speed with a probability that keeps police indifferent between enforcing and not enforcing speed laws, while police will enforce with a probability that keeps speeders indifferent between speeding and not speeding).

Even in cases in which the model points to a dominant strategy for all players, formal theorists have posited that some randomization may occur as the result of variations in utility that
are observed by the player but not by the scholar. Due to such variations, players are assumed to choose those strategies deemed optimal by theorists with a greater probability than they would non-optimal ones, but not with probability of 1. This is the concept of a quantal response equilibrium (QRE) (Morton, 1999).

Despite the apparent divergence in views about the sources of error, a more fundamental approach to the topic indicates that differences in common working assumptions about probabilism and determinism need not impede multimethod research. First, as Gary King, Robert Keohane, and Sidney Verba argue, ultimately we cannot know whether the social world that we imperfectly observe is a deterministic world measured with error or an inherently stochastic world. All researchers should be humble regarding their working assumptions on determinism and probabilism. Second, as Keohane, King, and Verba also note, the differences in assumptions on this issue between the different methodological traditions are not necessarily as sharp as they might appear. These authors suggest that while most physicists and economists tend to adopt probabilistic assumptions, most statisticians are closer to a deterministic perspective, in which most or all of the error term could in principle be accounted for by variables left out of our equations (King, Keohane, and Verba, 1994:59-60).

Keohane, King, and Verba do not offer any survey data on the default assumptions of different research communities regarding probabilism and determinism, however, so their assertion on this point remains an untested hypothesis. The more central point here is that one can in fact pursue statistical research with deterministic assumptions, or case study methods with probabilistic ones. Charles Ragin’s “fuzzy set” methods, for example, which are oriented toward research projects involving an intermediate number of roughly between ten and fifty cases,

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6 See Smith (1999) for a clear discussion of this point.
which is too many cases for detailed case studies and too few for standard statistical analysis, can be interpreted as a way to allow probabilistic measures in qualitative work (Ragin, 2000). What really matters is not any general assumption a researcher makes about whether the social world is stochastic, but the pragmatic implicit working assumption that the researcher makes when he or she decides at what point to stop in seeking to reduce or explain the error term in a particular research project. If a researcher using statistical methods decides to explore whether adding one more variable will improve their model, they are implicitly assuming that the world may be at least a little more deterministic than their initial model indicates. If a researcher undertaking a case study decides to stop pursuing every anomalous case or every unexpected aspect of a case, they are behaving no differently than if they had decided that this is the place to allow for the possibility that the remainder of the error term is generated by a stochastic process.

The real question, then, is how to decide when to stop pursuing an explanation of the error term, and it is on precisely this question that the advantages of multimethod work are evident. A researcher using statistical tests who suspects that the error term may be legitimately reduced by adding an additional variable might look at an outlier case (or several such cases) to uncover a candidate variable, or justify an added variable by demonstrating that it not only improves the statistical fit but is consistent with the process-tracing evidence from intensive study of a case from the sample. A case study researcher worried that s/he might be generating a “just so” story in trying to explain an anomalous case by adding a variable might use a statistical test to assess whether the additional variance explained outweighs the decrease in parsimony, or more ambitiously, whether the new variable improves the fit of the model. A game theorist who hypothesizes that one branch of a game tree resembles a Prisoner’s Dilemma but who finds through statistical analysis that the variance of outcomes at that branch is significantly greater
than anticipated might reasonably suspect either that the measured payoffs do an unusually poor job of capturing actual payoffs or that the structure of the game at that branch has been misspecified. A detailed heuristic case study or two could go a long way toward improving understanding of the players’ behavior at this juncture.

The Meaning of Outliers

On a second and related issue, each method views outliers---cases that are significantly mispredicted by a model---in a substantially different ways, depending largely on whether the outcome is thought to be stochastic. Individual outliers in statistical analyses are typically not thought to be as informative as patterns of outliers: if a linear model systematically overpredicts at low and high levels of some independent variable but underpredicts at intermediate levels, for example, a curvilinear functional form is worth exploring. It is tempting to assume, as a case study researcher typically would, that large outliers represent cases that are poorly explained by the theory and that small outliers represent cases that are well explained by the theory, and therefore that exploring the largest outliers will have some heuristic value in repairing or improving the theory. This may be so. It may also be the case, though, that large outliers represent nothing more than a case with a very substantial error component---a perfectly normal, even expected, occurrence. By the same token, a dramatic misprediction plus random noise could just as easily produce an artificially small outlier.

In the case of formal models that involve stochastic outcomes, the concept of an outlier is meaningless, or at a minimum uninteresting in isolation, for the reasons just described.\footnote{This generalization does not apply to cases in which, for example, a player is thought to be randomizing between options C and D but instead chooses out-of-equilibrium option E; in this case, an outlier is both meaningful and interesting.} In most
rational choice models, however, outliers represent out-of-equilibrium behavior. A prisoner playing C rather than D in a one-shot Prisoners' Dilemma, for example, would be a problem for a theory that predicts that such behavior will never occur. At the same time, given the noisy nature of data in IR and the ready acceptance by most rational choice theorists that the world contains something more than what their models capture, falsification of a rational choice model based on a single outlier, especially if many cases conform to the theory's predictions, would be unwise. Moreover, models such as those that utilize QRE as a solution concept (see above) are often designed to take such factors into account.

For case study researchers, a common working assumption is that outliers are fruitful cases to study for the purpose of inductively finding left-out variables, overlooked interactions effects, or mistaken assumptions about the functional form of the relationships among the variables. Case study researchers are also attentive to the possibility that measurement error may lead a case to be mis-classified as an outlier, but they tend to downplay the possibility that stochastic processes may also account for outliers.

Here again, we argue that researchers from each tradition should refocus on the more fundamental possibility that in principle the outliers in any particular piece of research could be any of the above: the product of stochastic processes, the result of measurement error, the consequence of a variable that affects the outcome of the outlier case and also those of other cases in the sample, or the consequence of a variable that is unique to the outlier case or nearly so and that may have explanatory power for the case even if it does not significantly improve the statistical fit for the sample.8 Combining different methods provides more powerful means than

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8 Another way to put the differences among researchers regarding outliers concerns their different views of the “unit homogeneity” assumption. Statistical researchers and formal modelers tend to assume that unit homogeneity, or at least the weaker version of constant causal
any one method used alone for discerning which of these holds true. Statistical methods help identify which cases are outliers relative to a particular model. Case studies of outliers can help test if measurement error is involved, and can also identify candidate variables that might explain why the outcomes in these cases deviate from the model’s expectations. Formalization of these variables can help elucidate their logic and clarify the forms they might take in statistical equations. Statistical studies using these new variables (and omitting the cases from which they were derived) can then help determine if they improve the fit for a wider sample of cases. Even if a variable inductively identified from a case does not improve the statistical fit for the rest of the sample, it might still explain the outlier case if it is relatively unique to that case and if it is backed up by convincing logic and process-tracing evidence from that case. If, for example, an individual voted for a candidate despite ideological, party, and socio-economic variables that led us to predict the individual would oppose the candidate, but we find through a case study that the voter and the candidate are blood relatives, we may feel confident that we have explained this anomalous case even if we do not expect that blood relationships between the candidate and the voters were sufficiently common to influence the outcome of the election.

A more difficult methodological challenge is that it is hard to identify cases that are in fact outliers relative to a statistical model but do not appear to be so because of measurement error or stochastic processes. One possibility, which we have not seen discussed, would be to use Bayesian model averaging (Bartels, 1997) for an unorthodox purpose: to generate model-averaged error terms that would represent our best guess on the size of the error term across effects, will hold for large populations of cases. Case study researchers are more sensitive to the possibilities of “equifinality” (multiple causal paths to the same outcome), “multifinality” (multiple outcomes associated with any one value of a variable), and general heterogeneity among cases that are influenced by numerous variables and interactions among these variables. Thus, case study researchers are more likely to break populations down into narrower sub-types
multiple model specifications. While not a cure-all, especially if the irreducibly stochastic component of the error term is large, such a technique could at least minimize error due to model mis-specification and represent an improvement over simple residuals. It might be possible, and more straightforward, to identify such cases by exploring whether statistical results are robust in the face of slightly different measurements of the variables. Even if such different measures have roughly equivalent fit for the whole sample, they may point to different cases as possible outliers for examination in detailed case studies.

Parameter-Value Assumptions

Anyone who has engaged in formal modeling realizes that the implications of a model, even a fairly straightforward one, become dramatically more complex as the number of included variables rises. Moreover, readers are typically left cold when the implications of the model can only be expressed in page-long systems of convoluted equations. The temptation exists, therefore, to make life (and equilibrium solutions) easier by making a few simplifying assumptions about the magnitude of some of the model's parameters, either in absolute terms or relative to one another.

From a statistical point of view, assumptions about parameter values are rarely made, and when they are, they are typically made for an entirely different reason, e.g., to permit estimation in cases in which it would otherwise be difficult or impossible. For example, standard probit techniques do not actually estimate a beta coefficient on its own; rather, they estimate beta/sigma, that is, the ratio of the coefficient to the variance. Neither can be estimated separately. To circumvent this problem, the variance is set to 1 and the coefficient estimated within which unit homogeneity is more likely to hold, such as types of democracies (or “democracy with adjectives” (Collier and Levitsky (1997))).
accordingly. Because the scale is irrelevant to the substantive and statistical significance, the assumption is soon forgotten. In other contexts, such as the assumption of homoskedastic errors or the assumption of a symmetric rather than a skewed sigmoid curve in logit and probit estimation, relaxing the assumption may be possible, though it does require information that may not be available unless the data contain a large number of cases.

For case study researchers, adding variables adds combinatorial complexity to the types or configurations of cases that are possible. This does not necessarily or inherently reduce the inferential leverage a case study can bring to bear on the question of whether a particular model explains a particular case. Case study researchers can use methods of typological theorizing to both logically and empirically reduce the theoretical “property space” of all the possible combinations of (categorically measured) variables to the smaller number of cases that are not overdetermined or theoretically impossible and that are thus potentially relevant and interesting to study (George and Bennett, 2005, Elman, 2005). Moreover, the ability to exclude all but one possible explanation of a case on the basis of process tracing evidence depends not on the number of independent variables, but on how alternative hypotheses match up against one another, on whether they predict different processes should be evident in the case, and on whether evidence on the alternative predicted processes is accessible. A study of burden-sharing in the 1991 Persian Gulf War, for example, focused on cases in which some variables pushed toward states making contributions and other variables pushed in the opposite direction. This study concluded that Germany and Japan made substantial economic contributions to the coalition due solely to security dependence upon and political pressure from the United States, even though all the other variables highlighted in alternative explanations (collective action theory, balance of threat theory, and theories of domestic politics) weighed against making a
contribution. This finding substantially raised the scope and importance of alliance dependence in the literature on burden-sharing, whereas the literature up to that time had heavily emphasized collective action dynamics and the temptation to free-ride (Bennett, Lepgold, and Unger, 1994).

The more fundamental point for all researchers is that while theoretical parsimony is desirable, our theories need to be as complex as social reality demands (King, Keohane, and Verba, 1994). As Einstein famously said, “a scientific theory should be as simple as possible, but no simpler.” If a social phenomenon appears to require more explanatory variables than we can wrap our minds around, it is often better to define sub-types of the phenomenon more narrowly, control for some of the variables, and theorize around more narrowly-defined types, rather than omit potentially relevant variables or engage in conceptual stretching. In deciding where to partition a phenomenon into relevant sub-types, empirically observed statistical distributions, logic, and case studies of the ideal type and boundary cases all have roles to play.

**Human Motivation**

Game theory is the best extant example of a symbiotic relationship between theory and methodology: the methodological component (the mathematics of optimization) grows to fit the needs of the theoretical component (prediction of human behavior as comprising rational choices among alternatives), and theory is spurred on by results derived in the process of creating the methodology. For that reason, to say that game theorists make a rationality assumption about human motivation is to miss the point: game theory is the embodiment of that assumption---a methodology derived from first principles based on a largely unified theoretical orientation.

Even within game theory, however, assumptions about human motivations vary to some degree. Theories of bounded rationality attempt to incorporate human beings' imperfections as
utility calculators (Simon, 1957). Evolutionary game theory can even involve entirely myopic actors. Nevertheless, the sources of human motivation rarely stray from the a-rational to the irrational: to our knowledge no game-theoretic models of states that seek to take the most appropriate action rather than the one with the highest utility exist, despite the fact that nothing about the mathematics of optimization inherently limits its applicability to theories of rationality.

Much is made of the difference between game theory and other forms of theorizing on the issue of human motivation. In fact, however, assumptions about rationality vary among formal modelers, and it is also possible to model formally non-rational heuristics and biases. Some construe rationality in the “thin” sense of inferring actors’ preferences from their behavior and using these inferred preferences to predict future behavior based on the assumption that actors will make correct choices based on their preferences, power, and information. This thin notion of preferences can encompass desires for non-material goods such as social status. A challenge to even this thin form of theorizing about preferences and choices is evident in the work of behavioral economists like Amos Tversky and Daniel Kahneman, who explore the ways in which people's cognitive limitations and motivational predispositions can foil their attempts to act in accordance with their preferences and beliefs (Tversky and Kahneman, 1974). There is no inherent reason that many of the heuristics and biases addressed by Kahneman and Tversky cannot be formally modeled, however, though they seldom are. For example, one could formally model prospect theory, which posits that actors will take bigger risks to prevent perceived losses than to achieve perceived gains, by introducing different discount rates for the domains of losses and gains.

Other formal modelers construe rationality in the “thick” sense, in which not just decision processes are assumed to be rational, but preferences and beliefs are posited by theoretical
assumptions (such as maximization or material benefits, power, security, social status, etc.) and are further assumed to be hierarchical, complete, and internally consistent (Elster, 1985). There is substantially more theoretical opposition to such “thick” rationalism and its positing of preferences by assumption. Liberalism and constructivism, for example, focus on the origins of preferences and the ways in which actors view the world and one another; in neither case are the answers to these questions taken to be exclusively, or even primarily, rationalist in the thick sense (Moravcsik, 1997, Wendt 1999).

The most serious divide, however, may have to do with the explicitness, not the content, of the theory's assumptions about human motivation. While game theorists are wont to characterize the motivations of actors and the implications of their interactions in excruciating detail, statistical and case-study analyses typically opt not to do so, preferring to focus more on the variety of factors that could conceivably influence behavior in a given situation rather than on the very specific (and hence more empirically restrictive) logic of the situation itself. As a result, formal theorists often question the utility of empirical studies based on informal or poorly specified microfoundations, while case study and statistical researchers often raise concerns about the overemphasis on theoretical parsimony (and consequent issues such as omitted variable bias and inattention to contest) in the work of formal modelers.

Productive multimethod work, therefore, depends on achieving two goals: striking a reasonable balance between richness and rigor on the issue of human motivation, and achieving some degree of harmony on the nature of that motivation. On the former point, case studies can be fruitfully employed in a heuristic capacity to highlight the most relevant factors weighing in the minds of actors (with the usual methodological caveats on the challenges of inferring the beliefs and preferences of other actors). Increasingly, formal models are used to provide some
guidance for the functional form of a statistical analysis, though the added value of a precise logical specification relating two variables to a third becomes questionable when ten others are thrown in as additive “controls” without any thought as to how they might interact with the primary variables (indeed, “control” is often used as a euphemism for “poorly modeled cause”). Finally, it would behoove formal modelers and statisticians alike to think more about models that permit richer combinations of sets of variables, rather than simply creating models in which one variable maps to one parameter.

Greater harmony on the question of human motivation may at first seem like an untenable goal, but it is worth remembering that the ties between theory and method, while strong in the field of political science as it is currently practiced, may not follow from any inherent theoretical or methodological logic. As we have noted, there is no necessary connection between formal modeling as a method and rational choice assumptions as a theory. One can formally model non-rational decision processes, and both case studies and statistical studies can test rational and non-rational models alike (Morton, 1999).

**Functional Form Assumptions**

The strength of functional form assumptions varies widely across methodological techniques. Case study researchers tend to be less explicit and precise about the functional forms their theories lead them to expect to find. When a researcher is dealing with many observations within a given case---as in content analysis, descriptions of interview results, etc.---the tendency is to “let the data speak” by presenting graphical trends or marginals and discussing the implications for theory. When researchers are explicit about the functional forms of the relationships they expect to find, however, it may be possible to winnow these forms down on
the basis of process-tracing evidence. Even when many alternative functional forms are equally consistent with the outcomes observed in the cases, they may not be equally consistent with the observable processes through which these outcomes arose.

To formal theorists, on the other hand, the functional form of an empirical analysis is critical and is strictly dictated by the deductive theory that makes the test meaningful in the first place. To test another functional form might or might not produce interesting results; it certainly will not, absent the derivation of an alternative theory that justifies the different functional form, produce theoretical knowledge of much interest.

Users of statistics fall neatly between these two relatively defensible stools. Traditionally, functional form assumptions have mostly been based on the most appropriate statistical technique given the nature of the data. If the dependent variable is binary, a logit or probit functional form is chosen; if a count variable, then Poisson; if continuous and unbounded, some flavor of (typically linear) regression. These techniques were, typically, originally designed with statistical parsimony rather than theoretical generality in mind. The implications of that fact are, one, that their mathematical properties are typically well understood, and two, that they may or may not have anything to do with the functional form implied by the theory actually being tested.

Attempts to rectify this disjuncture between assumptions and theory, on the one hand, and data, on the other, have led large-N methodologists in two opposite directions. The first involves tailoring functional-form assumptions more carefully to the implications of theory: Signorino (1999) and Smith (1999) have sought to capture the empirical implications of game-theoretic models, for example, while Braumoeller (2003) has attempted to give statistical meaning to the case-study researcher's argument that multiple causal “paths” can lead to the same outcome. The potential drawback to taking this approach toward functional-form assumptions,
of course, is that the data will provide a poor fit, either in absolute terms or relative to another plausible model. For that reason, the importance of model specification tests like the Vuong (1989) test, the J-test, and the Clarke (2003) test are coming to the fore.

The alternative to deriving functional-form assumptions more directly from theories is to relax them as much as possible. To take a straightforward example: although many users seem unaware of this fact, maximum likelihood techniques make assumptions about the shape of the distribution of the dependent variable that may or may not be justified (probit, for instance, assumes that the error term will conform to a cumulative normal density). If these assumptions are problematic, coefficient estimates are not even asymptotically unbiased, that is, they would not converge to the correct value no matter how many observations are added. One straightforward solution to this problem is to estimate additional \``junk\'' parameters to permit the estimated distribution to conform more closely to the data. This was the justification behind \``scobit,\'' or skewed logit, in which an additional skew parameter is estimated but not interpreted to be particularly relevant to the theory. Another solution is to maximize the quasi-likelihood function rather than the likelihood, and in so doing to do away with assumptions about the shape of the distribution altogether. Finally, one might, using Bayesian techniques, estimate the posterior density of the data directly. Each solution adds flexibility in proportion, inevitably, to its additional cost in terms of number of observations required.

Perhaps the most extreme example of assumption-free modeling is Rubin's idea of causal matching, in which a variable measuring the individual's propensity for having been exposed to the \``treatment\'' is used to create two matched samples of individuals, one of which was \``treated\'' and the other not (Rubin, 1973). Once the samples have been created, the effect of treatment can be measured via a simple comparison of means. The benefits of such an approach
are substantial (especially since the robustness tests that can be applied are remarkably sophisticated). Even this most assumption-free technique must rely on some assumptions to generate its results: the stable unit treatment value assumption (SUTVA), which requires that the outcome for individual i not be altered by the outcome for individual j, is one, as is the assumption that omitted covariates do not interact with treatment values. Moreover, even a simple comparison of means is not completely innocent about functional forms: it assumes, for example, that the treatment does not have a curvilinear impact (low at non-treated and treated values, high in between).

Regardless of the radical disparities between these two schools, they do agree up to a point on the question of functional-form assumptions: both would argue that too many assumptions are made for the sake of mathematical convenience rather than theoretical meaning, and that the inferences derived from such models are not to be trusted. Some, like normality of errors, are typically made for no theoretical reason and are easily violated. The bone of contention between the two groups lies in what to do with theoretically derived functional-form assumptions: one group prefers to model them as carefully as possible, while the other would eliminate them.

**Time**

Like the IR theories that they capture, most formal and statistical models are largely atemporal. That does not mean that the effects that they model are thought to follow instantaneously from the causes, but rather that time is not a particularly important part of the theory. When time does matter, however, it can be incorporated into theoretical and empirical analysis in a variety of ways.
Dynamic formal models and their statistical counterpart, time-series analysis, are designed to model situations in which a cause in one time period produces an effect in another, typically the next. The time period in question can be a day, a month, a quarter, a year, or whatever; time in the usual sense is typically not as important as ensuring that a relationship among variables that plays out over time is modeled appropriately.

At other times, however, time itself is the phenomenon of greatest interest, as in evolutionary game theory or theories that examine the durations of wars or crises using duration or hazard models. In the former case, time is an important independent variable because it permits optimal strategies to be rewarded and suboptimal strategies to be punished, and it permits the reproductive repercussions of those rewards and punishments to unfold. In duration or hazard models, time is an important dependent variable in its own right.

For case study researchers, sequencing and timing are critical sources of inference through the method of process-tracing. Even if the outcomes of different cases are similar, and are equally consistent with competing explanations, these outcomes may have arisen through different sequential processes, and the differences may be relevant to the explanation of the case as well as to its future trajectory. Case study researchers are thus attentive to the possibility of path dependency, and frequently look for indications that self-reinforcing dynamics or self-denying prophecies are at work (Pierson, 2004, Mahoney 2000, Bennett and Elman 2006).

**External Validity**

One substantial but often implicit clash of assumptions should be noted on the subject of external validity, or the extent to which findings can reasonably be said to generalize beyond the cases that have been examined in the course of a study. Reasonable scholars surely differ on this
issue. Still, it seems likely that the distributions of answers given by practitioners of the two empirical methods under examination would overlap little if at all.

Case study researchers are acutely aware of the problem of external validity and generally skeptical of assumptions that non-trivial single-variable causal processes apply in unadulterated forms to large populations; in their world, credit for the ability to generalize beyond the cases studied must be earned. Typically this is done in one of three ways. First, a better understanding of a hypothesized causal process in a particular case, often developed partly inductively through close study of the case, may itself provide clues on the scope conditions of the process, or the types of cases, populations, or contexts to which this process is relevant. In the hypothetical example used above, if we find that an individual voted for a candidate despite differences in party and ideology because the candidate was a blood relative, this provides an explanation that should only generalize to candidates and voters who are blood relatives (or perhaps more diffusely, neighbors, personal friends, and co-workers). Conversely, an improved understanding of a process developed through close study of a case may be very broadly generalizable. Charles Darwin’s study of a small number of species in the Galapagos Islands, for example, helped him develop a theory that by its very nature should apply to all living things. For any explanation derived in part inductively through the study of a case, it is impossible to make an informed assessment of the generalizability or scope conditions of that explanation prior to developing the explanation itself.

A second and more deductive approach is to use extant theories to carefully specify a type of case that represents a particular combination of variables within certain ranges of values, and to then study that type of case with the presumption that any findings will take the form of “contingent generalizations” that will apply mostly or only to cases of this same type. Bennett
and George discuss this approach as the development of “typological theories;” Charles Ragin has talked about similar methods of treating cases as “configurations’ of variables, and David Collier and Steven Levitsky note the development of a literature on “democracy with adjectives” in which each additional adjective denotes a sub-type of the overall concept of democracy (such as a “parliamentary” versus a “presidential” democracy) (George and Bennett 2005, Ragin 1987, Collier and Levitsky 1997). In these approaches, the goal is to develop more internally valid and intensive concepts and hypotheses even if this comes at the expense of being able to apply them only to narrower types of cases.

A third approach to generalizing from cases is to seek to either identify cases that disprove claims of necessity or sufficiency, or to search out instances in which a theory fails to explain a case in which it is “most likely” to apply or succeeds in explaining a case in which it was “least likely” to hold true. If any such cases exist with respect to the phenomenon under study, even a single case can provide presumptive evidence that a claim of necessity or sufficiency is unfounded, or that a theory that fails in a most likely case should have narrower scope conditions and one that succeeds in a least likely case probably deserves wider scope conditions. An example of the latter is the above-noted research in which alliance dependence was the only explanation to correctly predict the behavior of Germany and Japan in the 1991 Gulf War, which suggests that the alliance dependence variable might be a powerful one in any cases in which it is present.9 In all three of the approaches to generalization from case studies, however, any findings on scope conditions are suggestive rather than definitive, as they could be

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9 Strictly speaking, the 1991 Gulf War was not a completely least likely case for the alliance dependence hypothesis. Even though all other hypotheses pushed against the actual outcomes in Germany and Japan, these countries were highly dependent on the U.S. for their security. Had Germany and Japan been only modestly dependent on the United States, and had it still succeeded in explaining their behavior despite the effects of all the other variables of interest, we could infer that the alliance dependence variable was even stronger and more generalizable.
undermined by measurement error or specification error in the cases studied. This is why studies of additional cases, whether through case study or statistical analysis, are valuable means of verifying the scope conditions of a theory that emerge from the analysis of case studies. Researchers utilizing statistical methods, on the other hand, tend to view their set of cases as a sample representative of a larger universe of cases.10 For that reason, the presumption of external validity is typically conditional on the inclusion of the proper controls. To take an example, Reiter and Stam's findings on democratic efficacy in warfighting are based mostly on the behavior of a very small number of democracies (Reiter and Stam, 2002). Being conscientious researchers, the authors included every control variable that they could think of that would make these countries unique or unrepresentative of democracies in general. Having done so, they were comfortable making generalizations about the larger population of democracies as a whole.

Researchers working in the case study tradition might question the assumption of unit homogeneity in this research and object that Reiter and Stam's conclusions do not extend much beyond these few democracies, regardless of which controls have been entered.11 Whether one believes the authors or their qualitative critics depends in large part on how far one is willing to extend the benefit of the doubt on external validity, and why. One means for judging such debates is to apply the case study standard noted above and ask whether there is some theoretical reason to believe that Reiter and Stam’s cases were “least likely” cases, or tough test cases, or most likely/easy test cases for their theory. Another approach would be to address the implicit unit-homogeneity argument head on: for the critique to hold, the relationship between democracy

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10 Very rarely, though, is this universe of cases explicitly defined, and as a result the relationships are treated as though they were in fact universal. Not many researchers would actually argue that their
and victory would have to be different in the countries that dominate the Reiter-Stam study from that in other countries. The authors, to their credit, attempt to correct for this possibility by including control variables that might confound the relationship between democracy and war. Similarly, it is incumbent upon critics not just to argue that other democracies are different but to specify how they differ, so that the importance of those differences can be investigated. A third technique, which can be quite convincing, involves estimating the model on one subset of cases and then assessing its external validity by testing its predictions on another, different subset. Braumoeller (2008:86-7) provides an example of this approach. Finally, in any case in which unit homogeneity is at issue, straightforward equality-of-coefficient tests can typically help determine the validity of the assertion that the relevant coefficients differ across subsets.

**Comparative Advantages and Limitations of Combinations of Methods**

The analysis thus far indicates that the differing working assumptions of statistical, formal, and case study methods can often be reconciled. This motivates and enables multimethod work. We now turn to the advantages and uses of different combinations of methods. In this task, we build upon the typologies of theory-building studies developed by Arendt Lijphart and Harry Eckstein (Lijphart 1971, Eckstein 1975). Although their typologies and terms are not exactly the same, Lijphart and Eckstein identify four theory-building goals or types of study that are useful for present purposes: plausibility probes, disciplined-configurative studies, heuristic or theory-generating studies, and theory-testing studies (this sets aside purely descriptive or ideographic studies, as statistical and case studies can be descriptive but neither would be used with a formal model for descriptive purposes only). Plausibility probes are
“quick and dirty” studies to determine whether more laborious or expensive studies are justified, and to make changes to the research design before investing greater effort. Disciplined-configurative studies use a theory to explain a case or a statistical correlation whose variables or outcomes are already well known but whose result may have heretofore remained puzzling. Heuristic studies use deductive and/or inductive logic to identify new or left-out variables or interactions. Theory-testing studies test whether theories apply to or explain particular cases or populations of cases (strictly speaking, they test whether theories fail to apply, or at least fail worse than the available alternative theories).

When methods are used in combination, sequencing matters. It matters, in other words, whether in a formal-statistical study the formal model is derived first and then tested against statistical data, or statistical data were first analyzed and then used to help build a formal model. These sequences are different for two reasons. First, they fit different theory-building goals. The formal-statistical sequence fits most closely the purpose of theory testing, while the statistical-formal sequence fits the heuristic purpose of building new models. Case studies can precede statistical analysis to develop theories, variables, and measurements to be tested statistically, or statistical analysis might be performed first and then used to help identify which cases are likely to be most fruitful for either testing whether hypothesized mechanisms operated as theorized in individual cases or exploring whether important variables may have been omitted from the statistical analysis. Second, the sequence determines which evidence has “use novelty” for the resulting model, or which evidence can be used to test the model without risk of confirmation bias. An independently-derived formal model can be tested against statistical data, but a model derived from statistical data cannot be tested independently against that same data (although a model derived from one subset of the data could reasonably be tested on a different

11 In fact, they did so, at a presentation at Harvard's Olin Institute.
subset; this intuition informs the use of neural networks (Beck, King, and Zeng, 2000, 2004, De Marchi, Gelpi, and Grinaviski 2004).

At the same time, to say that sequencing matters is not to discourage iteration between methods when they are used in combination—in fact, the right kind of iteration is to be encouraged as it is one of the main potential contributions of multimethod work, and such work can serve more than one theory-building purpose in a single study (such as developing and then testing a theory or using a theory to explain an important case and then testing it more generally). A formal model might be deductively derived, then tested against statistical data, which might lead to modifications to the model, which in turn can lead to tests against different or newly-coded statistical data. Such iterations provide legitimate, progressive theorizing rather than illegitimate “curve fitting” as long as researchers are careful to establish the use-novelty and/or “background theory novelty” of their empirical tests at every step of the iteration (background theory novelty refers to the absence of a plausible alternative explanation). Still, not all kinds of iteration are possible for all kinds of theory-building goals. We can do statistical tests of formal models, for example, but we cannot do formal model “tests” of statistical findings. Our inability to come up with a formal model of statistical findings might not undermine our confidence from correlational data that something causal is going on, even if we don’t understand what exactly the something is or even which variables are dependent and which independent. For example, the dearth (until recently) of formal models on the democratic peace phenomenon did not undermine the widespread (but not unanimous) belief that democracies rarely if ever go to war with other democracies.

Building on the Lijphart-Eckstein typology, and keeping the issue of sequencing in mind, Table 1 summarizes the comparative advantages and uses of different combinations of methods:
Table 1
Comparative Advantages of Combinations of Methods

| Theory-Building Goal | Formal-Statistical | Type of Study | Case Study-Statistical |
|----------------------|--------------------|---------------|------------------------|
| Plausibility Probe   | A) See if re-doing models and/or data sets is worth the effort | B) Identify changes to model or choose among models before further testing | C) Identify changes to the model or which cases to study next |
| Disciplined Configurative | D) Use correlations to guide construction of a model, or model to explain a correlational finding | E) Use a model to understand a case or a case to modify a model | F) Use statistical findings to structure study of a case, or a case to identify variables for statistical models |
| Heuristic Theory-Generating | G) Use a formal model to identify new or counter-intuitive variables for statistical study | H) Use deduction (model) or induction (case) to identify new variables to use in the complementary method | I) Use statistical findings to identify outliers, then study these cases to find new or left-out variables |
| Theory Testing       | J) Statistical tests of formal models | K) Case study/process tracing tests of formal models | L) Cases test if putative processes behind correlations took place, statistical Study tests if case findings Apply to a population |

Before examining each combination of methods and research goals and examples thereof, a few general observations on multimethod work in each of the theory-building purposes in Table 1 are in order. First, we suspect that most plausibility probes are implicit rather than formally
structured, and don’t make it into print. This is probably most true of combinations of methods that use statistical tests, as it is quite easy to run different model specifications on existing data sets and quite laborious to devise and carry out original data codings. Although this can lead to a kind of selection bias where only the “successful” regressions make it into print, there is nothing inherently wrong with iteration among methods or between data and methods if the researcher applies relevant standards of use novelty and background theory novelty at each step. It is highly desirable, however, for researchers to be more explicit in their published research about what steps they took at this “soaking and poking” stage. The one kind of plausibility probe that does tend to appear in print is formal-case work, several examples of which are noted below, in which cases either motivate or illustrate a formal model.

Second, disciplined-configurative goals are fairly common in multimethod work. Often, these are mixed in with theory-testing goals, though it is important to keep these different theory-building purposes conceptually distinct and operationally distinct in one’s research. Explaining a particular case or statistical distribution usually requires a somewhat different approach than testing a theory. The two goals are closely related—as Eckstein (1975) notes, a disciplined-configurative study constitutes a kind of theory test when a theory should fit a case but does not—and can be pursued in the same study, but researchers need to self-consciously fulfill the requirements of both purposes.

Third, the goal of using combinations of methods to generate new theories appears to be relatively neglected, even though it is potentially one of the most important contributions of combining methods. Many of the examples discussed below did not give much attention to

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12 Of course, if the researcher were not applying relevant standards at each step, and they tried out 20 different model specifications and applied a 5% significance level, we would expect that even if none of the models were truly causal on average at least one would achieve the 5% level of significance. Kennedy (2000) offers sage advice here, in the form of ten econometric commandments that help avoid a tautological dialogue between theory and evidence, including: thou shalt know the context; though shalt beware the costs of data mining (by, for example, not worshipping the R-squared coefficient and the 5% significance level); thou shalt engage in sensitivity tests.
“outlier” cases in statistical distributions, or when they did, with some exceptions that noted below, the discussion was rather cursory or ad hoc. This constituted a missed opportunity for many studies -- researchers often invested enormous effort in constructing formal models and databases and could have reaped important theory-building rewards through just a little more effort in focusing on the outliers.

Fourth, theory-testing combinations of methods seem to be the most common, and of these, the formal-statistical and statistical-case study combinations, consistent with the survey data noted above, were the most common in my (not necessarily representative) sample. There appear to be missed opportunities here to use case studies to test formal models, rather than just combining the two methods for the disciplined-configurative purposes of illustrating models or explaining cases. Moreover, among the many examples of theory-testing by combining formal or case study methods with statistics, there was relatively little attention to testing alternative explanations or alternative formulations of statistical or formal models (or at least not much of this process made it into print).

The sections below discuss in more detail the research design issues involved in each combination of methods for each theory-building purpose.

**Multimethod Works Selected for Analysis**

Even for those expert in these methods, it is difficult to evaluate how well an author has done in implementing a study unless the evaluator has intimate knowledge of the databases the author is using (or modifying) and the particular research puzzle they are addressing. We therefore focus on research examples from our own field of international relations. From a survey of multimethod work, we selected for further analysis 11 multimethod articles in international relations from the journals *International Organization, International Studies Quarterly, The Journal of Conflict Resolution,* and *World Politics*. This sample is of course not necessarily representative of multimethod work generally, but it does include four examples of formal-
statistical work, five of formal-case study work, and two of statistical-case study work. We supplemented this sample with analysis of eight multimethod books on international relations, two of which used all three methods and six of which used a combination of statistical and case study methods, and one book on comparative politics which combines formal models and case studies (the books and articles analyzed are listed in a separate bibliography below). The overall sample thus includes more than a half-dozen examples of each binary combination of methods, and while this sample is not necessarily representative of multimethod work in international relations or in political science more generally, we argue that it does illustrate the methodological strengths and limitations we would expect to see in view of earlier studies of the comparative advantages of individual methodological approaches (Collier, 1991, Goertz and Mahoney, 2006, Morton, 1999).

**Formal-Statistical Work**

There were no clear examples of formal-statistical plausibility probes among the works selected for analysis.\(^{13}\) In a sense this is surprising, as formal modeling and statistical analysis (especially if it involves coding data) can be quite laborious and thus are exactly the kind of venue in which plausibility probes would be expected in order to save time, effort, and expenses. In another sense this is not surprising, as this kind of research is less likely to get published than the full-scale research to which it leads. Some have urged journal editors to publish more “negative results,” but with the important exception of negative results on widely accepted theories, this rarely happens and is unlikely to change. Perhaps a more realistic solution, as King, Keohane and Verba (1994) suggest, is for researchers to “leave up the scaffolding” and provide more information, at least in footnotes, on the alternative models and measures that they considered, and why they rejected them.

\(^{13}\) For an analysis of formal-statistical work, see Cameron and Morton, 2002.
An excellent example of a disciplined-configurative study in this combination is the work by Kenneth Schultz on the democratic peace (Schultz, 2001). Numerous statistical studies have established the finding (though still contested) that democracies rarely if ever fight other democracies even though they have often fought against non-democracies, and many case studies have been done on the close calls of near-democracies that went to war or near wars between democracies. Schultz’s work began filling an important gap in this literature by bringing formal models to bear on the question and testing them through statistical means. This work is disciplined-configurative in that Schultz is attempting to explain a surprising statistical distribution (the relative absence of inter-democratic wars) that was well-known but that lacked a consensus explanation among candidate theories focusing on democratic norms, democratic institutions, and combinations thereof. Schultz focuses on problems of credible commitment and finds that they are an important part of explaining the democratic peace.14

This is an important contribution to the literature, and it also illustrates the kind of iteration that is necessary to keep advancing research programs through formal-statistical work. If the democratic peace literature is to be truly re-cast as a “credible commitment” literature, we will need databases that measure the ability to make credible commitments rather than democracy per se.15 Presumably, democracies vary in their ability to make credible commitments, depending on state strength on foreign policy issues, parliamentary versus presidential system, proximity and frequency of elections, and so on. Non-democracies should also be expected to vary in their ability to make such commitments. If credible commitment arguments fare well when tested against dedicated databases, and if their dynamics are evident in individual case studies, they will deserve strong support.

Formal-statistical work often serves a heuristic purpose in which a formal model is used to derive deductively a new variable or specification, which is then tested statistically.

14 Check and explain Schultz’s research in more detail.
15 Similarly, Morgan and Campbell (1991) assess whether domestic decisional constraints rather than democracy are the root cause of the democratic peace phenomenon.
Two examples, both of which use all three methods, illustrate this process. Lisa Martin, in *Coercive Cooperation*, uses game theory to derive hypotheses about cooperation in imposing economic sanctions. The most counterintuitive of these hypotheses are that cooperation will increase when the major sanctioner bears high costs, since the incentives and ability to coerce others to join in imposing sanctions increase, and that states will bandwagon in imposing sanctions (that is, as the larger the sanctioning group becomes, the more states will want to join this; this is counter-intuitive as the rewards for bypassing sanctions will increase as the target country becomes more desperate to acquire the embargoed goods) (Martin, 1992:44-5). Martin then tests these hypotheses through statistical means as well as case studies. In a second example, Hein Goemans, in *War and Punishment*, develops a formal model of war termination that includes differences in regime type, and he hypothesizes that semi-repressive regimes will refuse to lower their war aims (and may even increase them) when they learn from the battlefield that they will probably lose, whereas democratic and fully repressive regimes are more likely to lower their aims and seek to settle a war when they learn they are likely to lose. This is because leaders of semi-repressive regimes are likely to lose power and possibly their lives whether they lose badly or lose moderately in a war, whereas leaders of democratic and fully repressive states are at great personal risk only if they lose badly in a war.16

These are novel, counterintuitive claims that illustrate very well the heuristic potential of formal models that are subsequently tested through statistical means. At the same time, these studies, while generally exemplary in their methodologies, miss opportunities for further developing their theories with little additional effort by paying attention to the outliers in their statistical tests. Goemans’s chapter presenting his statistical results, while clear in its codings and conscientious in its specification checks (and a model to be emulated in “leaving the scaffolding up”) does not discuss or even identify the cases that were outliers in the statistical tests (Goemans, 200: 53-71). These cases could prove rich sources for further development and

16 Hein E.Goemans, War and Punishment: The Causes of War Termination and the First World War (Princeton University Press, 2000), pp. 34-52.
refinement of Goemans’s theory (or of alternative explanations). Similarly, while Martin notes that she excludes the cases of sanctions against South Africa and Rhodesia from her sample because of the unique degree of consensus about the immorality of apartheid and minority rule skews the statistical results, she does not introduce a control variable for “degree of consensus on morality” (admittedly difficult to code), but more importantly she does not identify or discuss which other cases might still be relative outliers in her statistical work (Martin, 1992:51).

More generally, researchers using statistical methods and formal models typically have very different views of “outliers,” and both groups could benefit from considering the views of case study researchers on this subject. Statistical researchers are prone to looking at outlying cases as the result of either random errors, measurement error, or unique and identifiable causes, so that these cases can be safely ignored or should even be excluded from statistical analysis. Formal modelers, unless their models include a stochastic element, view outliers as impossible cases that, if they in fact occur, indicate a flaw in the formal model. Case study researchers would place more emphasis on the fact that formal and statistical models are always incomplete, so that outlier cases are potentially very fruitful grounds for identifying left-out variables and specification errors (or alternatively confirming that measurement error is indeed the source of the outlying observations, or finding that no suitable explanation exists, which may indicate either randomness or the limits of current knowledge). As noted below, researchers who have combined statistical methods and case studies have sometimes been very conscientious in identifying and discussing outlier cases; at a minimum, it should be standard practice to identify such cases.

Theory testing is perhaps the most common goal of combining formal and statistical methods. Good examples here, in addition to Goemans and Martin, include Downs, Rocke, and Barsoom (1998), Iverson (1998), Epstein and O’Halloran (1996), and Lohmann and O’Halloran (1994). These examples underscore that a general challenge in statistical tests of formal models is that standard statistical models, and even more importantly standard statistical databases, do not typically allow seamless statistical testing of exactly the same model as
specified in the formal work. The examples cited here generally devote great attention to this problem and go to considerable efforts to adapt standard databases to the variables defined in the formal model, often carrying out new codings of key variables or modifying existing measures. Without detailed knowledge of the databases in question it is impossible to judge how successful the authors have been or how they might have done better in testing the actual specifications of the formal model -- our purpose here is merely to draw attention to this as the central challenge in this combination of methods.

Formal-statistical theory testing also appears to give only infrequent attention to fully-specified alternative explanations.17 To some degree, this is understandable in view of the difficulty of specifying even a single formal model and testing it statistically, and in view of space constraints in journals. Also, at present most formal models have been developed in “high density” research programs, or programs that have attracted the attention of many researchers.18 As formal models become more common in a wider range of domains, however, more attention needs to be given to testing alternative explanations, whether formal or otherwise.

**Formal-Case Study Work**

Much of the work that combines formal models and case study analysis includes elements of plausibility probes and disciplined-configurative studies. The case studies are often presented here as illustrations or demonstrations of the formal model, without necessarily being full tests. In other words, the cases are often (but not always) chosen as motivating examples or most-likely cases, rather than being least-likely cases or tough tests of the formal model. One example here is Peter Feaver and Emerson Niou’s (1996) study of interactions between powerful states that have nuclear weapons and small states that may choose to try to acquire nuclear weapons. Feaver and Niou focus their analysis on the United States as the prototypical powerful state and North Korea as the potential proliferator, and they explore the preference orderings

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17 Cameron and Morton?
18 Cameron and Morton?
under which pre-emption, tolerance, or even limited assistance from the United States would be likely. The authors do not conduct any empirical research on the actual preference orderings of U.S. or North Korean leaders, however, and indeed such research would be all but impossible in view of the challenges of getting U.S. leaders to reveal their preferences on such a critical issue and getting North Korean leaders to reveal anything at all.

Similarly, Richard Rosecrance and Chih-Cheng Lo use a nested game to help understand why European states failed to balance against Napoleon until the threat he presented began to decline, and why Western balancing against the Soviet Union was more uniform in the early Cold War years (Rosecrance and Lo, 1996). Using both the model and the historical examples, these authors argue that the relationship between the balance of threat and actual balancing behavior is curvilinear rather than linear. These authors give ample attention to alternative explanations (traditional balance of power, balance of threat, and offense-defense balance theories) and conclude that balancing is unlikely when a threatening state is too powerful, and might be mitigated when that state offers side payments or when geographic impediments make it difficult for potential balancers to move forces to the front lines and such forces are not already in place.

This study has elements of theory testing, as well, as the authors argue that Napoleonic France should be a most-likely case for traditional theories so that these theories’ failure to explain this case is particularly telling.

One other clear example of a disciplined-configurative study using these methods is Paul Papayoanou’s (1997) article on intra-alliance bargaining in U.S. policy toward Bosnia from 1991 to 1995. Papayoanu uses a game theory model to “interpret” U.S. behavior and to explain why the U.S. did not bargain more toughly with its NATO allies. The author considers, in general terms, the neorealist and neoliberal alternative explanations to his model, which combines domestic and international pressures, but while he argues U.S. policy on Bosnia presents a puzzle, he does not argue that it is a tough test case for his explanation or an easy case for the alternatives.
Formal models can be used heuristically to derive deductively new models, two examples of which are the works by Goemans and Martin discussed above, but this does not depend on combining these methods with case studies. More interesting for present purposes is that case studies can be used inductively to develop new theories or variables that can be used to build formal models. This process has been relatively neglected, however, at least in the works surveyed herein. This is related, as noted above, to the fact that work using formal models frequently does not focus on outlier cases or even consider fully alternative explanations. The multi-authored book *Analytic Narratives*, for example, while exemplary in many respects and laudable in its efforts to combine formal models and case studies, starts out with rational choice formal models and gives attention to alternative explanations only to explain the considerable variance that remains in each of the empirical chapters after rational choice models have been given their due (Bates, Grief, Levi, Rosenthal, and Weingast, 1998). In several chapters, for example Margaret Levi’s chapter on actors’ choices among alternative forms of conscription for military service or Robert Bates’s chapter on the international coffee market, important preferences beyond the original formal model emerge only toward the end of the study, rather than being treated from the outset as complementary or alternative explanations deserving of equal consideration. In Levi’s case, this includes considerations of “fairness” among alternative conscription methods, and in Bates’s chapter, it involves U.S. fears over the susceptibility to communist revolutions of coffee-producing states in the developing world. Such variables might be included in the attendant formal models (admittedly a challenging task), rather than relegated to explaining the left-over variation.

Case studies can also be used to test whether the causal mechanisms developed in formal models actually applied in historical cases. This potential is often under-achieved, however, due to problems of case selection. Here again, the *Analytic Narratives* volume illustrates this problem, as it states that “our cases selected us, rather than the other way around.” (Bates et al, 1998:13) It is important to note that these authors’ goal was primarily disciplined-configurative studies, rather than full theory testing, and their case selection was more appropriate for their
own research purposes, but it is important that formal-case study works that aspire to theory testing give sufficient attention to alternative explanations and choose cases that are not subject to selection bias or that pose tough tests for the theory of interest. One useful example here is Kenneth Schultz’s and Barry Weingast’s study of their model suggesting that democratic states should enjoy an advantage in enduring rivalries because of their ability to borrow money more readily and at lower interest rates than states lacking mechanisms for sanctioning leaders who default on state debts. These authors faced a potential selection bias if they randomly chose which cases to study because earlier research indicated that democracies tend to choose to fight wars in which they have a strong probability of victory. This selection effect, rather than any financial advantage, might have accounted for a democracy’s success in any particular war or rivalry. Schultz and Weingast thus chose to study long-term rivalries between relatively evenly-matched states, including the Anglo-French rivalry from 1689-1815 and the United States-Soviet Union rivalry during the Cold War, thereby bypassing the possibility that the democracies in these rivalries had simply chosen weaker foes over whom they were likely to prevail.

Oona Hathaway’s work on trade liberalization and demands for protection illustrates the value of studying tough test cases to assess a formal model. Hathaway developed a model suggesting that industries susceptible to pressure from foreign competition will become less rather than more protectionist as the result of trade liberalization. She then tested this model against three cases—the footwear, textiles, and apparel industries—which she justifiably argues are tough test cases because they are ordinarily considered to be highly protectionist due to the threats they face from low-wage foreign competition. Hathaway then established convincing process-tracing evidence that these industries in fact became less protectionist through the dynamics predicted by her model (Hathaway, 1998).

One other issue that does not consistently receive sufficient attention in formal-case study work is the problem of generalizing from case studies. There might be a temptation to assume that the deductive nature of formal models automatically assures them some generality, but of course this is not an unproblematic assumption. Generalization also requires carefully
specifying the population to which the model is to be applied, or put in other terms, the contextual conditions in which it is relevant. Taking on tough test cases, as Hathaway does, provides a presumption that a model will apply in easier test cases, but this presumption cannot be accepted on faith. The generalizability of a model is even less clear when the model is applied to most-likely cases or when the author is not clear in arguing how likely a case is for the model or how tough a test it provides. For example, Suzanne Lohmann has studied in remarkable detail, almost to the hour and to the city block, the uprisings in East Germany in 1989 as an example of an “information cascade.” Lohmann gives a strong account of her own formal model’s explanation of this event relative to those of other models or theories, and is generally conscientious in many aspects of theory testing, but she does not give any attention in her 59-page article to the question of how tough a test the German case is for her model or of how her model might apply to potential uprisings against other unpopular authoritarian regimes, such as those in Cuba, Iraq, or North Korea (Lohmann, 1994).

Lohmann gives much closer attention to the issue of generalizability in her 1998 study of the German central bank’s independence and its ability to make credible commitments. This study actually combines statistical and case study methods, but since the statistical work is on Germany alone, the issue of generalizability is similar to that for studies combining formal models and case studies. Here, Lohmann first notes that the German central bank is a tough test case for her theory of informal party influence over formally independent institutions, as this bank is considered one of the most independent in the world. Then, after presenting both case study and statistical evidence in support of her argument that Germany’s political parties did in fact influence the bank’s behavior, Lohmann further bolsters the case for her theory’s generalizability by citing several cross-national studies of central banks that are consistent with her argument. She is nonetheless careful not to make excessively broad claims for her theory, noting that “any sweeping conclusions about the link between federalism and central bank independence are suspect. Part of my message is that the specifics of how a central bank is embedded in a larger political system matter a great deal, and for this reason my analysis is
unlikely to generalize to other countries in a straightforward way” (Lohmann, 1998:445). Lohman goes on to suggest specific reasons why her theory is more applicable to the developed countries than to the developing countries, and calls for further research on this subject. She arrives, in short, at a well-defined contingent generalization that identifies the scope conditions of her theory and the population to which it best applies.

Case Study-Statistical Work

Analyses of combining statistical and case study methods often assume that researchers typically begin with a statistical analysis and use this to inform their subsequent case study research, particularly on the issue of selecting which cases to study in detail (Lieberman 2005, Gerring and Seawright 2007). This is a useful and common sequence, and we focus upon it because it addresses some of the most important, recent, and controversial elements of this combination of methods, but it is important to note that case studies can also usefully precede statistical analysis. Case studies can constitute what Eckstein termed a “plausibility probe,” developing the theories to be tested, identifying the variables to be included, devising ways of measuring these variables, and clarifying the attributes that constitute the relevant population before investing the time and resources necessary for coding numerous cases for statistical study (Eckstein, 1975, Rohlfing 2009:1497). Such plausibility probing is likely to be more common than it might seem, as much of it is unlikely to get reported in the final results of published research.

We also focus on the use of statistical analysis to help select case studies, however, as one of the most common criticisms of case study-statistical works is that they are not sufficiently rigorous in their selection of cases. James Fearon and David Laitin, for example, warn against “cherry-picking” or selection bias if researchers choose to study cases that had low
error terms in a statistical model, and they are skeptical of many authors’ claims that they chose cases that posed tough tests for their theories (Fearon and Laitin, 2008). Fearon and Laitin are more supportive of studying “off the line” or deviant cases, but on the whole they urge random selection of cases from stratified samples, rather than intentional selection of cases based on information about how these cases compare to the relevant population or fit competing theoretical explanations. These authors even suggest that “missing variables are more likely to be found in cases forced upon the investigator than in cases she or he chose” (Fearon and Laitin, 2008: 765), although they acknowledge that random selection has important disadvantages, especially when the quality and number of case studies that can be done are limited (in their own example, these authors chose 25 cases at random for study, a higher number of cases than in the other examples of case study-statistical research that we analyzed).

Fearon and Laitin’s concerns regarding the potential misuse of selecting cases on the basis of prior statistical information are well worth noting. Selection bias, confirmation bias, over-generalization, and exaggerated claims of how toughly a case tests a theory are all dangers to be wary of and to avoid. Ingo Rohlfing identifies a related problem that can afflict combinations of statistical and case study methods: the errors that arise from either method, particularly errors in specifying the causal model, can “travel” from one method to another and become compounded and generalized (Rohlfing, 2009:1493). Yet the proper response, as Rohlfing notes, is not to abandon multimethod work but to combine methods in ways that minimize the risks of inferential errors. Fearon and Laitin embrace this goal as well, but we concur with other methodologists who warn that although their preferred solution of randomized

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19 It is important to note that even cases picked on the basis of knowledge of the values of their independent and dependent variables and their error terms relative to a particular statistical model still have some independent power to support or disconfirm the model because there may be considerable evidence on the processes through which the outcome arose that was not initially known to the researcher or used as a basis for case selection.
selection is useful and even necessary in many kinds of studies, it can be inefficient at best and “wildly unrepresentative” at worst when used to select small numbers of cases for study (Gerring and Seawright, 2007: 87; see also King, Keohane, and Verba, 1994: 125-128). In particular, it is hard to see how random selection of cases would be more likely to uncover omitted variables than the intentional selection of cases with large error terms, although we note below that the latter approach has potential pitfalls as well.

Selecting cases for close study on the basis of information from a statistical analysis is not a one-size-fits-all affair. Different case selection criteria are necessary for different theory-building purposes. As John Gerring and Jason Seawright argue, depending on one’s theory-testing and theory-building purposes, one might choose from a statistical analysis cases for detailed study that are typical, deviant, influential, extreme, diverse, most likely or least likely, most-similar or most-different, or representative of a particular pathway to the outcome of interest (Gerring and Seawright, 2007). We discuss each of these in turn.

Gerring and Seawright define a “typical” case primarily as a case with a low error term relative to a statistical model, but it is important to also take into consideration whether the values of a case’s independent and dependent variables are close to and potentially representative of a cluster of other cases. This is not the same as being a case with average values on its variables, as Gerring and Seawright discuss, as it depends on the distribution of the population—in a population with many wealthy people, many poor people, and few people in-between, a person of average wealth is not representative. Also, as Gerring and Seawright note, one cannot assume that a case has a low error term because it fits the model. The size of the error term can be misleading if the statistical model is misspecified (Gerring and Seawright, 2007:96-97, Rohlfing 1496). Measurement error, one omitted variable with large effects, or many omitted
variables with small effects, can make a case appear to be typical of the processes underlying a population when it in fact reflects different causal processes.

Although claims and measures of typicality are not foolproof, studying a case with a low error term and high similarity to many other cases in a population can help assess potential spuriousness by testing whether the causal mechanisms hypothesized as possible explanations of a statistical distribution are in fact evident in a case that is by all known information the most representative of the population. In addition, the study of such cases, and the sequences through which their outcomes arose, can help address the potential for endogeneity by establishing whether the independent variables of interest appeared to precede and cause the dependent variable, or the reverse. Moreover, although it is not possible to eliminate the danger of model misspecification, case studies that proceed inductively as well as deductively, and those that attempt to trace backward from observed effects to possible causes as well as forwards from hypothesized causes to observed effects, provide the potential for uncovering any omitted or superfluous variables that might make a deviant case wrongly appear to be typical (Rohlfing, 2009).

There is a clear and strong consensus among the researchers who have combined statistical and case study methods regarding the value of this kind of analysis. (Goemans 2000:15, Simmons 1994:14, Martin 1992: 4, 8, 10; Shambaugh 1999:54, Reiter 1996:55) Hen Goemans, for example, argues that case studies can more readily deal with problems of strategic interaction, potential endogeneity, and some kinds of selection effects, as they can examine through process tracing whether actors considered actions or outcomes that were “off the equilibrium path.” He also notes that case studies offer advantages when it is difficult to develop and measure complex variables over a large number of cases (including, in his research, new
information from the battlefield on who is likely to prevail in a war and changing terms of proposed war settlements) (Goemans, 200:17,71).

In contrast to a typical case, a deviant case is one with a high error term relative to a statistical model; whether the processes behind such a case are representative of those in the rest of the population is difficult to assess a priori, but this can be assessed by subsequent statistical analysis. Gerring and Seawright, among others, argue that the study of deviant cases provides an opportunity to look for explanations or variables left out of a statistical model (Gerring and Seawright, 2007: 106-107). Paul Huth, for example, notes the importance of analyzing the outlier cases in his book on extended deterrence, and he selects several deviant cases for detailed study (Huth, 1988:85). Dan Reiter takes this approach one step further in his study of how leaders draw lessons from their historical experiences with alliances that affect their subsequent alliance strategies (Reiter, 1996). After performing statistical analysis of 127 cases, Reiter not only studied cases that fit his theory to assess whether the hypothesized causal mechanisms were in fact in evidence, but he studied all 16 of the 127 cases that had high error terms in his regression analysis. From these deviant cases, Reiter identified the presence or absence of democracy as an important variable that conditions whether and how lessons from formative experiences influences alliance strategy choices. Reiter then added this variable to his statistical model and re-analyzed the results in his original database and in two additional statistical studies. The results strengthen the conclusion that democracies make alliance choices in line with lessons from their formative experiences, though in other respects the findings are mixed. It is important to note, however, that the processes that produce a large error term, whether involving one strong variables or many weak ones, may not be relevant to other cases in a population. If
so, subsequent statistical analysis like that which Reiter carried out is likely to reveal this to be the case.

Influential cases are a third kind of case that can be usefully selected from a statistical distribution for closer study. In contrast to analysis of a deviant case, the study of an influential case primarily seeks to affirm or disconfirm the validity of a statistical model, rather than to uncover omitted variables, although it can have the latter result as well (Gerring and Seawright, 109). An influential case is one whose removal from a statistical sample would have a large effect on the regression estimates. For this reason, it is often important to study influential cases to assess whether their outcomes arose through the processes theorized as possible explanations of the correlations observed in a population. If closer study reveals that an influential case does not exhibit these processes, then the results of the statistical analysis are suspect and an argument can be made for removing the influential case from the statistical data set (presuming that there are strong theoretical and methodological reasons to believe that the hypothesized processes, and not those actually observed in the influential case, might explain the population).

Important controversies can turn on the analysis of influential cases. Vipin Narang and Rebecca Nelson, for example, use their interpretation of influential cases to critique Jack Snyder and Edward Mansfield’s multimethod finding that states in transition to democracy are prone to war due to the temptations for their insecure leaders to appeal to ethnic or nationalist themes. In Narang and Nelson’s view, Snyder and Mansfield’s statistical analysis “rests entirely on a cluster of unrepresentative observations involving the dismemberment of the Ottoman Empire prior to World War I” (Narang and Nelson, 2009:358). Snyder and Mansfield reply that “the Ottoman cases conform extremely well to the causal pathways to war that we expect for incomplete democratizers with weak institutions.” Snyder and Mansfield bolster this conclusion with case
study evidence and conclude that the Ottoman cases should rightly remain in their statistical analysis, and they further strengthen their argument with brief analyses of more recent cases not included in their original statistical analysis (Snyder and Mansfield, 2009: 385-387). Whether one finds the critique or response more convincing (we are inclined toward the latter), the important point here is that both sets of authors agree that the analysis of the statistical results rests largely on the interpretation of these influential cases.

The selection of diverse cases from a statistical distribution is designed to maximize variance on the variables of interest (Gerring and Seawright, 2007, Lieberman, 2005). A related criterion for case selection is to select cases that have extreme values on the independent or dependent variables. An extreme value case may also have either a high or a low error term; the former constitutes a deviant as well as extreme case, and the latter may be an opportunity to observe in sharp relief causal mechanisms that may be relevant to the rest of the population (with the caveat noted above that a case with a small error is not necessarily typical or representative). The study of diverse cases is fairly common in multimethod works (see, for example, Simmons, 1994: 16-17, Martin, 1992: 99). Choosing diverse cases can raise the likelihood that the results of the case study analysis will represent the population, or its stratified subsets, though there is a danger that the results will not be representative. For example, if there are many “high” cases and few “low” cases and the researcher chooses one of each, generalizing between the two can be misleading (Gerring and Seawright, 2007: 100). There is a greater danger of inferential error in just choosing one extreme case and generalizing the results to a population (Collier and Mahoney, 1996). Here, the argument for generalization can be strengthened by demonstrating that the processes behind the outcome of an extreme case were also evident in a typical case.
Most likely or least likely: FINISH, USING FAZAL EXAMPLE

Tanisha Fazal, “State Death in the International System,” *International Organization* vol. 58, no. 2 (Spring, 2004), pp. 311-344. Fazal argues that buffer states, or states located between two powerful rivals, are most likely to suffer occupation or dismemberment. After testing this argument statistically, Fazal studied one case of a buffer state, Poland, whose partitions in 1772 and 1793 appeared likely to fit her argument, and one case, the 1916 U.S. occupation of the Dominican Republic, that at first glance appeared by virtue of its geography to be unlikely to classify as a buffer state. Finding that U.S. motives in occupying the Dominican Republic were in fact related to concerns over rising German influence in the Caribbean, Fazal claims that this tough test case corroborates her argument rather than merely illustrating it.

most-similar or most-different cases: FINISH THIS, DRAWING ON GERRING AND SEAWRIGHT PP. 131-147

Pathway case: FINISH THIS, DRAWING ON GERRING AND SEAWRIGHT PP. 122-131

Many of these more approaches to case selection can create misleading results if the researcher, in the absence of additional evidence, generalizes the case study findings to broad populations. This is why case study researchers often limit their findings to “contingent generalizations,” or generalizations on a well-defined subset of a population. At the same time, subsequent statistical research that incorporates the findings and variables of case studies, as in
Reiter’s work, as well as additional case studies on typical or diverse cases, can help establish the validity and scope conditions of case study findings. The combination of statistical and case study methods, in other words, can ease the necessity in statistical studies of making unit homogeneity assumptions or studying only subtypes for which sufficient examples exist for statistical methods.

**Combinations of All Three Methods**

Work combining all three methods has the potential for bringing together many of the comparative advantages of each binary combination of methods. In particular, such work allows for many kinds of iterations between methods: formal models can be tested statistically, then case study methods can focus on finding the potential causes behind the outlier cases, these can then be incorporated into the formal model, which can then be tested statistically, and so on. This is an ideal seldom reached in any single piece of research, however, as it requires extraordinary skills, enormous amounts of work, and longer writings than most journals allow. More often, these kinds of iterations take place sequentially among different pieces of research by researchers using different methods. Still, the two works of those surveyed that combined all three methods, the books by Goemans and Martin, do achieve many of the comparative advantages of each method, and deserve emulation by those willing to develop the demanding skills necessary for such research.

**Conclusions: Encouraging and Improving Multimethod Research**

Combining research methods can contribute greatly to progressive theorizing. Formal-statistical work can bring together the deductive theory-development power of formal models and the theory-testing power of statistical methods to derive and generalize counter-intuitive insights. Formal-case study work can use both deductive and inductive methods to
develop new theories and test them against tough cases. Case study-statistical work can provide convincing evidence that hypothesized causal mechanisms were operative in historical cases and that these mechanisms generalize to wider populations.

The increasingly evident complementarity of case studies, statistical methods, and formal models, and the continuing improvement of graduate training in each of these methods, provide the potential for increasingly sophisticated multimethod research. As each individual method becomes more sophisticated, however, it may become more difficult for any single researcher to be adept at more than one set of methods while also attaining a cutting-edge theoretical and empirical knowledge of their field. Successful collaboration is therefore likely to take the form of several researchers working together using different methods, as in many of the examples discussed above, or of researchers more self-consciously building on the findings generated by those using different methods. In either form, effective collaboration requires that even as they become expert in one methodological approach, scholars must also become conversant with alternative approaches, aware of their strengths and limits, and capable of an informed reading of their substantive results.

Unfortunately, for a variety of historical and institutional reasons, cross-method collaboration is not as common or as well-rewarded in the discipline as it should be. Graduate curricula do not consistently require or even offer courses in all three methods, and case study methods courses in particular are under-represented relative to the proportion of published work that uses these methods (Bennett, Barth, and Rutherford, 2003). Journals in the same subfield—such as the *Journal of Conflict Resolution* and *International Security*—often focus only on one or two methods (formal models and statistics in the former, and case studies in the latter) and infrequently cite one another.

Increasing and improving multimethod work will require important changes in the discipline. The disincentives attached to multi-authored works in the academic culture of political science need to be addressed. Such work is treated in tenure and salary reviews as if the author of a work co-authored by two people, for example, had done less than half the work of
a sole-authored article, whereas anyone who has ever co-authored a work feels with some justification that both authors have done more than half the work of a single article. Authors and journal editors might consider making it standard practice to state in the first footnote or the preface the division of labor among co-authors to help address this problem, but wider changes in the culture of the discipline are needed as well. Cross-method training also has to be improved, both in regular graduate curricula offerings and requirements and in dedicated training programs, such as the ICPSR program centered at the University of Michigan or the Consortium on Qualitative Research Methods (CQRM) institute at Arizona State University, that are designed to improve scholars’ skills in one method or another. Finally, researchers from all three methodological approaches need to collaborate in the hard work of improving the definitions, measurements and operationalizations, and codings of databases. As our concepts and definitions continue to evolve, these databases require continual updating, and formal modelers and case study researchers need to contribute their expertise on concepts and cases to the enormous investment of effort that statistical researchers have made in developing datasets. The disciplinary and technical challenges to multimethod work are daunting, but the exemplary multimethod research that is already available demonstrates that the results will be well worth the effort.

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