

Case-Selection Techniques in Case Study Research: A Menu of Qualitative and Quantitative Options

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ABSTRACT

How can scholars select cases from a large universe for in-depth, case-study analysis? Random sampling is not typically a viable approach when the total number of cases to be selected is small. Hence, attention to purposive modes of sampling is needed. Yet while the existing qualitative literature on case selection offers a wide range of suggestions for case selection, most techniques discussed require in-depth familiarity of each case. Seven case-selection procedures are considered, each of which facilitates a different strategy for within-case analysis. The case selection procedures considered focus on typical, diverse, extreme, deviant, influential, most-similar, and most-different cases.

For each case-selection procedure, quantitative approaches are discussed that meet the goals of the approach while still requiring information that can reasonably be gathered for a large number of cases.

Case-selection is the primordial task of the case study researcher, for in choosing cases one also sets out an agenda for studying those cases. This means that case selection and case analysis are intertwined to a much greater extent in case study research than in large-N cross-case analysis. Indeed, the method of choosing cases and analyzing those cases can scarcely be separated when the focus of a work is on one or a few instances of some broader phenomenon.

Yet, choosing good cases for extremely small samples is a challenging endeavor. Consider that most case studies seek to elucidate the features of a broader population. They are about something larger than the case itself, even if the resulting generalization is issued in a tentative fashion (Gerring 2004). In case studies of this sort, the chosen case(s) is asked to perform a heroic role: to stand for (represent) a population of cases that is often much larger than the case itself. If cases consist of countries, for example, the population might be understood as a region (e.g., Latin America), a particular type of country (e.g., oil-exporters), or the entire world (over some period of time). Evidently, the problem of representativeness cannot be ignored *if* the ambition of the case study is to reflect on a broader population of cases. At the same time, a truly representative case is by no means easy to identify. Additionally, chosen cases must also achieve variation on relevant variables, a requirement that is often unrecognized. A third difficulty is that background cases often play a key role in case study analysis. They are not cases per se, but they are nonetheless integrated into the analysis in an informal manner. This means that the distinction between the case(s) and the population that surrounds it is never as clear in case study work as it is in the typical large-N cross case study. (Further issues are discussed in Gerring 2007: chs 2 & 4).

Despite the importance of the subject, and its evident complexities, the question of case-selection has received relatively little attention from scholars since the pioneering work of Harry Eckstein (1975), Arend Lijphart (1971, 1975), and Przeworski & Teune (1970). One should also mention J.S. Mill's classic *System of Logic* (1834/1872), which continues to inspire commentary nearly two centuries later. To be sure, recent work has noted the problem of sample bias, and debated its sources and impact at great length (Achen and Snidal 1989; Collier and Mahoney 1996; Geddes 1990; King et al. 1994; Rohlfing 2004; Sekhon 2004). But no solutions to this problem have been proffered beyond those implicit in work by Eckstein, Lijphart, and Przeworski & Teune.

In the absence of detailed, formal treatments, scholars continue to lean on pragmatic considerations, as well as a host of terms – case-study types -- invented by Mill, Eckstein and their compatriots. As we shall see, each of these approaches has its limitations.

Pragmatic considerations include such factors as time, money, expertise, access, and theoretical prominence. Some cases are more accessible to intensive study than others, and for this reason may warrant selection. Yet, while pragmatic factors often loom large, none of these factors constitutes a *methodological* explanation for why Case A might be legitimately preferred over Case B. One cannot justify such a choice simply by pointing out the greater convenience of Case A. The best that can be said for a pragmatic choice is that it does not introduce systematic bias into the resulting analysis. Correspondingly, researchers are usually keen to demonstrate that pragmatic case-selection factors are “arbitrary” (stochastic) relative to the causal relationship of interest, and hence unlikely to be correlated with the outcome. Thus, even if, as sometimes happens, cases are chosen for pragmatic reasons it is essential for researchers to understand retroactively how the properties of the selected cases compare with the rest of the population. The tools for systematic case selection described below provide a range of simple, quantitative summaries of such similarity and/or difference along multiple dimensions of possible comparison.

Given the difficulties with pure pragmatic justifications, it is not surprising that case study researchers usually offer additional reasons for their choice of cases. These are characteristically summarized as case-study types -- “extreme,” “deviant,” “crucial,” “most-similar,” and so forth.

Each term offers a somewhat different method of case-selection. However, these terms are poorly understood and often mis-applied. The techniques we discuss below thus offer the possibility for small-N scholars to develop more rigorous and detailed explanations of how their cases relate to the others in a broader universe. Moreover, they offer little practical direction in circumstances where the *potential* cases – the population of cases -- are numerous. How are we to know which cases are deviant, or most-deviant, if the population numbers in the hundreds or thousands? Qualitative inquiry is of little utility in such contexts. Finally, the menu of options provided by Eckstein and colleagues is notably incomplete.

In this paper, we clarify the methodological issues involved in case-selection where the scholar's objective is to build and test general causal theories about the social world on the basis of one or a few cases. We also attempt to provide a more comprehensive menu of options for case-selection in case study work, including some standard items along with a few new additions. Our final objective is to offer new techniques for case-selection in situations where data for key variables is available across a large sample. In these situations, we show that standard statistical techniques may be profitably employed to clarify and systematize the process of case-selection. Thus, with the techniques discussed below, both the case-study analyst and the consumer of research can have a better idea of what the cases under consideration mean for broader theory and substantive generalizations. Of course, this sort of large-N analysis is not practicable in all instances; but where it is – i.e., where data and modeling techniques are propitious – we suggest that it has a lot to offer to case-study research. To the extent that these techniques are successful they may provide a concrete and fruitful integration of quantitative and qualitative techniques, a line of inquiry pursued by a number of recent studies (e.g., Bennett & George 2005; Brady & Collier 2004; Gerring 2001, 2007; Goertz 2006; King et al. 1994; Ragin 2000).

WHY NOT SAMPLE RANDOMLY?

Before exploring specific techniques for case selection in case study research, it is worth asking at the outset whether such approaches are in fact necessary. Given the dangers of selection bias introduced whenever researchers choose their cases in a purposive fashion, perhaps case study researchers should choose cases randomly. Randomization procedures have a long history in large-N research, multiple implementation techniques are readily available, and these procedures are specifically designed to avoid the sort of selection-bias problems that have plagued case study research.

The law of large numbers in statistics (Stone 1996: 110-21) tells us that, if the sample consists of a large enough number of independent random draws, the selected cases are likely to be fairly representative of the overall population on any given variable. Furthermore, because a large number of cases are chosen, the researcher can usually be assured that the sample will provide sufficient variation on key variables. If cases in the population are distributed homogeneously across the ranges of the variables, then it is highly probable that some cases will be included from each important segment of those ranges. (For situations in which cases with theoretically relevant values of the variables are rare, a stratified sample, which oversamples some values, may be employed.) A demonstration of the fact that random sampling is likely to produce a representative sample is shown in Figure 1, which shows a histogram of the mean values of five hundred random samples, each consisting of one thousand cases. For each case, one variable has been measured: a continuous variable that falls somewhere between zero and one. In the population, the mean value of this variable is 0.5. How representative are the random samples? A good way of judging this is to compare the means of each of the five hundred random samples with the population mean. As can

be seen in the figure, all of the sample means are very close to the population mean. So random sampling was a success, and each of the five hundred samples turns out to be fairly representative of the population.

However, in case-study research the sample is small (by definition) and this makes randomization problematic. Consider what would happen if the sample size was changed from one thousand cases to five. The results are shown in Figure 2. On average, these small-N random samples produce the right answer, so the procedure culminates in results that are unbiased. However, many of the sample means are rather far from the population mean, and some are quite far indeed. Hence, even though this case-selection technique produces representative samples *on average*, any given sample may be wildly unrepresentative. In statistical terms, the problem is that small sample sizes tend to produce estimates with a great deal of variance. Therefore, random sampling is highly unreliable in small-N research. Evidently, techniques that parallel quantitative or statistical procedures cannot be expected to have the same desirable properties when choosing a small number of cases that they have when choosing large numbers of cases.

Given the insufficiencies of other approaches to case-selection – e.g., those motivated by pragmatic concerns and those following some version of randomization – the case for systematic, purposive case-selection seems clear. It is true that the latter methods cannot entirely overcome the unreliability of either theoretical or substantive generalization from small-N samples. But they can nonetheless make an important contribution to the inferential process by enabling researchers to choose the most appropriate cases for a given within-case research strategy, which may be either quantitative or qualitative. Consider that a within-case study of the effects of economic crisis on voters' evaluations of politicians would be quite difficult to carry out successfully in a case in which (a) *no* voters perceived themselves as having been affected by economic crisis, or (b) *all* voters saw themselves as having lost economic ground due to the crisis. Hence, case selection for this sort of research question must involve a systematic search for cases in which an economic crisis affected some regions or social groups, but not others, i.e., in which there is variation on relevant parameters. (Sekhon [2004] explains why studies designed to estimate conditional probabilities must select high- and low-scoring cases on the independent variable.) Other strategies for within-case analysis entail their own modes of systematic case selection.¹

TECHNIQUES OF CASE-SELECTION

How, then, are we to choose a sample for case-study analysis? Note that case selection in case study research has the same twin objectives as random sampling. That is, one desires a) a representative sample and b) useful variation on the dimensions of theoretical interest.² One's choice of cases is therefore driven by the way a case is situated on such dimensions within the population of interest. It is from such cross-case characteristics that we derive the following seven case study types: *typical*, *diverse*, *extreme*, *deviant*, *influential*, *most-similar*, and *most-different*.

¹ It is perhaps for this reason that much of the existing qualitative literature on case selection (e.g., Lijphart 1971, 1975, Eckstein 1975) prescribes systematic, if often non-quantitative, approaches to case selection. This essay's emphasis on systematic case selection criteria is thus in line with an important existing qualitative tradition.

² Where multiple cases are chosen the researcher must also be aware of problems of case-independence. However, these problems are in no sense unique to case study work (Gerring 2001: 178-81).

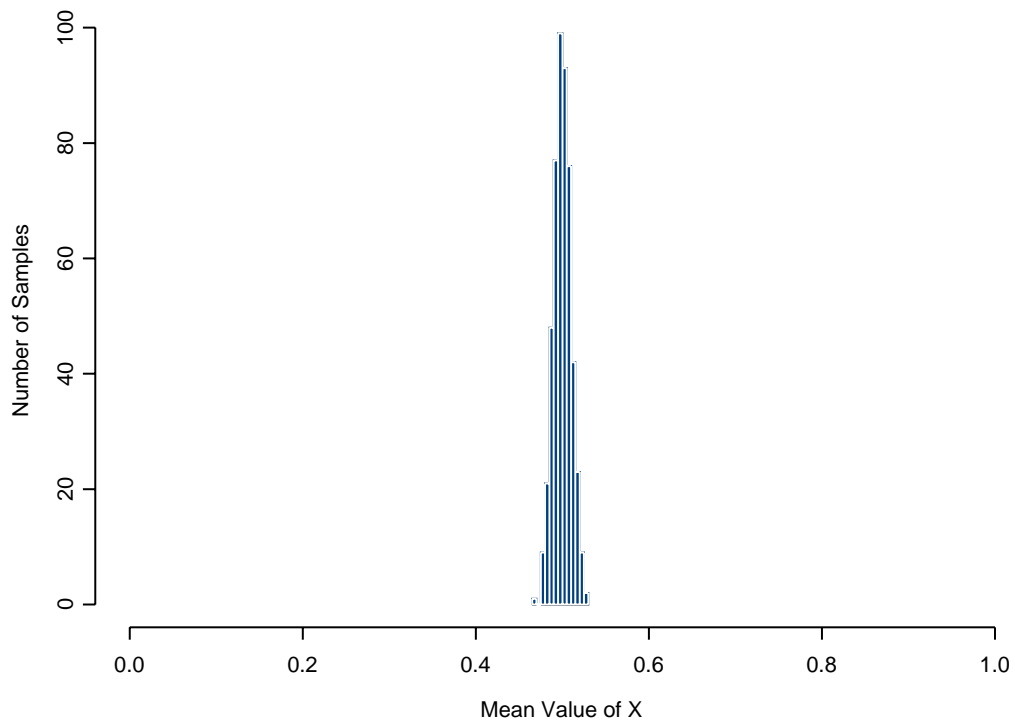


Figure 1: A histogram showing the mean values of one variable in 500 samples of 1000 cases each. The population mean is 0.5.

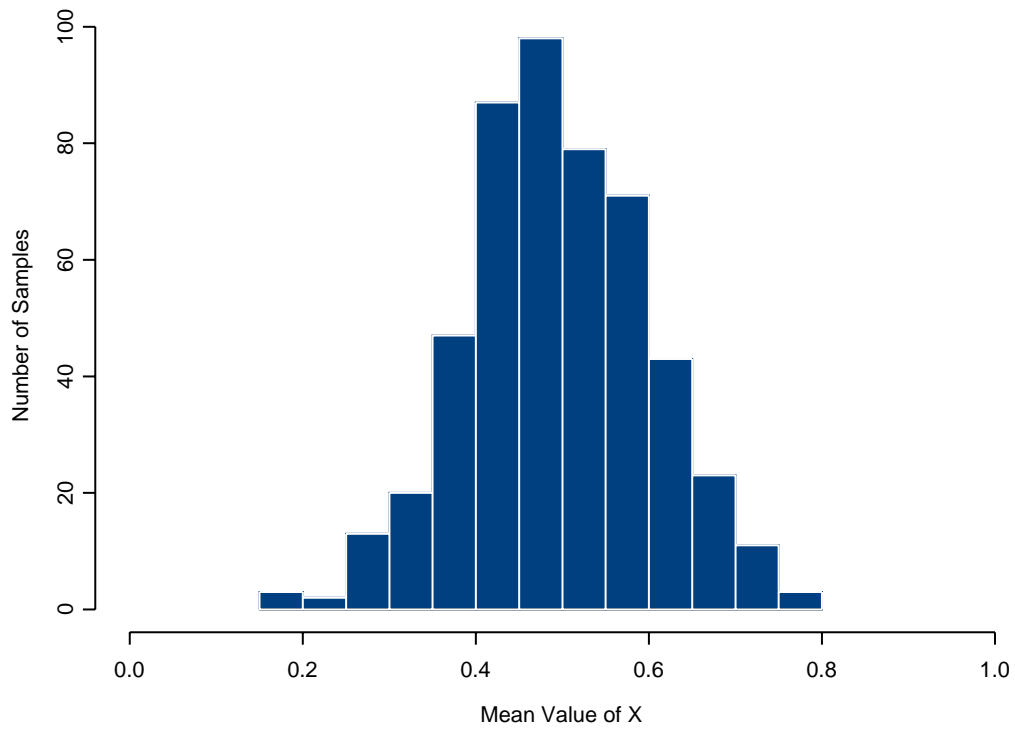


Figure 2: A histogram showing the mean values of one variable in 500 samples of 5 cases each. The population mean is, once again, 0.5.

Most of these terms will be familiar to the reader from studies published over the past century (e.g., by Mill, Eckstein, Lijphart, Przeworski & Teune). What bears emphasis is the variety of methodological purposes that these case-selection techniques presume.

Table 1 summarizes each case study type, including its general definition, a technique for identifying it within a large-N population, its uses, and its probable representativeness. Note that most of these methods may be practiced on a single case, while three – the diverse, most-similar, and most-different – require at least two cases. However, all *may* employ additional cases, with the proviso that, at some point, they will no longer offer an opportunity for in-depth analysis and will thus no longer be “case studies” in the usual sense.

Note also that all of the following research designs involve at least one independent variable (X) and one dependent variable (Y). If the analyst seeks to explain a puzzling outcome but has no preconceptions about its causes then the research is described as *Y-centered*. If a researcher plans to investigate the effects of a particular cause with no preconceptions about what these effects might be, the research is described as *X-centered*. Note that both *Y-* and *X-centered* research begins with a research question but without a hypothesis. If, on the other hand, an analyst wants to explore a particular causal relationship the research is described as *X/Y-centered*, for it connects a particular cause or set of causes with a particular outcome.

Our discussion of various techniques will be fairly straightforward: we will briefly state an idea about case selection from the tradition of case-study research, we will specify the central issue involved in that approach to case selection, and then we will review available statistical tools for addressing this issue. It should be clear that the goal of this paper is not to develop new quantitative estimators but rather to show how existing estimators can be put to good use in case selection.

The exposition will be guided by an ongoing example, the (presumably causal) relationship between per capita GDP and level of democracy (Lipset 1959). Figure 3 displays the basic data about this relationship in the form of a scatterplot. The Democracy variable from the Polity IV dataset (Marshall and Jaggers 2000) is used as a measure of democracy; per capita GDP data is taken from the Penn World Tables dataset (Summers and Heston 1991). The classical result in this field is strikingly illustrated: wealthy countries are almost exclusively democratic. For heuristic purposes, certain unrealistic simplifying assumptions will be adopted. We shall assume, for example, that the Polity measure of democracy is continuous and unbounded (but see Trier and Jackman 2003). We shall assume, more importantly, that the true relationship between economic development and democracy is log-linear, positive, and causally asymmetric, with economic development treated as exogenous and democracy as endogenous (but see Gerring et al. 2005; Przeworski et al. 2000).

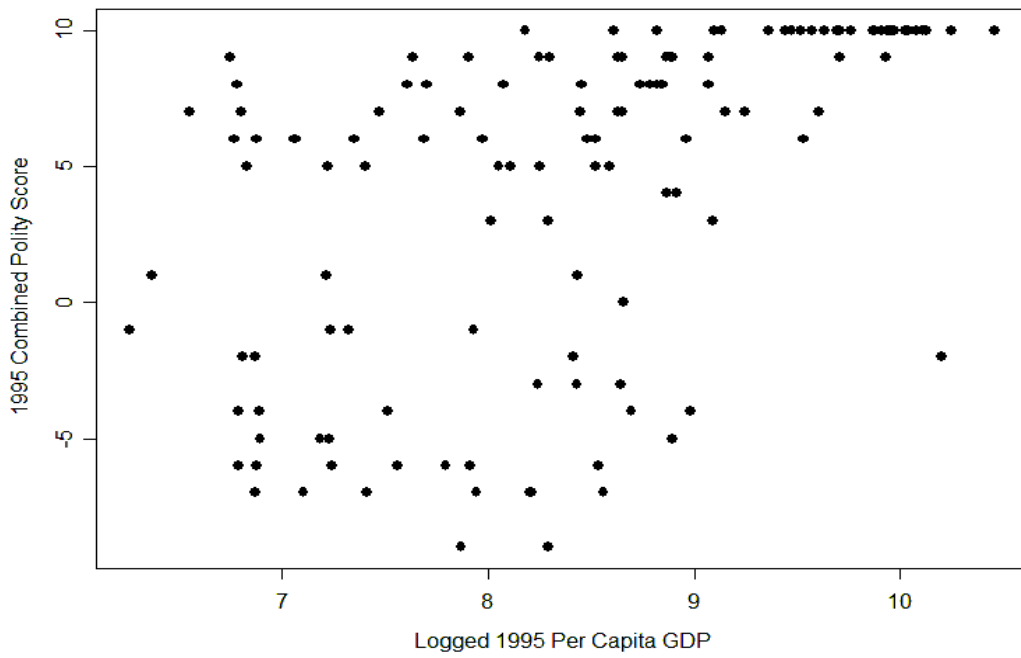


Figure 3: A scatterplot showing level of democracy (on the vertical axis) and level of wealth (on the horizontal axis) of all available countries in 1995. A total of 131 countries have 1995 scores in both data sets.

Table 1:
Cross-Case Methods of Case-Selection and Analysis

1. Typical

- *Definition:* Cases (1 or more) are typical examples of some cross-case relationship.
- *Large-N technique:* A low-residual case (on-lier).
- *Uses:* Confirmatory. To probe causal mechanisms that may either confirm or disconfirm a given theory.
- *Representativeness:* By definition, the typical case is representative, given the specified relationship.

2. Diverse

- *Definition:* Cases (2 or more) exemplify diverse values of X, Y, or X/Y.
- *Large-N technique:* Diversity may be calculated by a) categorical values of X or Y (e.g., Jewish, Catholic, Protestant), b) standard deviations of X or Y (if continuous), c) combinations of values (e.g., based on cross-tabulations, factor analysis, or discriminant analysis).
- *Uses:* Exploratory or confirmatory. Illuminates the full range of variation on X, Y, or X/Y.
- *Representativeness:* Diverse cases are likely to be representative in the minimal sense of representing the full variation of the population. (Of course, they may not mirror the *distribution* of that variation in the population.)

3. Extreme

- *Definition:* Cases (1 or more) exemplify extreme or unusual values on X or Y relative to some univariate distribution.
- *Large-N technique:* A case lying many standard deviations away from the mean of X or Y.
- *Uses:* Exploratory. Open-ended probe of X or Y.
- *Representativeness:* Achievable only in comparison with a larger sample of cases.

4. Deviant

- *Definition:* Cases (1 or more) deviate from some cross-case relationship.
- *Large-N technique:* A high-residual case (outlier).
- *Uses:* Exploratory or confirmatory. To probe new explanations for Y, to disconfirm a deterministic argument, or to confirm an existing explanation (rare).
- *Representativeness:* After the case study is conducted it may be corroborated by a cross-case test, which includes a general hypothesis (a new variable) based on the case study research. If the case is now an on-lier, it may be considered representative of the new relationship.

5. Influential

- *Definition:* Cases (1 or more) with influential configurations of the independent variables.
- *Large-N technique:* Hat matrix or Cook's Distance.
- *Uses:* Confirmatory. To double-check cases that influence the results of a cross-case analysis.
- *Representativeness:* An influential case is typically not representative. If it were typical of the sample as a whole, it would not have unusual influence on estimates of the overall relationship.

6. Most-similar

- *Definition:* Cases (2 or more) are similar on specified variables other than X₁ and/or Y.
- *Large-N technique:* Matching.
- *Uses:* Exploratory if the hypothesis is X- or Y-centered. Confirmatory if X/Y-centered.
- *Representativeness:* Most-similar cases that are broadly representative of the population will provide the strongest basis for generalization.

7. Most-different

- *Definition:* Cases (2 or more) are different on specified variables other than X₁ and Y.
- *Large-N technique:* The inverse of the most-similar method of large-N case selection (see above).
- *Uses:* Exploratory or confirmatory. To a) eliminate necessary causes (definitively), or to b) provide weak evidence of the existence of a causal relationship.
- *Representativeness:* Most-different cases that are broadly representative of the population will provide the strongest basis for generalization.

Note: X₁ refers to the causal factor of theoretical interest.

CAVEATS AND CLARIFICATIONS

The case-selection procedures outlined in Table 1 and discussed at greater length below properly apply to some case studies -- but not all. Before delving into details, it is important to clarify the scope of this analysis lest our conclusions be extended beyond their intended purview.

Begin with the well-recognized fact that the key term, “case study,” is ambiguous, and refers to a heterogeneous set of research designs (Gerring 2004, 2007). In this study, we insist upon a fairly narrow definition: the intensive qualitative or quantitative analysis of a single unit or a small number of units (the cases) where the researcher’s goal is to understand a larger class of similar units (a population of cases). Note that some case studies (so-called) aim to elucidate features specific to a particular case. Here, the problem of case-selection does not exist (or is at any rate minimized), for the case of primary concern has been identified, a priori. This style of case study work is discussed in a companion piece (Gerring 2006).

A second matter of definition concerns the goals undertaken by a researcher. In this study, we are concerned primarily with causal inference, rather than inferences that are descriptive or predictive in nature. The reader should keep in mind that case studies that are largely descriptive in nature may not follow similar procedures of case-selection.

A third matter of clarification concerns the population of the (causal) inference. In perusing the different techniques listed in Table 1 it will be apparent that most of these depend upon a clear idea of what the breadth of the chief inference is. It is only by reference to this larger set of cases that one can begin to think about which cases might be most appropriate for in-depth analysis. If nothing -- or very little -- is known about the population, the methods described in this study cannot be implemented, or will have to be re-implemented once the true population becomes apparent. Thus, a case study whose primary purpose is “casing” – establishing what constitutes a case, and by extension what constitutes the population (Ragin 1992) – will not be able to make use of the techniques discussed here.

Several caveats pertain to the use of statistical reasoning in the selection of cases. First, the population of the inference must be reasonably large; otherwise, statistical techniques are inapplicable. Second, relevant data must be available for that population, or a sizeable sample of that population, on all of the key variables, and the researcher must feel reasonably confident in the accuracy and conceptual validity of these variables. Third, all the standard assumptions of statistical research (e.g., identification, specification, robustness, measurement error) must be carefully considered. Often, a central goal of the case study is to clarify these assumptions or correct errors in statistical analysis, so the process of in-depth study and case selection may be an interactive one. We shall not dilate further on these matters except to warn the researcher against the unthinking use of statistical techniques.

Finally, it is important to note that our discussion disregards three important considerations pertaining to case-selection: a) pragmatic, logistical issues, b) the theoretical prominence of a case in the literature on a topic, and c) within-case characteristics of a case. All of these factors influence a researcher’s selection of cases, and rightly so. However, the first two factors are not methodological in character; they do not bear on the validity of an inference stemming from case study research. Moreover, there is not much that can be said about them that is not already self-evident to the researcher. The third factor is methodological, properly speaking, and there is a great deal to be said about it (Gerring and McDermott forthcoming). If, for example, Case A provides an example of a change in one of the key variables of interest then it may be preferred over Case B for this reason alone. If that change is quasi-experimental in nature then Case A is even more likely to provide a useful tool for causal analysis. In this study, however, we focus on factors of case-selection that rest

on the *cross-case* characteristics of a case – how the case fits into the theoretically-specified population. This is how the term “case-selection” is typically understood, so we are simply following convention by dividing up the subject in this manner.³

TYPICAL CASE

The typical case study focuses on a case that exemplifies a stable, cross-case relationship. By construction, the typical case may also be considered a *representative* case, according to the terms of whatever cross-case model is employed. Indeed, the latter term is often employed in the psychological literature (e.g., Hersen and Barlow 1976: 24).

Because the typical case is well-explained by an existing model, the puzzle of interest to the researcher lies *within* that case. Specifically, the researcher wants to find a typical case of some phenomenon so that she can better explore the causal mechanisms at work in a general, cross-case relationship. This exploration of causal mechanisms may lead toward several different conclusions. If the existing theory suggests a specific causal pathway, then the researcher may perform a pattern-matching investigation, in which the evidence at hand (in the case) is judged according to whether it validates the stipulated causal mechanisms or not. If it does not, the researcher may try to show that the causal mechanisms are other than previously stipulated. Or she may argue that there are *no* plausible causal mechanisms connecting this independent variable with this particular outcome. In the latter case, a typical-case research design may provide disconfirming evidence of a general causal proposition. But the usual employment of a typical case is to provide support for, or clarification of, an existing causal hypothesis.

Large-N Analysis. One may identify a typical case from a large population of potential cases by examining the residual – the distance between the predicted value and the actual (measured) value -- for all cases in a multivariate analysis. Granted, in a large sample there will often be many cases with almost identical high (i.e., near-zero) typicality scores. In such situations, researchers may elect not to focus on the cases with the highest estimated typicality, for such estimates may not be accurate enough to distinguish among several almost-identical cases. Instead, researchers may choose to randomly select from the set of cases with very high typicality, or even to choose from among these cases according to non-methodological criteria. However, scholars should try to avoid selecting from among the set of typical cases in a way that is correlated with relevant omitted variables; such selection procedures complicate the task of causal inference.

Returning to the example introduced above, involving the relationship between per capita GDP and level of democracy, how might a set of typical cases be selected? In this instance, it is relatively easy to specify an appropriate function capturing the relevant relationship. After all, the y variable is simply the Polity democracy score, and there is only one independent variable: logged per capita GDP. Hence, the simplest relevant model is:

$$E(\text{Polity}_i) = \beta_0 + \beta_1 \text{GDP}_i \quad (3)$$

³ It may be worthwhile to recall that case selection is often an iterative process; within-case research may suggest revisions to the statistical techniques used to select cases, potentially leading to a new sample and new opportunities for within-case analysis. Nonetheless, the distinction between within-case and cross-case analysis is indispensable.

Scholars may also wish to include other nonlinear transformations of the logged per capita GDP variable, in order to allow a more flexible functional form. In the current example, we will add a quadratic term. Hence, the model to be considered is:

$$E(\text{Polity}_i) = \beta_0 + \beta_1 \text{GDP}_i + \beta_2 \text{GDP}_i^2 \quad (4)$$

For the purposes of selecting typical cases, the specific coefficient estimates are relatively unimportant, but we will report them, to two digits after the decimal, for the sake of completeness:

$$E(\text{Polity}_i) = 10.52 - 4.59 \text{GDP}_i + 0.45 \text{GDP}_i^2 \quad (5)$$

Much more important are the residuals for each case. Figure 4 shows a histogram of these residuals. Obviously, a fairly large number of cases have quite low residuals and are therefore considered typical. A higher proportion of cases fall far below the regression line than far above it, suggesting either that the model may be incomplete or that the error term does not have a normal distribution. Hopefully, within-case analysis will be able to shed light on the reasons for the asymmetry.⁴

Because of the large number of cases with quite small residuals, the researcher will have a range of options for selecting typical cases. In fact, in this example, 26 cases have a typicality score between 0 and -1. Any or all of these might reasonably be selected for within-case analysis as typical cases with respect to the model described in Equation 4.

Conclusion. Typicality responds to the first desideratum of case selection, that the chosen case be representative of a population of cases (as defined by the primary inference). Even so, it is important to remind ourselves that the single-minded pursuit of representativeness does not ensure that it will be achieved. Note that the test of typicality introduced here, the size of a case's residual, can be misleading if the statistical model is misspecified. And it provides little insurance against errors that are purely stochastic. A case may lie directly on the regression line but still be, in some important respect, atypical. For example, it might have an odd combination of values; the interaction of variables might be different from other cases; or additional causal mechanisms might be at work.

DIVERSE CASES

A second case-selection strategy has as its primary objective the achievement of maximum variance along relevant dimensions. We refer to this as a *diverse-case* method. For obvious reasons, this method requires the selection of a set of cases – at minimum, two – which are intended to represent the full range of values characterizing X_1 , Y , or some particular X_1/Y relationship.⁵ As previously, the investigation is understood to be exploratory when the researcher focuses on X or Y , and confirmatory when she focuses on a particular X_1/Y relationship (a specific hypothesis).

⁴ In this example, the asymmetry is probably due to the failure of the model to take into account the restricted range of the dependent variable, as discussed above.

⁵ This method has not received much attention on the part of qualitative methodologists; hence, the absence of a generally recognized name. It bears some resemblance to J.S. Mill's Joint Method of Agreement and Difference (Mill 1834/1872), which is to say, a mixture of most-similar and most-different analysis, as discussed below. Patton (2002: 234) employs the concept of “maximum variation (heterogeneity) sampling.”

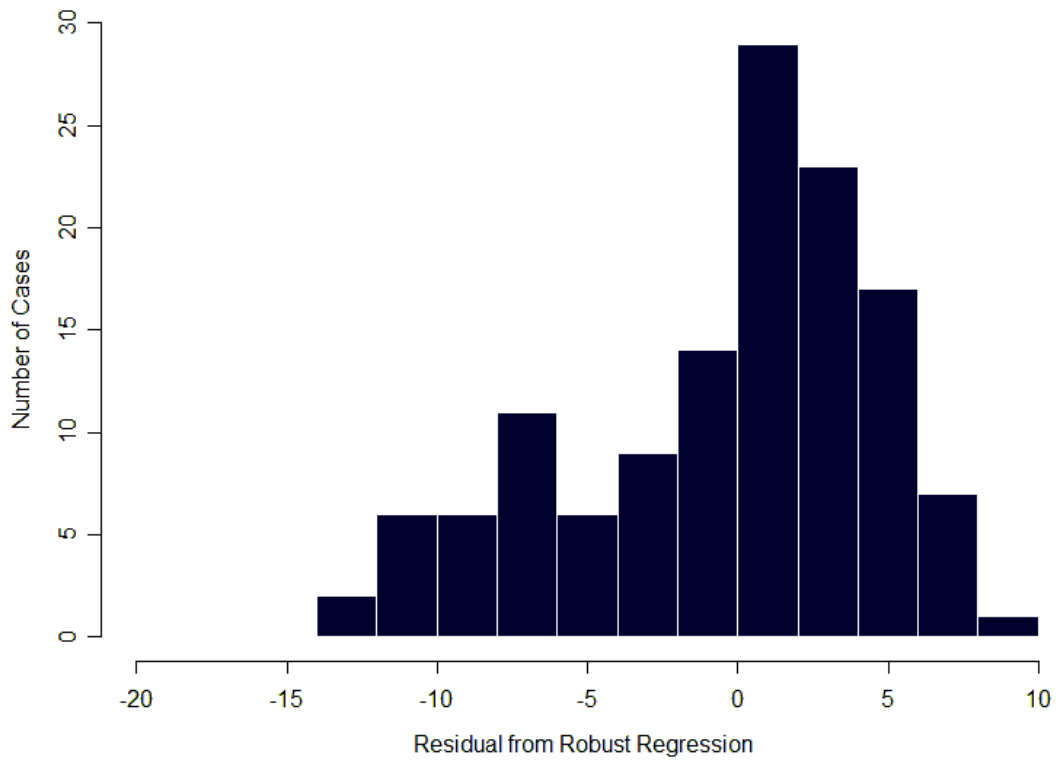


Figure 4: A histogram of the residuals from a robust regression of logged per capita GDP on level of democracy.

Where the individual variable of interest is categorical (on/off, red/black/blue, Jewish/Protestant/Catholic), the identification of diversity is readily apparent. The investigator simply chooses one case from each category. For a continuous variable, the choices are not so obvious. However, the researcher usually chooses both extreme values (high and low), and perhaps the mean or median as well. The researcher may also look for break-points in the distribution that seem to correspond to categorical differences among cases. Or she may follow a theoretical hunch about which threshold-values count, i.e., which are likely to produce different values on Y. Where the causal factor of interest is a *vector* of variables, and where these factors can be measured, the researcher may simply combine various causal factors into a series of cells, based upon cross-tabulations of factors deemed to have an effect on Y. Let us say that some outcome is thought to be affected by sex, race (black/white), and marital status. Here, a diverse-case strategy of case-selection would identify one case within each of these intersecting cells – a total of eight cases. Things become slightly more complicated when one or more of these factors is continuous, rather than dichotomous, since the researcher will have to arbitrarily re-define that variable as a categorical variable (as above). Where causal variables are continuous and the outcome is dichotomous, the researcher may employ discriminant analysis to identify diverse cases.

Diversity may also be understood in terms of various causal paths running from exogenous factors to a particular outcome. Perhaps X_1 , X_2 , and X_3 all cause Y, but they do so independently of each other and in different ways. Each is a sufficient cause of Y.⁶ George and Smoke, for example, wish to explore different types of deterrence failure – by “fait accompli,” by “limited probe,” and by “controlled pressure.” Consequently, they wish to find cases that exemplify each type of causal mechanism.⁷ This may be identified by a traditional form of path analysis, by Qualitative Comparative Analysis (QCA), by sequence analysis, or by informal (non-quantitative) methods.⁸

“Diversity” may thus refer to a range of variation on X or Y, to a particular combination of causal factors, or to various causal pathways. In each instance, the goal of case-selection is to capture the full range of causal types along the dimension of interest -- either X_1 , Y, or X_1/Y .

Large-N Analysis. Diverse-case selection is easily accommodated in a large-N context by using some version of stratified random sampling. In this approach, the researcher identifies the different substantive categories of interest, as well as the number of cases to be chosen from each category. Then, the needed cases may be randomly chosen from among those available in each category (Cochran 1977).

One assumes that the identification of diverse categories of cases will, at the same time, identify categories that are *internally* homogenous (in all respects that might affect the causal relationship of interest). Because of the small number of cases to be chosen, the cases selected are not guaranteed to be representative of each category. Nevertheless, if the categories are carefully constructed, the researcher should in principle be indifferent among cases within a given category. Hence, random sampling is a sensible tie-breaker. However, if there is suspected diversity within

⁶ This sometimes referred to as causal equifinality (Elman 2005; George and Bennett 2004).

⁷ More precisely, George and Smoke (1974: 534, 522-36, ch 18; see also discussion in Collier and Mahoney 1996: 78) set out to investigate causal pathways and discovered, through the course of their investigation of many cases, these three causal types. Yet, for our purposes what is important is that the final sample include at least one representative of each “type.”

⁸ Path analysis is discussed in most introductory statistics texts. QCA is discussed in Drass and Ragin (1992), Hicks (1999: 69-73), Hicks, Misra, Ng (1995), Ragin (1987, 2000), and several chapters by Ragin in Janoski and Hicks (1993). Sequence analysis is explained in Abbott and Tsay (2000).

each category, then measures should be taken to assure that the chosen cases are typical of each category. A case study should not focus on an atypical member of a sub-group.

Indeed, considerations of diversity and typicality often go together. Thus, in a study of globalization and social welfare systems, Duane Swank first identifies three distinctive groups of welfare states: “universalistic” (social democratic), “corporatist conservative,” and “liberal.” Next, he looks within each group to find the most-typical cases. He decides that the Nordic countries are more typical of the universalistic model than the Netherlands since the latter has “some characteristics of the occupationally based program structure and a political context of Christian Democratic-led governments typical of the corporatist conservative nations.”⁹

Conclusions. Encompassing a full range of variation is likely to enhance the representativeness of the sample of cases chosen by the researcher. This is a distinct advantage. Of course, the inclusion of a full range of variation may distort the actual distribution of cases across this spectrum. If there are more “high” cases than “low” cases in a population and the researcher chooses only one high case and one low case, the resulting sample of two is not perfectly representative. Even so, the diverse-case method probably has stronger claims to representativeness than any other small-N sample (including the typical case).

EXTREME CASE

The extreme-case method selects a case because of its extreme value on the independent (X) or dependent (Y) variable of interest. Thus, studies of domestic violence may focus on extreme instances of abuse (Browne 1987). Studies of altruism may focus on those who risked their lives to help others (e.g., Holocaust resisters; Monroe 1996). Studies of ethnic politics may focus on the most heterogeneous societies in order to better understand the role of ethnicity in a democratic setting (e.g., Papua New Guinea; Reilly 2000/2001). Studies of industrial policy focus on the most successful countries (i.e., the NICs; Deyo 1987). And so forth (for further examples see Collier and Mahoney 1996; Geddes 1990).

The notion of “extreme” may now be defined more precisely. An extreme value is an observation that lies far away from the mean of a given distribution. That is to say, it is *unusual*. If most cases are positive along a given dimension, then a negative case constitutes an extreme case. If most cases are negative, then a positive case constitutes an extreme case. Evidently, one is not simply concerned with cases where something “happened,” but also with cases where something did not. For case-study analysis, it is often the rareness of the value that makes a case valuable, not its positive or negative value (contrast Emigh 1997; Mahoney and Goertz 2004; Ragin 2000: 60; Ragin 2004: 126).

Large-N Analysis. As we have said, extreme cases lie far from the mean of a variable.

Extremity (E) for the *i*th case, can be defined in terms of the sample mean (\bar{X}) and the standard deviation (s) for that variable:

$$E_i = \left| \frac{X_i - \bar{X}}{s} \right|$$

⁹ Swank (2002: 11). See also Esping-Andersen (1990).

This definition of extremity is the absolute value of the Z-score (Stone 1996: 340) for the i th case. Cases with a large E_i qualify as extreme. Decisions about how large the extreme-ness needs to be in order for cases to count as extreme are, to some extent, arbitrary. However, some general guidelines can be offered. In keeping with statistical tradition, cases with an extremeness score smaller than 2 would generally not be considered extreme, even if they are the most extreme cases in the sample. If the researcher wishes to be somewhat conservative in classifying cases as extreme, a higher threshold such as 3, can be used. In general, the choice of threshold is left to the researcher, to be made in a way that is appropriate to the research problem at hand.

The mean of the democracy measure is 2.76, suggesting that, on average, the countries in the 1995 data set tend to be somewhat democratic. The standard deviation is 6.92, implying that there is a fair amount of scatter around the mean in these data.

Figure 5 shows a histogram of the extremeness scores for all countries on level of democracy. As can easily be seen, no cases have extremeness scores greater than two. Hence, some flexibility is required in choosing extreme cases on this variable. The two countries with the largest extremeness scores are Qatar and Saudi Arabia, both of which have an extremeness of 1.84. These countries, which both have a democracy score of -10 for 1995, are probably the two best candidates for extreme cases.

Conclusion. The extreme-case method appears to violate the social science folk wisdom warning us not to “select on the dependent variable.”¹⁰ Selecting cases on the dependent variable is indeed problematic if a number of cases are chosen, all of which lie on one end of a variable’s spectrum (they are all positive *or* all negative), and if the researcher then subjects this sample to cross-case analysis as if it were representative of a population.¹¹ Results for this sort of analysis would almost assuredly be biased. Moreover, there will be little variation to explain since the contrasting values of each case are explicitly constrained.

However, this is not the proper use of the extreme-case method. (It is more appropriately labeled an extreme-*sample* method.) The extreme-case method refers back to a larger sample of cases that lie in the background of the analysis and provide a full range of variation as well as a more representative picture of the population. It is a conscious attempt to *maximize* variance on the dimension of interest, not to minimize it. If this population of cases is well understood -- either through the author’s own cross-case analysis, through the work of others, or through common sense -- then a researcher may justify the selection of a single case exemplifying an extreme value for within-case analysis. If not, the researcher may be well-advised to follow a diverse-case method (see below).

¹⁰ Geddes (1990), King, Keohane, and Verba (1994). See also discussion in Brady and Collier (2004), Collier and Mahoney (1996), Rogowski (1995).

¹¹ The exception would be a circumstance in which the researcher intends to disprove a deterministic argument (Dion 1998).

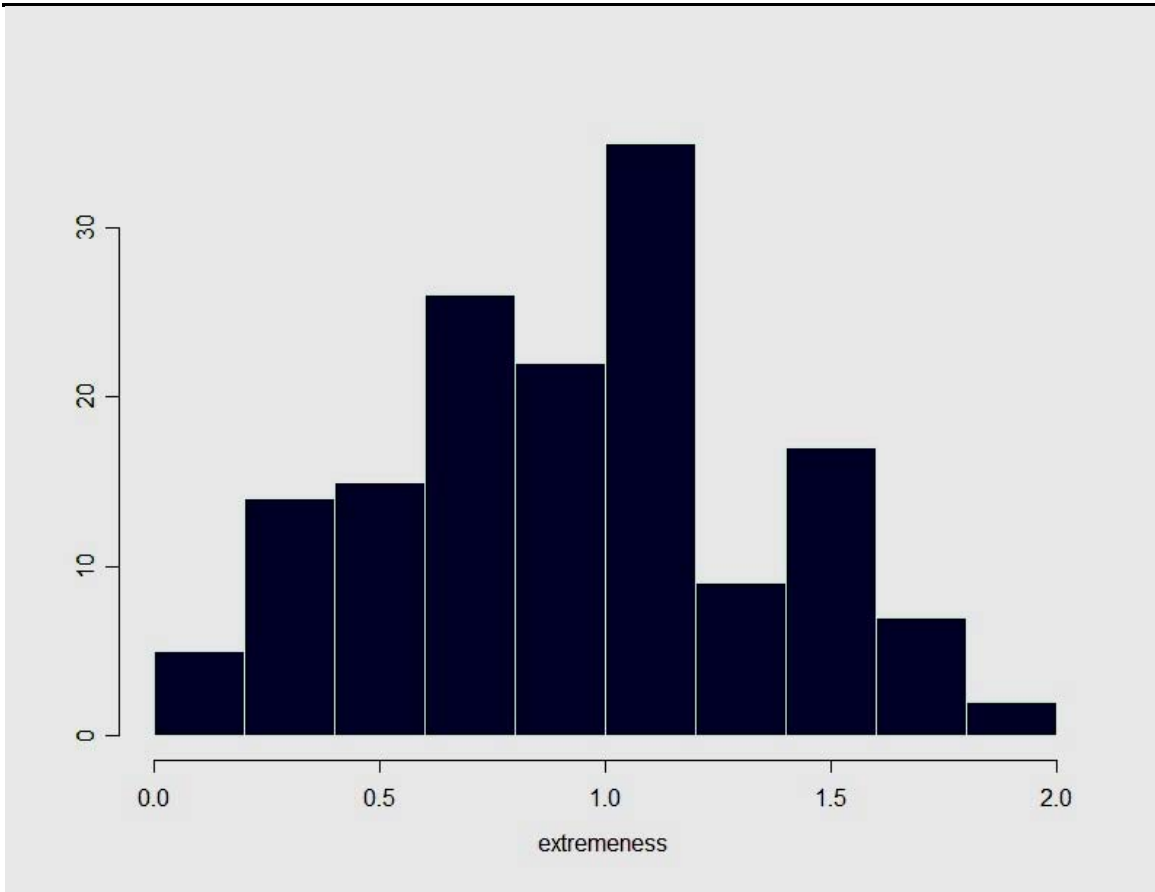


Figure 5: A histogram of the extremeness scores for all countries in the data set on level of democracy.

By way of conclusion, let us return to the problem of representativeness. In the context of causal analysis, representativeness refers to a case that exemplifies values on X and Y that conform to a general pattern. In a cross-case model, the representativeness of an individual case is gauged by the size of its residual. The representative case is therefore a typical case (as discussed above), not a deviant case (as discussed below). It will be seen that an extreme case may be typical *or* deviant. There is simply no way to tell because the researcher has not yet specified an X/Y causal proposition. Once such a causal proposition has been specified we may then ask whether the case in question is similar – in all respects that might affect the X/Y relationship of interest – to some population of cases. It is at this point that it becomes possible to say (within the context of a cross-case statistical model) whether a case lies near to, or far from, the regression line. However, this sort of analysis means that the researcher is no longer pursuing an extreme-case method. The extreme case method is purely exploratory -- a way of probing possible causes of Y, or possible effects of X, in an open-ended fashion. If the researcher has some notion of what additional factors might affect the outcome of interest, or of what relationship the causal factor of interest might have on Y, then she ought to pursue one of the other methods explored below. This also implies that an extreme-case method might morph into a different kind of approach as a study evolves, that is, as a more specific hypothesis comes to light.

DEVIANT CASE

The *deviant-case* method selects that case(s) which, by reference to some general understanding of a topic (either a specific theory or common sense), demonstrates a surprising value. Barbara Geddes notes the importance of deviant cases in medical science, where researchers are habitually focused on that which is “pathological” (according to standard theory and practice).¹² Likewise, in psychology and sociology case studies may be comprised of deviant (in the social sense) persons or groups. In economics, case studies may consist of countries or businesses that over-perform (e.g., Botswana; Microsoft) or under-perform (e.g., Britain through most of the twentieth century; Sears in recent decades) relative to some set of expectations. In political science, case studies may focus on countries where the welfare state is more developed (e.g., Sweden) or less developed (e.g., the United States) than one would expect, given a set of general expectations about welfare state development. The deviant case is closely linked to the investigation of theoretical anomalies. Indeed, to say “deviant” is to imply “anomalous.”¹³

Note that while extreme cases are judged relative to the mean of a single distribution (the distribution of values along a single variable), deviant cases are judged relative to some general model of causal relations. The deviant-case method selects cases which, by reference to some general cross-case relationship, demonstrate a surprising value. They are “deviant” in that they are poorly explained by the multivariate model. The important point is that deviant-ness can only be assessed relative to the general (quantitative or qualitative) model employed. This means that the

¹² Geddes (2003: 131). For other examples of case work from the annals of medicine see “Clinical Reports” in *The Lancet*, “Case Studies” in *The Canadian Medical Association Journal*, and various issues of the *Journal of Obstetrics and Gynecology*, often devoted to clinical cases (discussed in Jenicek 2001: 7).

¹³ For discussions of the important role of anomalies in the development of scientific theorizing see Elman (2003), Lakatos (1978). For examples of deviant-case research designs in the social sciences see Amenta (1991), Coppedge (2004), Eckstein (1975), Emigh (1997), Kazancigil (1994), Kendall, Wolf (1949/1955).

relative deviant-ness of a case is likely to change whenever the general model is altered. For example, the US is a deviant welfare state when this outcome is gauged relative to societal wealth. But it is less deviant – and perhaps not deviant at all -- when certain additional (political and societal) factors are included in the model (Alesina, Glaeser 2004). Deviance is model-dependent. Thus, when discussing the concept of the deviant case it is helpful to ask the following question: *relative to what general model* (or set of background factors) is Case A deviant?

The purpose of a deviant-case analysis is usually to probe for new – but as yet unspecified – explanations. In this circumstance, the deviant-case method is only slightly more bounded than the extreme-case method. It, too, is an exploratory form of research. The researcher hopes that causal processes within the deviant case will illustrate some causal factor that is applicable to other (deviant) cases. This means that, in most circumstances, a deviant-case study culminates in a general proposition – one that may be applied to other cases in the population. As a consequence, one deviant-case study may lead to a new cross-case model that identifies an entirely different set of deviant cases.

However, there is also a second, less common, reason for choosing a deviant case. If the researcher is interested in *disconfirming a deterministic proposition*, then any deviant case will do so long as it lies within the specified population of the inference (Dion 1998). Deterministic arguments may be framed as necessary, sufficient, or necessary and sufficient. They usually apply to variables that are understood to take dichotomous, rather than continuous, outcomes (or where scalar outcomes are dichotomized as high/low, big/small and so forth). Evidently, if a presumed necessary cause is not present when its effect is present, or a sufficient cause is not accompanied by its presumed effect, then the causal proposition has been disconfirmed. Note that while simply identifying a disconfirming case is enough in some situations, a careful case-study may still be important as a means of persuading readers that the relevant variables have been measured correctly and therefore that the case is, in fact, disconfirming.

Large-N Analysis. In statistical terms, deviant-case selection is the opposite of typical-case selection. Where a typical case is as close as possible to the prediction of a formal, mathematical representation of the hypothesis at hand, a deviant case is as far as possible from that prediction. Hence, referring back to the model developed in Equation 1, we can define the extent to which a case deviates from the predicted relationship as follows:

$$\text{Deviant-ness } (i) = \text{abs}[y_i - E(y_i | \mathbf{x}_{1,i}, \dots, \mathbf{x}_{K,i})] = \text{abs}[y_i - \mathbf{b}_0 + \mathbf{b}_1\mathbf{x}_{1,i} + \dots + \mathbf{b}_K\mathbf{x}_{K,i}] \quad (6)$$

Deviant-ness ranges from 0, for cases exactly on the regression line, to a theoretical limit of positive infinity.¹⁴ Researchers will typically be interested in selecting from the cases with the highest overall estimated deviant-ness.

Note that when the purpose of a deviant-case analysis is exploratory – i.e., when an author is searching for new causal factors that will be relevant across the broader set of cases – then missing variables are usually not problematic. The caveat is that the effort to identify these missing variables must be the focus of an author’s research.

In our running example, the most deviant cases fall below the regression line, as can be seen in Figure 4. In fact, all eight of the cases with a deviant-ness score of more than 10 are below the regression line. Those eight cases are: Croatia, Cuba, Indonesia, Iran, Morocco, Singapore, Syria, and Uzbekistan. An analysis focused on deviant cases might well select a subset of these.

¹⁴ We use the somewhat awkward term “deviant-ness” rather than the more natural “deviance” because deviance already has a somewhat different meaning in statistics.

Conclusion. As we have noted, the deviant-case method is usually an exploratory form of analysis. As soon as a researcher's exploration of a particular case has identified a factor to explain that case, it is no longer (by definition) deviant. (The exception would be a circumstance in which a case's odd combination of values is deemed to be "accidental," and therefore unexplainable by any general model.) If the new explanation can be accurately measured as a single variable (or set of variables) across a larger sample of cases, then a new cross-case model is in order. In this fashion, a case study initially framed as deviant case may transform into some other sort of analysis.

This feature of the deviant-case study also helps to resolve questions about its representativeness. Evidently, the representativeness of a deviant case is problematic since the case in question is, by construction, atypical. However, doubts about representativeness can be mitigated if the researcher generalizes whatever proposition is provided by the case study to other cases. In a statistical model, this is accomplished by the creation of a new variable, as discussed. In a small-N setting, it may be accomplished by the coding of adjacent cases so as to determine whether they confirm (or at least do not openly contradict) the hypothesis. In each scenario, the outcome of this new cross-case analysis should pull the deviant case towards the expected value predicted by the general model. The deviant case is no longer deviant; in statistical terms, its residual has shrunk. It is now typical, or at least more typical.

INFLUENTIAL CASE

Sometimes, the choice of a case is motivated solely by the need to check the assumptions behind some general model of causal relations. In this circumstance the extent to which a case fits the overall model is important only insofar as it might affect the overall set of findings for the whole population. Once cases that do influence overall findings have been identified, it is important to decide whether or not they genuinely fit in the sample (and whether they might give clues about important missing variables). Because the techniques for identifying this sort of case are slightly different than those used to identify the previous sort of deviant case, we apply a new term to this method – the *influential case*. This is consistent with the goal of this style of case study, to explore cases that may be influential vis-à-vis some larger cross-case theory. Influential-case studies can be valuable for identifying and correcting instances of measurement error or omitted variables that potentially have a large impact on a cross-case generalization. As a result, such case studies quite often lead to the development of a new cross-case model that may, in turn, support new studies of typical, deviant, or influential cases.

Large-N Analysis. Influential cases in regression are those cases that, if counterfactually assigned a different value on the dependent variable, would most substantially change the resulting estimates. Two quantitative measures of influence are commonly applied in statistical analysis. The first, often referred to as the leverage of a case, derives from what is called the "hat matrix."¹⁵ An interesting feature of the hat matrix is that it does not depend on the values of the dependent variable. This means that the measure of leverage derived from the hat matrix is, in effect, a

¹⁵ This somewhat curious name derives from the fact that, if the hat matrix is multiplied with the vector containing values of the dependent variable, the result is the vector of fitted values for each case. Typically, the vector of fitted values for the dependent variable is distinguished from the actual vector of values on the dependent variable by the use of the " $\hat{\cdot}$ " or "hat" symbol. Hence, the hat matrix, which produces the fitted values, can be said to put the hat on the dependent variable.

measure of *potential* influence. It tells us how much difference the case would make in the final estimate if it were to have an unusual score on the dependent variable, but it does not tell us how much difference each case actually made in the final estimate. Analysts involved in selecting influential cases will sometimes be interested in measures of potential influence, because such measures are relevant in selecting cases when there may be some *a priori* uncertainty about scores on the dependent variable. Much of the information in such case studies comes from a careful, in-depth measurement of the dependent variable – which may sometimes be unknown, or only approximately known, before the case study begins. The measure of leverage derived from the hat matrix is appropriate for such situations because it does not require actual scores for the dependent variable.

A second commonly-discussed measure of influence in statistics is Cook's distance. This statistic is a measure of the extent to which the estimates of the β_i parameters would change if a given case were omitted from the analysis. This, in turn, depends primarily on two quantities: the size of the regression residual for that case and the leverage for that case. The most influential cases are those with substantial leverage that lie significantly off of the regression line. These cases contribute quite a lot to the inferences drawn from the analysis. In this sense, such cases are vital for maintaining analytic conclusions.

Note that the hat matrix provides a measure of leverage, or potential influence. Based solely on each case's scores on the independent variables, the hat matrix tells us how much a change in (or a measurement error on) the dependent variable for that case would affect the overall regression line. Cook's distance goes further, considering scores on both the independent and the dependent variables in order to actually tell us how much the overall regression estimates would be affected if each case were to be dropped completely from the analysis. This produces a measure of how much actual—and not potential—influence each case has on the overall regression.

Either the hat matrix or Cook's distance may serve as an acceptable measure of influence for selecting case studies, although the differences just discussed must be kept in mind. In the examples below, Cook's distance will be used as the primary measure of influence because our interest is in whether any particular cases might be influencing the coefficient estimates in our democracy-and-development regression.

Figure 6 shows the Cook's distance scores for each of the countries in the 1995 per capita GDP and democracy data set. Most countries have quite low Cook's distances. The three most serious exceptions to this generalization are the numbered lines in the figure: Jamaica (74), Japan (75), and Nepal (105). Of these three, Nepal is clearly the most influential by a wide margin. Hence, any case study of influential cases with respect to the relationship modeled in Equation 4 would have to start with an in-depth consideration of Nepal.

Conclusions. The use of an influential-case strategy of case selection is limited to instances in which a researcher has reason to be concerned that her results are being driven by one or a few cases. This is most likely to be true in small-to-moderate sized samples. Where N is very large -- greater than 1000, let us say -- it is extremely unlikely that a small set of cases (much less an individual case) will play an "influential" role. Of course, there may be influential *sets* of cases, e.g., countries within a particular continent or cultural region, or persons of Irish extraction. Sets of influential observations are often problematic in a time-series cross-section dataset where each unit (e.g., country) contains multiple observations (through time), and hence may have a strong influence on aggregate results. Still, the general rule is: the larger the sample, the less important individual cases are likely to be and, hence, the less likely a researcher is to use hat matrix and Cook's distance statistics for purposes of case selection. In these instances, it may not matter very much what values individual cases display. (It may of course matter for the purpose of investigating causal mechanisms; however, for this purpose one would not employ influential statistics to choose cases.)

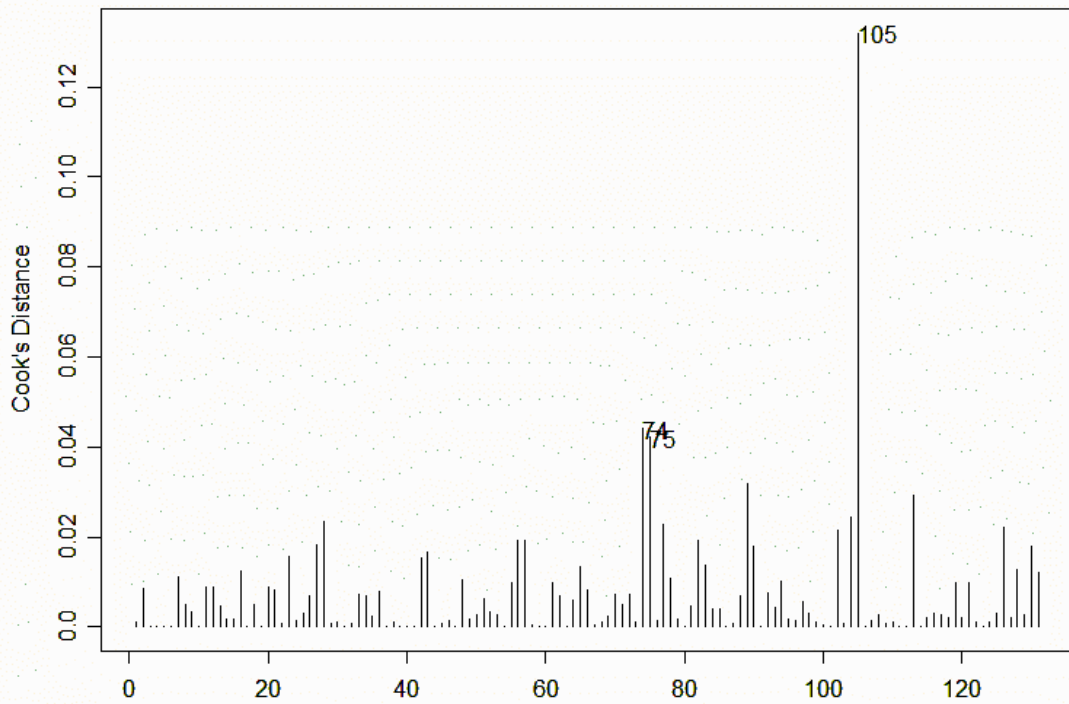


Figure 6. The Cook's distance scores for an OLS regression of democracy on logged per capita GDP. The three numbered cases have high Cook's distance scores.

MOST-SIMILAR/MOST-DIFFERENT CASES

The most-similar method, like the diverse-case method, employs a minimum of two cases.¹⁶ In its purest form, the chosen pair of cases is similar on all the measured independent variables *except* the independent variable of interest. Table 2 offers a stylized example of the simplest sort of most-similar analysis, with only two cases and with all variables measured dichotomously. Here, the two cases are similar in various respects that might be relevant to the outcome of interest, as signified by X_2 . This is the vector of control variables, and they are constant across the cases. The cases differ, however, on one causal variable – X_1 – and on the outcome. Thus, it is presumed that the presence or absence of this particular factor causes variation on the outcome.¹⁷

Large-N Analysis. Having outlined the most-similar research design, we turn to the question of how to identify such cases from within a large-N cross-case dataset. For heuristic purposes, we shall focus on two-case comparisons. Readers should be aware that this can, and often should, be adapted to more complex comparisons.

The most useful statistical tool for identifying cases for in-depth analysis in a most-similar setting is probably some variety of “matching” strategy.¹⁸ Statistical estimates of causal effects based on matching techniques have been a major topic in quantitative methodology over the last twenty-five years, first in statistics (Rosenbaum and Rubin 1983; Rosenbaum 2002) and subsequently in econometrics (Hahn 1998; Hirano, Imbens, and Ridder 2003) and political science (Ho, Imai, King and Stewart 2004; Imai 2005). Matching techniques are based on an extension of experimental logic. In a randomized experiment, elaborate statistical models are unnecessary for causal inference — because, for a large enough selection of cases, the treatment group and the control group have a very high probability of being quite similar, on both measured and unmeasured variables (other than the independent variable and its effects). Hence, quite simple statistical treatments (e.g., a difference of means test) may be sufficient to demonstrate a causal inference.

In observational studies, it is quite unusual to find situations in which the cases with a high score on the independent variable (which roughly correspond to the treatment group in an experiment) are similar on all measured and unmeasured variables—once again, other than the independent variable and its effects—to the cases with a lower score on the independent variable (corresponding to the control group). Typically, the treatment group in an observational study will differ in many ways from the control group. For example, in studies that seek to estimate the effects of democracy on wages to labor (e.g., Rodrik 1998), it is a problem that democracies are, on average, wealthier than other countries. After all, wealth may condition different approaches to economic distribution, thus introducing a confounding variable into the analysis.

¹⁶ Lijphart (1971, 1975), Meckstroth (1975), Przeworski and Teune (1970), Skocpol and Somers (1980). Sometimes, the most-similar method is known as the “method of difference” (Mill 1843/1872).

¹⁷ For examples of the most-similar method see Brenner (1976), Epstein (1964), Hamilton (1977), Lipset (1968), Moulder (1977), Posner (2004).

¹⁸ For good introductions see Ho et al. (2004), Morgan and Harding (2005), Rosenbaum (2004), Rosenbaum and Silber (2001). For a discussion of matching procedures in Stata see Abadie et al. (2001).

Table 2:
Most-Similar Analysis with Two Cases

| <i>Cases</i> | <i>Variables</i> | | |
|--------------|------------------|-------|-----|
| | X_1 | X_2 | Y |
| #1 | + | + | + |
| #2 | - | + | - |

+/-: the score demonstrated by a case on a particular dimension (variable), coded dichotomously. X_1 : the variable of theoretical interest. X_2 : the background/control variable or vector. Y : the outcome. Note that in this example all variables are positively correlated with the outcome.

One common approach to this problem is to introduce a variable for each potential confounder (e.g., wealth) in a general analysis of causal relationships (e.g., a regression model). Matching techniques have been developed as an explicit alternative to this control-variable approach. This alternative approach begins by identifying a set of variables (other than the dependent variable or the main independent variable) on which the cases are to be matched. Then, for each case in the treatment group, the researcher identifies cases from the control group with, if possible, the exact same scores on the matching variables (the covariates). Finally, the scholar looks at the difference on the dependent variable between the cases in the treatment group and the matching cases in the control group. If the set of matching variables is broad enough to include all confounders, the average difference between the treatment group and the matching control cases should provide a good estimate of the causal effect. Even in a situation in which the set of matching variables includes some, but not all, confounders, matching may produce better causal inferences than regression and related models because cases that match on a set of explicitly selected variables may also be more likely to be similar on unmeasured confounders.¹⁹

Unfortunately, in many observational studies the matching procedure described above – known as exact matching -- is impossible. This procedure fails for continuous variables such as wealth, age, or distance, since there are generally no two cases with precisely the same score on a continuous variable. For example, there is no undemocratic country with exactly the per capita GDP of the United States. Moreover, the larger the number of matching variables employed (either dichotomous or continuous), the lower the likelihood of finding exact matches.

In situations where exact matching is infeasible, researchers may instead employ approximate matching, in which cases from the control group that are “close enough” to matching cases from the treatment group are accepted as matches. Major weaknesses of this approach include the fact that multiple competing definitions of “close enough” inevitably exist, as well as the fact that, for large enough sets of matching variables, few if any treatment cases may have even approximate matches. Furthermore, scholars are faced with the need to choose some way of trading off between different degrees of mismatch on various independent variables.

To better deal with situations in which exact matching is impossible, methodologists have offered an alternative procedure, known as “propensity score matching.” This approach suggests a different definition of similarity than the previous two. Rather than focusing on sharing scores on the matching variables, propensity-score matching focuses on sharing a similar estimated probability of having been in the treatment group, conditional on the matching variables. In other words, when looking for a match for a specific case in the treatment group, researchers look for cases in the control group that—before the score on the independent variable was known—would have been as likely to be in the treatment group as the other case. This is accomplished by a two-stage analysis, the first stage of which approaches the key independent variable, X_1 (understood as the “treatment”), as a *dependent* variable and the *matching* variables as *independent* variables. Once this model has been estimated, sometimes using nonparametric regression techniques that replace the assumption of linearity with a looser assumption of smoothness, any resulting coefficient estimates are disregarded. Instead, the second stage of the analysis employs the fitted values for each case, which tell us the probability of that case being assigned to the treatment group, conditional on its scores on the matching variables. These fitted values are referred to as propensity scores. The final step in the

¹⁹ However, matching is clearly inferior to a well-designed and well-executed randomized experiment. The benefits of matching extend only so far as equivalence on the variables explicitly included and any unmeasured variables that fortuitously happen to be similar across the cases. By contrast, proper randomization handles all unmeasured variables.

process is to choose matches for each case in the treatment group. This is accomplished by selecting cases from the control group with similar propensity scores. Using the propensity score – which is a function of all of the matching variables – reduces the problem of trading off matches on multiple dimensions to a unidimensional decision task. This is helpful, indeed necessary, because the final case-selection decision is unidimensional: cases are either included or excluded. The propensity-score approach to matching provides a systematic approach to reducing the dimensionality of the decision problem – a process often implemented in a much less systematic way when researchers choose matching cases without large-N techniques. The end result of this propensity-score procedure is a set of matched-cases that can be compared in whatever way the researcher deems appropriate. These are the “most-similar” cases, returning to the qualitative terminology. Rosenbaum and Silber (2001: 223) summarize:

Unlike model-based adjustments, where patients vanish and are replaced by the coefficients of a model, in matching, ostensibly comparable patterns are compared directly, one by one. Modern matching methods involve statistical modeling and combinatorial algorithms, but the end result is a collection of pairs or sets of people who look comparable, at least on average. In matching, people retain their integrity as people, so they can be examined and their stories can be told individually.

Matching, conclude the authors, “facilitates, rather than inhibits, thick description” (Ibid.). The same matching techniques that have been used successfully in observational studies of medical treatments could also be adopted to the study of nation-states, political parties, cities, or indeed any traditional paired cases in the social sciences.

Suppose that, in order to study the relationship between wealth and democracy, the researcher wishes to select cases that are as similar as possible to India and Costa Rica in background variables—while being as different as possible on per capita GDP.

In order to select most-similar cases for the study of the relationship between wealth and democracy, we will need a statistical model of the causes of a country’s wealth. Obviously, such a proposition is complex; since this is simply an illustrative example, we will be satisfied with a cartoon model that only includes two independent variables. Specifically, a country’s wealth will be assumed to be a function of the origin of its legal system (including the British legal heritage variable discussed in the previous example, as well as dummy variables for French legal heritage, socialist legal heritage, German legal heritage, and Scandinavian legal heritage), and a variable measuring the latitude of the country’s capital.

The first step in selecting most-similar cases is to run a nonparametric regression with these two variables as the independent variables and logged per capita GDP (the independent variable of theoretical interest) as the dependent variable. The fitted values from this regression serve as propensity scores, and cases with similar propensity scores are interpreted as matching. It is important to keep in mind that the quality of the match depends on the quality of the statistical model used to generate the propensity scores; a superficial model, like the one used here, obviously produces superficial matches.

The propensity scores for our two focus cases are: Costa Rica, 7.63, and India, 8.02. Examining the propensity score data, we see that Benin has a propensity score of 7.58—quite similar to Costa Rica’s—and a per capita GDP of \$1163, which is substantially different from Costa Rica’s \$5486. Hence, Benin and Costa Rica may be seen as most-similar cases for testing the relationship between wealth and democracy. Similarly, Singapore’s propensity score of 7.99 is a very nearly exact match for India’s, in spite of a noticeable difference between Singapore’s per capita GDP of \$27,020 and India’s \$2066. These two pairs of cases thus meet the criteria for most-similar-case comparison.

Conclusion. The most-similar method is one of the oldest recognized techniques of qualitative analysis, harking back to J.S. Mill’s classic study, *System of Logic* (first published in 1834). By contrast, “matching” statistics are a relatively new technique in the arsenal of the social sciences, and have rarely been employed for the purpose of selecting cases for in-depth analysis. Yet, we believe that there may be a fruitful interchange between the two approaches. Indeed, the current popularity of matching among statisticians – relative, that is, to garden-variety regression models – rests upon what qualitative researchers would recognize as a “case-based” approach to causal analysis. Hence, we think it perfectly reasonable to appropriate this large-N method of analysis for case study purposes.

The most-different method of case selection is the reverse-image of the previous research design. Here, variation on independent variables is prized, while variation on the outcome is eschewed. Rather than looking for cases that are most-similar, one looks for cases that are most-different. Specifically, the researcher tries to identify cases where just one independent variable, as well as the dependent variable, co-vary, and all other plausible independent variables show different values. These are deemed “most different” cases, though they are similar in two essential respects -- the causal variable of interest (X_i) and the outcome (Y).²⁰ Analysts have usually taken the position that this research design is a weaker tool for causal inference than the most-similar method (Gerring 2007).

COMPLICATIONS

The seven-part typology summarized in Table 1 is intended to provide a menu of options for researchers seeking to identify useful cases for in-depth research, a means of implementing these options in large-N settings, and useful advice for how to maximize variation on key dimensions while maintaining claims to case-representativeness within a broader population. In this final section we summarize and extend our comments with respect to several complications that may arise in the course of implementing these procedures.

Some case studies follow only one strategy of case selection. They may be neatly classified as *typical*, *diverse*, *extreme*, *deviant*, *influential*, *most-similar*, or *most-different*, as discussed. However, it is important to recognize that many case studies also mix and match among these case selection strategies. We have noted that insofar as all case studies seek representative samples, they are always in search of “typical” cases. Thus, it is common for writers to declare that their case is both extreme and typical; it has an extreme value on the X or Y of interest but is not, in other respects, idiosyncratic. There is not much that we can say about these combinations of strategies except that, where the cases allow for a variety of empirical strategies, there is no reason not to pursue them. And where the same cases can serve several functions at once (without further effort on the researcher’s part), there is little cost to a multi-pronged approach to case analysis.

The second complication that deserves emphasis is the changing status of a case during the course of a researcher’s investigation – which may last for years, if not decades. Often, a researcher

²⁰ Traditionally, outcomes are understood as “positive” outcomes; things that happened. However, as we have argued above, they could just as well be composed of negative outcomes; things that did not occur. Indeed, outcomes that are continuous in nature such as welfare spending or growth are not readily understandable in this positive/negative fashion. The most-different method is also sometimes referred to as the “method of agreement,” following its inventor, J.S. Mill (1843/1872). See also DeFelice (1986), Gerring (2001: 212-4), Lijphart (1971, 1975), Meckstroth (1975), Przeworski and Teune (1970), Skocpol and Somers (1980).

begins in an exploratory mode and proceeds to a confirmatory mode -- that is, she develops a specific X/Y hypothesis. The goal of research, after all, is discovery, not simply the verification or falsification of existing hypotheses.

Unfortunately, research strategies that are ideal for exploration are not always ideal for confirmation. The extreme-case method, for example, is inherently exploratory; the researcher is concerned merely to explore variation on a single dimension (X or Y). Once a specific hypothesis is adopted, the researcher must shift to a different research design. This transformation of research designs is quite common to case study research. One cannot construct the perfect research design until a) one has a specific hypothesis and b) one is reasonably certain about what one is going to find out there in the empirical world. In short, the perfect case study research design is often apparent to the researcher only *ex post facto*.

There are three ways to handle this. One can explain, straightforwardly, that the initial research was undertaken in an exploratory fashion, and therefore not constructed to test the specific hypothesis that is -- now -- the primary argument. Alternatively, one can try to re-design the study after the new (or revised) hypothesis has been formulated. This may require additional field research or perhaps the integration of additional cases or variables that can be obtained through secondary sources or through consultation of experts. A final approach is to simply jettison, or deemphasize, the portion of research that no longer addresses the (revised) key hypothesis. A three-case study may become a two-case study, and so forth. The lost time and effort are the only costs of this down-sizing. In the event, practical considerations will probably determine which of these three strategies, or combinations of strategies, is to be followed. (They are not mutually exclusive.) The point to remember, and our point of conclusion in this article, is that revision of one's cross-case research design is *normal* and perhaps to be expected. Not all twists and turns on the meandering trail of truth can be reasonably anticipated. Where one starts is not always where one ends up.

A final complication, which we have noted in each section of the paper, is that of representativeness. There are only two situations in which a case study researcher need not be concerned with the representativeness of her chosen case. The first is the influential case research design, where a case is chosen because of its possible influence on a cross-case model, and hence is not expected to be representative of a larger sample. The second is the deviant case method, where the chosen case is employed to confirm a broader cross-case argument to which the case stands as an apparent exception. Here, the researcher's task may be simply to show that deviant-case A is poorly explained by theory B because of idiosyncratic feature C. In this instance the case is also not intended to represent a broader class of cases.

In all other circumstances, cases must be representative of the population of interest in whatever ways might be relevant to the proposition in question. Note that where a researcher is attempting to disconfirm a deterministic proposition the question of representativeness is perhaps more appropriately understood as a question of classification: is the chosen case appropriately classified as a member of the designated population? If so, then it is fodder for a disconfirming case study.

If the researcher is attempting to confirm a deterministic proposition, or to make probabilistic arguments about a causal relationship, then the problem of representativeness is of the more usual sort: is Case A unit-homogenous relative to other cases in the population? This is not an easy matter to test. However, in a large-N context the residual for that case (in whatever model the researcher has greatest confidence in) is a reasonable place to start. Of course, this test is only as good as the model at hand. Any incorrect specifications or incorrect modeling procedures will likely bias the results and give an incorrect assessment of each case's "typicality." In addition, there is the possibility of stochastic error, errors that cannot be modeled in a general framework. Given the explanatory weight that individual cases are asked to bear in a case study analysis, it is wise to

consider more than just the residual test of representativeness. Deductive logic – expectations about the causal relationships of interest and the case of choice – are sometimes more useful than purely inductive exercises, whether qualitative or quantitative.

In any case, there is no dispensing with the question. Case studies (with the two exceptions already noted) rest upon an assumed synecdoche: the case should stand for a population. If this is not true, or if there is reason to doubt this assumption, then the utility of the case study is brought severely into question.

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