Case-Selection:
A Diversity of Methods and Criteria

John Gerring
(jgerring@bu.edu)

Lee Cojocaru
(Cojocaru@bu.edu)

Department of Political Science
Boston University
232 Bay State Rd
Boston, MA 02215

Draft: 6 January 2015
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Case-selection plays a pivotal role in case study research. This is widely acknowledged, and is implicit in the practice of describing case studies by their method of selection – typical, deviant, crucial, and so forth. It is also evident in the centrality of case-selection in methodological work on the case study, as witnessed by this symposium. By contrast, in large-N cross-case research one would never describe a study solely by its method of sampling. Likewise, sampling occupies a specialized methodological niche within the literature and is not front-and-center in current methodological debates. The reasons for this contrast are revealing and provide a fitting entrée to our subject.

First, there is relatively little variation in methods of sample construction for cross-case research. Most samples are randomly sampled from a known population or are convenience samples, employing all the data on the subject that is available. By contrast, there are myriad approaches to case-selection in case study research, and they are quite disparate, offering many opportunities for researcher bias in the selection of cases (“cherry-picking”).

Second, there is little methodological debate about the proper way to construct a sample in cross-case research. Random sampling is the gold standard and departures from this standard are recognized as inferior. By contrast, in case study research there is no consensus about how best to choose a case, or a small set of cases, for intensive study.

Third, the construction of a sample and the analysis of that sample are clearly delineated, sequential tasks in cross-case research. By contrast, in case study research they blend into one another. Choosing a case often implies a method of analysis, and the method of analysis may drive the selection of cases.

Fourth, because cross-case research encompasses a large sample – drawn randomly or incorporating as much evidence as is available – its findings are less likely to be driven by the composition of the sample. By contrast, in case study research the choice of a case will very likely determine the substantive findings of the case study.

Fifth, because cross-case research encompasses a large sample claims to external validity are fairly easy to evaluate, even if the sample is not drawn randomly from a well-defined population. By contrast, in case study research it is often difficult to say what a chosen case is a case of – referred to as a problem of “casing.”

Finally, taking its cue from experimental research, methodological discussion of cross-case research tends to focus on issues of internal validity, rendering the problem of case-selection less relevant. Researchers want to know whether a study is true for the studied sample. By contrast, methodological discussion of case study research tends to focus on issues of external validity. This could be a product of the difficulty of assessing case study evidence, which tends to demand a great deal of highly specialized subject expertise and usually does not draw on formal methods of analysis that would be easy for an outsider to assess. In any case, the effect is to further accentuate the role of case-selection. Rather than asking whether the case is correctly analyzed readers want to know whether the results are generalizable, and this leads back to the question of case-selection.

Given the central role of case-selection in case study research it is essential that we come to terms with options that are currently in use, or might be put to use. To this end, a number of case-selection typologies have been proposed over the years. Mill (1843/1872) proposed the method of difference (aka most-similar method) and method of agreement (aka most-different method), along with several others that have not gained traction. Lijphart (1971: 691) proposes six case study types: a-theoretical, interpretative, hypothesis-generating, theory-confirming, theory-infirming, and deviant. Eckstein (1975) identifies five species: configurative-idiographic, disciplined-configurative, heuristic, plausibility probes, and crucial-case. Skocpol & Somers (1980) identify three logics of comparative history: macro-causal analysis, parallel demonstration of theory, and contrast of contexts. Gerring (2007) and Seawright & Gerring (2008) identify nine techniques: typical, diverse, extreme, deviant, influential, crucial, pathway, most-similar, and most-different. Levy (2008) identifies five case study
research designs: comparable, most and least likely, deviant, and process tracing. Rohlfing (2012: ch.3) identifies five case-types – typical, diverse, most-likely, least-likely, and deviant – which are applied differently according to the purpose of the case study. Blatter & Haverland (2012: 24-26) identify three explanatory approaches – covariational, process tracing, and congruence analysis – each of which offers a variety of case-selection strategies.

This paper builds directly on these studies, incorporating their labels and their insights, wherever possible, in an effort to provide a clearer and more comprehensive typology. Note that some extant typologies are not explicit about rules for case-selection and may focus more on the goals of the case study than on the task of case-selection. Others are incomplete. In particular, they are insufficiently disaggregated, conflating case-selection techniques that are, in fact, quite disparate. This means that they fail to capture the diversity of methodological criteria that rightly apply to case-selection. Finally, extant studies that promote cross-case formulas for selecting cases have not fully appreciated the costs of “ex ante” case-selection.

The first section of this study presents an extended typology of case-selection methods. It identifies five main case study types and eighteen sub-types – a larger set of options than is recognized by any extant typology. The second section reviews prominent examples of case study work, coded into the categories of the typology. An initial canvas focuses on most-cited studies in social science, as revealed by Web of Science. A second canvas focuses on key exemplars, studies that have been identified in methodological work on the case study or which exemplify varying subjects or techniques of analysis. This exercise serves several purposes. It tests the capacity of the typology to categorize extant work; it fleshes out the meaning and use of each case study type; and it provides an identifiable pool of prominent case studies that help to delimit our topic (an “extensional” approach to definition). Finally, we review the protocol for ex ante case-selection and the benefits and costs of this approach. These issues have been discussed in the literature but not thoroughly vetted.

I. A Case-selection Typology

By way of definition, we stipulate that a case study (a) focuses on one or several cases that are explored in depth, (b) integrates diverse styles of (observational) evidence, and (c) potentially sheds light on a broader population, which it represents in an imperfect manner. The case(s) under study always provides more than one observation, which may be constructed diachronically (by observing the case or some subset of within-case units through time) and/or synchronically (by observing within-case variation at a single point in time).

Sometimes, the distinction between cases and within-case observations is ambiguous. For example, Daniel Posner’s study of the politicization of ethnic identity in southern Africa includes a modest-sized survey (N=180) of individuals – members of the Chewa and Tumbukka ethnic groups in Malawi and Zambia.¹ This provides fodder for a regression analysis. However, the theoretical question of interest (politicization) and the treatment (relative group size) are both at the group level. Accordingly, individuals within each group share many characteristics (clustering); they affect each other (violating the condition of non-interference); and they are liable to a similar set of confounders. The research design is thus similar to a clustered experiment with only two clusters. For all these reasons, we classify Posner’s study and others like it as case studies.²

² E.g., Childs (2005), Miguel (2005), Mondak (1995).
Empirical studies that do not satisfy the definitional attributes of a case study are referred to as cross-case, implying that a large number of cases are examined in a more schematic fashion – either in narrative format (e.g., McNeill 1963) or in a formal model based on set theory (e.g., Qualitative Comparative Analysis) or statistics.

With this definitional ground beneath us we can proceed to our topic. Case study research begins with a selection of cases to be subjected to intensive analysis, i.e., the construction of a sample. The most important features of this process may be represented in a tabular format, as shown in Table 1. This allows us to identify case study types \( (N=5) \) and sub-types \( (N=17) \), as listed in the first column.

In considering the criteria that apply to case-selection we begin with a few criteria that virtually all case studies aim to satisfy.

Any case chosen for in-depth analysis must afford enough data to address the question of interest, as indicated in column 2. If sources are unreliable, scarce, or for one reason or another inaccessible, the case is of little value. Note that the purpose of a case study is to extend our knowledge beyond what it is possible to explore in a large sample. One important way that a case extends our knowledge is by providing information on things that we cannot measure, or cannot easily assemble, across a large number of units. This includes within-case evidence. So, the availability of evidence for the chosen case(s) – and the researcher’s access to that evidence – must be a key criterion of case-selection. Of course, this is usually not the only reason for choosing a case and is rightly subordinate to other features. That is why data availability is an over-arching criterion, not a case study type. (Researchers never justify their selection of a case solely because of the availability of data.)

If the case study is designed to shed light on a causal question the chosen cases must be independent of each other and of other cases in the population, as indicated in column 3. If cases affect each other they are not providing independent evidence of the proposition. (The exception would be a situation where interaction across cases happens to be the subject of investigation, as it would in a study of diffusion.) If the question of interest is causal this problem may be understood as one of interference, where the treatment status of one case affects outcomes for the other case(s). (It may also be referred to as Galton’s problem or a violation of SUTVA.)

Although case studies are focused on one or several cases they usually aim to represent features of a larger population. The representativeness of the cases is therefore a fairly ubiquitous concern. Some case studies are chosen primarily because of their presumed representativeness (representative cases). Others are expected to possess the quality of representativeness along with other features that more clearly define the case-selection method, as indicated in column 4.

Some case-study techniques may be practiced with one or several cases \( (N=1+) \). Others require at least two cases \( (N=2+) \), as indicated in column 5. All may be extended to include a larger number of cases with the proviso that the more cases are included the less intensively each one can be analyzed – and thus the less the resulting study qualifies as a “case study.”

Case-selection procedures often hinge on the status of key factors, which we refer to as variables. Descriptive case studies must be concerned with whatever descriptive features of a phenomenon are relevant to the analysis, denoted \( D \). Causal/explanatory case studies may be concerned with the status of a case vis-à-vis \( Y \) (the outcome), \( X \) (the causal factor of interest), \( X \) (the vector of causal factors that are thought to cause \( Y \)), or \( Z \) (a vector of additional factors that affect \( Y \) and may also affect \( X \)). Selection on one or more of these factors is indicated in columns 6-9 of Table 1.

Desiderata specific to each case-study type (and sub-type) are summarized in column 10. Wherever possible, we summarize these desiderata with a formula – which may be as simple as a
For simplicity, we assume interval variables and linear relationships. Usually, these features are generalizable to binary variables and non-linear relationships.

Most of these case-selection techniques can be implemented *ex ante*, as indicated in column 11. This means that a researcher can choose cases – from a larger sample of potential cases – using one of the formulas laid out in column 10 (or analogous formula adapted for non-scalar variables and non-linear relationships) and thus without fore-knowledge of the identity of the chosen case(s). Of course, just because it is possible does not mean that case study researchers have done so in the past, or should do so in the future (see Section III).

The final section of Table 1 depicts research goals that correspond to each case-selection method. (Naturally, additional goals may also be embraced but they are less plausible in light of the case-selection procedure.) A case study may attempt to ascertain the *scope-conditions*, i.e., the population of an inference, which may also be considered a matter of causal heterogeneity – denoted Scope. A case study may attempt to *describe* a phenomenon – denoted D. Causal claims may be of various sorts. A case study may attempt to *identify* a novel causal factor (understood as a possible cause of Y) – denoted X. A case study may try to *eliminate* a causal factor (if X is regarded as a necessary condition of Y) or reduce confidence in a causal factor (if X is regarded as a probabilistic cause of Y) – denoted $X \rightarrow Y$. A case study may attempt to measure a specific causal effect (an effects-of-causes style of analysis) – denoted $X \rightarrow Y$. A case study may attempt to identify, and if possible estimate, *all causes* of an outcome (a causes-of-effects style of analysis) – denoted $X \rightarrow Y$. A case study may attempt to identify, and if possible measure, causal mechanisms (mediators) leading from X to Y – denoted $M$. A case study, finally, may help verify a causal effect estimated by a cross-case model – denoted $aX$ (where $a$ is the estimated coefficient for X).

Putting these features together, we can now briefly describe the various types and sub-types contained in Table 1.

**Representative**

Representative cases are intended to represent a broader population of cases in some relevant respect, which may be descriptive or causal.

*Random* cases are drawn from a population randomly, such that each case is equally likely to be selected. Cases chosen in this manner may be used to pursue any goal (it is not clear what goal they are most likely to achieve). For example, as part of a wide-ranging study of civil war, James Fearon and David Laitin choose a country (Japan) randomly from the universe of sovereign nation-states to test their theory about the causes of civil war.4

*Typical* cases are intended to represent descriptive features of a broader set of cases. For this purpose, descriptive statistics measuring the central tendency of a case – the mean, mode, or median – may be employed. The goal of such a study is likely to be descriptive. For example, Robert and Helen Lynd’s study of “Middletown” focuses on a city (which we now know to be Muncie, Indiana) that is intended to represent cities across the United States.5

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3 Naturally, case selection based on such a model depends upon the veracity of these assumptions. Model-based techniques of case-selection are model-dependent, a caveat that researchers must take very seriously. Problems of causal models using observational data and regression analysis are discussed in Freedman (1991), Kittel (2005), Seawright (2010).
5 Lynd & Lynd (1929).
Conforming cases are those that conform to expectations according to a causal model. In a regression model where Y is regressed on X the chosen case(s) would have low residuals, i.e., their actual values would conform closely to the values predicted by the regression model. The goal of a conforming case study is usually to explore causal mechanisms. For example, to investigate the relationship between globalization and welfare state retrenchment Duane Swank conducts a panel analysis of fifteen OECD countries. He then selects seven countries that exemplify the general patterns found in the crossnational analysis for intensive analysis and also incorporate variation in the key variables of interest. His case-selection method thus combines the diverse-case method and the conforming-case method.

Diverse cases – two or more – are intended to maximize variance across a dimension(s) of interest – D, Y, X, X, X/Y, or X/Y (Seawright & Gerring 2008). Thus, one might choose one case that scores very high on X and another that scores very low on X. They are “diverse” with respect to X. Or one might choose cases that result from the intersection of X₁ and X₂. In addition to exemplifying diversity the chosen cases are expected to be representative of a sub-set of the population – in this instance, cases that score low or high on X, respectively. Thus, among cases that provide variance across the desired dimension(s), one would choose a case that is also regarded as representative, following the criteria associated with random, typical, or conforming cases. In effect, a diverse-case approach to case-selection is the iteration of other representative-case procedures among different sub-populations. It may be oriented toward a descriptive typology, the establishment of scope-conditions, or an account of all the causes or all the mechanisms contributing to Y. As an example, let us consider Barrington Moore’s comparative-historical study of modernization. Moore’s cases are chosen to exemplify distinct routes to the modern world. From a common starting point (agrarian bureaucracy), each route is defined by a unique combination of three factors – bourgeois impulse (weak/strong), mode of commercial agriculture (market/labor-repressive), and peasant revolutionary potential (low/high) – along with a critical political event (bourgeois revolution, revolution from above, or peasant revolution). This produces an enduring regime outcome: democratic capitalism, fascism, or communism. Cases are chosen to represent each route. They are diverse, therefore, with respect to X → Y.

Census cases – two or more – include all cases that fall within the population, or a well-defined subset of the population of theoretical interest. A comprehensive approach to case-selection is possible only when the population, or the specified subset of the population, is small or moderate in size. (Otherwise, the census becomes too large to handle in a case study format.) The resulting case study may focus on a causal effect, multiple causes of Y, and/or causal mechanisms. For example, Robert Putnam’s study of social capital in Italy includes all regions of Italy (N=20). The regions obviously share a good deal in common and in this respect might be regarded as most-similar cases. However, regions are not chosen on the basis of their scores on X and Y. They are chosen by virtue of being part of a single country – which, depending on how one looks at it, might be regarded as a background condition (Z). Of course, another level of inference – from Italy to other countries – is required if the study is to satisfy demands for external validity beyond the

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6 Sometimes, this is referred to as a typical case (Lieberman 2005; Seawright & Gerring 2008). However, we need a way to distinguish selection by central tendency and selection by residuals in a causal model. Since the former is much more common (see Table 2), it made sense to use the established term (typical) for that and the newer term (conforming) for the latter.


9 Note that Moore’s work foreshadows Qualitative Comparative Analysis, developed several decades later (Schneider & Wagemann 2012).
immediate population of interest. (Like most case studies, Putnam’s work traverses several levels of analysis.)

Anomalous

Anomalous cases are those that do not conform to common understandings of a subject and therefore present an anomaly, which sets the agenda for a case study.

Idiographic cases are selected because of the intrinsic or theoretic importance of a case, and usually because they are not (in the author’s view) well understood (Eckstein 1975; Gerring 2007: epilogue; Levy 2008; Lijphart 1971). Because the importance of the case is often deemed self-evident, authors characteristically do not spend a great deal of time justifying (or clarifying) their case-selection. Although such studies may shed light on broader phenomena the cases are not chosen with an eye to representativeness. Resulting case studies may aim for descriptive or causal inferences. If the latter, the researcher is likely to try to identify all the causes of Y. It is worth noting that idiographic case studies sometimes take rather idiosyncratic forms, focusing on aspects of a case that are unlikely to be generalizable.10 Token causes (aka actual causes or causes-in-fact) are a common ingredient. Other idiographic cases are keen to build general theories that are travel-worthy, and thus avoid elements that might be viewed as idiosyncratic. For example, Douglass North & Barry Weingast examine the development of limited government in England through the seventeenth century, in the wake of the Glorious Revolution.11 This case is chosen (it would appear) because of its central importance in the literature on the topic, and because it is (for a host of additional reasons) a prominent and much-studied case. The authors endeavor to demonstrate that changes in political institutions paved the way for subsequent economic development by committing the government to protect property rights.

Outcome cases (aka extreme cases) select on the dependent variable (Y), which exhibits extreme or rare values or extreme/rare changes over time (Gerring 2007; Seawright & Gerring 2008). Its purpose is generally to identify the cause, or causes, of that outcome. For example, John Caldwell’s influential study of mortality is structured around three polities with extremely low mortality in the present era – Costa Rica, Sri Lanka, and Kerala (a state in India).12 On the basis of these cases – contrasted schematically with other developing countries – Caldwell concludes that female autonomy and local health care are critical ingredients in human development, ingredients that could be replicated elsewhere.

Deviant cases deviate from an expected causal pattern and thus invoke the opposite selection criterion of the conforming case (Eckstein 1975; Gerring 2007; Lieberman 2005; Lijphart 1975; Seawright & Gerring 2008). Given Z (a set of background conditions), the outcome (Y) of a case is un-expected. In a regression model, a deviant case thus exhibits a high residual. The purpose of a deviant case study is to explain the deviant case – and, in so doing, to explain other cases, providing a generalizable hypothesis about Y. Alternatively, the case study may help to establish scope-conditions for the theory, showing why the deviant case does not exhibit the expected relationship between Z and Y. An early example is provided by Werner Sombart’s study, Why is There No Socialism in the United States, often regarded as the first entry in a long line of studies focused on American exceptionalism.13 The US seemed deviant to Sombart in 1906 because, despite the advanced development of capitalism, it failed to exhibit the expected rise of a (sizeable) socialist party. In explaining this non-event, Sombart called attention to the consciousness of American workers,

12 Caldwell (1986).
which seemed uniquely favorable to capitalism and to the American system of government. In the process, he opened the way for an approach to politics resting on what we would now call political culture or ideology.

**Influential** cases are those that have a large impact on an estimated causal relationship from a cross-case model (Seawright & Gerring 2008). The purpose of such a case study is to help confirm – or possibly disconfirm – the putative relationship between X and Y. This may be accomplished by verifying that values for X, Y, and Z are properly measured, that the influential cases are properly assigned to the population, and/or to examine expected causal mechanisms. We have been unable to identify any examples of this style of case study.

**Most-different**

Most-different cases (aka Method of Agreement) vary widely in background factors regarded as potential causes (Z), while sharing a common outcome (Y) (DeFelice 1986; Lijphart 1971, 1975; Meckstroth 1975; Mill 1843/1872; Przeworski & Teune 1970; Skocpol & Somers 1980). If one is willing to assume that Y has only a single cause, all factors that vary across the chosen cases can be eliminated (as causes in the observed cases). Likewise, any remaining factors may be regarded as the cause (singly or in combination) of the outcome in the observed cases.

The *exploratory* version of this design minimizes variation in Y and maximizes variation in Z across the chosen cases. Its purpose is to eliminate necessary conditions of Y, to cast doubt on probabilistic causes of Y, to determine scope-conditions, or – most probably – to discover a new cause, X. For example, Marc Belich explores seven examples of settler-driven expansion during the nineteenth and early twentieth centuries – the United States, Canada, Australia, New Zealand, South Africa, Argentina, and Siberia. Despite their varying histories, each episode is found to observe a similar three-step cycle: “an explosive population boom marked by net imports of goods and capital,…a dramatic ‘bust’ decimating growth rates and bankrupting farms and businesses, and finally an export rescue creating a new economy based on mass export of staples to a distant metropolis.”

The common element, and hence the suspected causal factor, is the population and economic dynamics shared across these diverse cases.

The *pathway* version of this design minimizes variation in Y, maximizes variation in Z, and minimizes variation in X across the chosen cases. Since X is known prior to the case study, the role of the pathway case is to explore causal mechanisms (M) or to verify other aspects of the chosen cases to assure that they fit theoretical expectations. For example, Marc Howard explores the negative and enduring impact of communism on civil society by focusing on two former members of the Soviet bloc, Russia and East Germany. Howard argues that while these countries have very low civic engagement in the present era, they are different in all respects except their communist experience. This is regarded as the putative cause, an argument bolstered by a variety of within-case evidence.

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14 Belich (2012).
Crucial

A crucial case, in my rendering, exemplifies a natural experiment within a single case. This case is usually observed over time, and thus mimics a one-group experiment, where $X$ changes in an as-if random fashion while $Z$ remains constant and $Y$ is observed before and after the intervention.\textsuperscript{17}

A crucial-testing case exhibits great variation in the causal factor of interest ($X$) and minimal variation in background characteristics ($Z$). The purpose of such a study is to measure the impact of $X$ on $Y$, or to observe causal mechanisms. For example, in order to understand the interrelationship between monetary policy ($X$) and economic fluctuations ($Y$), Milton Friedman and Anna Schwartz look closely at US history, locating four occasions in which the stock of money changed due to policy choices largely unrelated to the behavior of the economy (and hence exogenous to the research question).\textsuperscript{18} These four interventions consisted of “the increase in the discount rate in the first half of 1920, the increase in the discount rate in October 1931, the increase in reserve requirements in 1936–1937, and the failure of the Federal Reserve to stem the tide of falling money in 1929–1931.”\textsuperscript{19} It turns out that each was followed by a substantial change in the behavior of the stock of money, validating a central pillar of monetarist theory.

A least-likely case is employed to demonstrate a hypothesis by choosing a case that seems least likely to exhibit the hypothesized relationship between $X$ and $Y$, based on background characteristics ($Z$), but in the event does so (Eckstein 1975). Thus, selection is on $X$, $Z$, and $Y$. An example is provided by Robert Michels’ study of the “iron law of oligarchy,” which he saw as the inevitable by-product of any enduring organization.\textsuperscript{20} To explore this thesis, Michels focuses on an organization, the German Social Democratic Party (SDP), that embodies an egalitarian social philosophy and thus seems unlikely to succumb to elite dominance. The fact that party leaders exercised considerable control over rank-and-file members thus offers strong corroboration of the theory.

A most-likely case is employed to disconfirm (or cast doubt on) a hypothesis by choosing a case that seems most likely to exhibit the hypothesized relationship between $X$ and $Y$, based on values for $X$ and background characteristics ($Z$), but in the event does not (Eckstein 1975). Selection, again, is on $X$, $Z$, and $Y$. As an example, we shall consider a well-established argument about democratization, which suggests that diverse countries are unlikely to establish and maintain a democratic form of government. A most-likely case is provided by Papua New Guinea, which exhibits a very high level of ethnolinguistic heterogeneity and a low level of modernization—a background factor also thought to discourage democratization. Despite these features, PNG has sustained a vibrant multiparty democracy since independence in 1975. In a detailed case study, Ben Reilly points out that this constitutes strong evidence against the pessimistic view of diversity.\textsuperscript{21}

A pathway case is one where causal mechanisms are mostly likely to be observed (Barnes & Weller 2014, 2015; Gerring 2007). Generally, this is a case where the apparent impact of $X$ on $Y$ is greatest, while background conditions ($Z$) are held constant or bias the analysis against the hypothesis. The case study might identify unknown mechanisms (exploratory) or search for those stipulated by a theory (in a confirmatory mode). An example of the latter sort is provided by Allan Dafoe & Nina Kelsey’s study of a (presumed) causal relationship between capital openness ($X$) and

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\textsuperscript{17} My interpretation of “crucial” is looser than that propounded by Eckstein (1975), but nonetheless similar in spirit and in research design.

\textsuperscript{18} Friedman & Schwartz (1963).

\textsuperscript{19} Miron (1994: 19).

\textsuperscript{20} Michels (1911).

reduced military disputes between states \((Y)\).\(^{22}\) According to the theory, a situation of capital openness and military disputes should be accompanied by mechanisms \((M)\) such as a devaluation of assets, pressure on exchange rates, sanctions, and costs imposed upon economic agents engaged in foreign trade. These mechanisms should serve, in turn, to mitigate the likelihood of conflict, and thus confirm the theory. Dafoe & Kelsey choose cases with high capital openness and escalating disputes because it is in this situation that the stipulated mechanisms are mostly likely to materialize. (In this somewhat unusual situation, a change in \(Y\) is thought to affect \(M\).) In the event, they find little evidence of the mechanisms, concluding that the theory may be faulty.

Most-similar

Most-similar cases are intended to provide quasi-experimental comparisons between a treatment case and a control case, while controlling background features. It may also be referred to as the Method of Difference, following J.S. Mill \((1843)\).\(^{23}\)

In the exploratory version of this design, the objective is to maximize variation in \(Y\), while minimizing variation in \(Z\), or assuring that background factors run counter to the pattern of variation in \(Y\) (so they cannot serve as confounders). The purpose of such a study is to identify potential causes of \(Y\). For example, in a medical study Paul Rosenbaum and Jeffrey Silber compare patients within a hospital who have similar symptoms \((Z)\), some of whom survive their spell of hospitalization \((Y=0)\) and others of whom die \((Y=1)\). The goal is to better ascertain the causes of death by probing differences across these closely matched cases using information gleaned from a close examination of Medicare records.\(^{24}\)

In the testing version of this design, variation in \(X\) is maximized while variation in \(Z\) is minimized, or runs counter to the hypothesis of theoretical interest.\(^{25}\) The goal here is to measure a causal effect and perhaps to ascertain the causal mechanisms at work. For example, Jeffrey Mondak examines two cities – Cleveland and Pittsburgh – that are similar in background conditions. One of the cities experiences a newspaper strike, exhibiting a change in the causal factor of theoretical interest. The goal of the study is to determine to what extent the absence of a newspaper affects citizen knowledge of politics, as measured through a post-test survey of political awareness.\(^{26}\)

In the pathway version of this design, the treatment case has the characteristics of a pathway case, as described above, while the control case exemplifies the control condition \((X=0)\) and background factors \((Z)\) that mirror those of the treatment case. Its purpose is examine causal mechanisms \((M)\) – either to identify what they might be (exploratory) or to search for those stipulated by a theory (confirmatory). For example, in order to shed light on the persistent relationship between socioeconomic status and health, Karen Lutfey and Jeremy Freese compare high and low status individuals who suffer from diabetes, with the knowledge that the latter are more likely to succumb to the effects of the disease. This is accomplished by focusing on two endocrinology clinics, one located in an affluent neighborhood and the other in a poor neighborhood. The focus of the study is on factors inside the clinic (continuity of care, in-clinic educational resources, bureaucratic organization), outside the clinic (financial limitations, occupational constraints, social support networks), and among the patients (motivation, cognitive

\(^{22}\) Dafoe & Kelsey \((2014)\).
\(^{23}\) For further discussion see Meckstroth \((1975)\), Nielsen \((2015)\), Sekhon \((2004)\).
\(^{24}\) Rosenbaum & Silber \((2001)\).
\(^{25}\) Also referred to as the “comparative method” strategy \((Lijphart 1975)\), discussed in Glynn & Ichino \((2015)\).
\(^{26}\) Mondak \((1995)\).
ability) that might affect compliance with an exacting medical regime.\textsuperscript{27} These are regarded as causal mechanisms in the relationship between SES and health.

\textsuperscript{27} Lutfey & Freese (2005).
### Table 1: Case-selection Typology

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<th>TYPES, SUB-TYPES</th>
<th>SELECTION CRITERIA</th>
<th>RESEARCH GOALS</th>
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<td>Scope D X → Y</td>
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<td>1. Representative</td>
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<td>a. Random</td>
<td>A case that exemplifies a larger population</td>
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<td>c. Deviant</td>
<td></td>
<td></td>
</tr>
<tr>
<td>d. Influential</td>
<td></td>
<td></td>
</tr>
<tr>
<td>3. Most-different</td>
<td></td>
<td></td>
</tr>
<tr>
<td>a. Exploratory</td>
<td></td>
<td></td>
</tr>
<tr>
<td>b. Pathway</td>
<td></td>
<td></td>
</tr>
<tr>
<td>4. Crucial</td>
<td></td>
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</tr>
<tr>
<td>a. Testing</td>
<td></td>
<td></td>
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<tr>
<td>b. Most-likely</td>
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<td>c. Least-likely</td>
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</tr>
<tr>
<td>d. Pathway</td>
<td></td>
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<tr>
<td>5. Most-similar</td>
<td></td>
<td></td>
</tr>
<tr>
<td>a. Exploratory</td>
<td></td>
<td></td>
</tr>
<tr>
<td>b. Testing</td>
<td></td>
<td></td>
</tr>
<tr>
<td>c. Pathway</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

D: a vector of descriptive (non-causal) features. Data: data availability. Ex ante: case-selection may occur prior to learning about details of the case (other than those identified in this table) perhaps with some form of stratified random sampling. H: causal hypothesis (H1: X → Y, H0: X → Y). Ind: case-independence. M: one or more pathways (variable or vector) from X to Y. Max: maximize. Min: minimize. N: number of cases. Rep: representativeness of chosen cases vis-à-vis a larger population. Scope: scope-conditions (population). Var: variance. X: causal factor of theoretical interest, assumed to be a single factor unless otherwise noted. X vector of all suspected causes of Y (including interactions). Y → Y, X is not, or is unlikely to be, a cause of Y. X → Y causal effect (effects-of-causes). Y: outcome of interest. aX the causal effect of X on Y in a cross-case model. Z vector of background factors that may affect Y. ?: possible selection criterion.
Caveats and Clarifications

Having laid out the essential features of the typology we turn to caveats and clarifications.

First, with the exception of idiographic cases, all case-selection procedures rest upon a case’s relationship to a larger population of cases. For example, the deviant case is that case(s) in the studied population (or the observed sample) that exhibits the most deviant features. In addition, some case-selection strategies require the researcher to consider the status of the chosen cases relative to each other. This applies to diverse, most-different, and most-similar research designs.

Second, features that define a case-selection method can usually be understood cross-sectionally or longitudinally (in a time-series). For example, the test of “deviance” might be a case’s status at a particular point in time, or its change of status over an observed period of time. Usually, the latter is more informative for purposes of in-depth case analysis. A case that happens to be deviant may have undergone a change at some point in the past, or perhaps its deviance is a product of changes in other cases. By contrast, if we can ascertain the point of inflection – the time at which a case becomes deviant – we can focus subsequent study of the case on this time-period. This is usually more informative. Thus, wherever possible, researchers should measure these case-selection variables through time rather than cross-sectionally. This point is often neglected, perhaps because techniques of case-selection are often set forth in a simple, cross-sectional format. However, readers should bear in mind that clues to causal relationships are virtually impossible to tease apart without longitudinal data. If such data is available it should be integrated into the case-selection process.

Third, this typology – along with others that have been or might be constructed – can never be entirely exhaustive. There are always additional case-selection procedures that, although rare, might be added to the list, or might be invented in the future. The categories in Table 1 should therefore be regarded as a work in progress. Likewise, one might construct typologies based on other features of the case study. Different organizing principles will produce different typologies and each may have its uses. This one is focused narrowly on the task of case-selection.

Fourth, categories in the typology are not strictly exclusive. Three case-selection criteria – data, independence, and representativeness – are shared by virtually all case studies and the general objectives – e.g., describing, identifying causal effects, identifying causal mechanisms, identifying scope-conditions – are likewise often shared.

Fifth, some case-selection strategies are purely exploratory. For example, the outcome-case is designed to construct an explanation for Y in situations where very little is known about the causes of Y. Other case-selection strategies may be carried out in an exploratory or a confirmatory mode. For example, the crucial-pathway case may be enlisted to identify an unknown mechanism, or it may be enlisted to test for the existence of a mechanism previously identified by a theory.

Sixth, case-selection strategies 1-2 specify a method of case-selection – representative or anomalous – but say very little about how the chosen case(s) should be analyzed. By contrast, strategies 3-5 offer a mode of case-selection and have a bit more to say about how the case(s) should be analyzed. This means that case-selection options 1-2 may morph into options 3-5 once a case is chosen.

This brings us to a final point. The status of a case may change during the course of a researcher’s investigation (which may last for years, if not decades). This is especially likely when a researcher begins in an exploratory mode and proceeds to hypothesis-testing or where the hypothesis of interest evolves. The problem is that the case chosen for the original purpose may not be the ideal case for the purpose that eventuates. Consequently, there is often slippage between the way a case study research design might be described at the beginning of a case study and how it is described at the end, during the write-up.
II. Surveying the Field

How common are the case study types identified in Table 1? We take two approaches to this question. Our first approach rests on Web of Science, a collection of periodicals that extends back to 1965 and includes over 300,000 articles published in journals across the social sciences. The latter is divided into four broadly defined disciplines: anthropology, including physical and cultural anthropology as well as archaeology (54 journals); economics, including business & management (214 journals); political science, including international affairs & public administration (202 journals); and sociology, including demography, cultural studies, gender studies, ethnic studies, & racial studies (286 journals).

To determine the most influential work we cull the 100 most-cited publications – articles or books – in each of these four disciplinary areas. Thus, in anthropology we identify the 100 most cited publications from the Web of Science pool, which includes 217,415 articles and books. In economics we identify the 100 most cited publications from the Web of Science pool, which includes 683,034 articles and books. In political science we identify the 100 most cited publications from the Web of Science pool, which includes 559,294 articles and books. And in sociology we identify the 100 most cited publications from the Web of Science pool, which includes 312,060 articles and books. Among these 400 highly-cited studies, 37 were identified as case studies or mixed-method studies.

In Table 2, these 37 studies are further classified into one of the five case study types – representative, anomalous, most-different, crucial, or most-similar. (We look for the best fit if multiple options seem plausible.) Strikingly, two-thirds of these case studies seem to have been chosen on the basis of their apparent representativeness. This is followed by anomalous and crucial case-selection techniques (tied at 16.2%). There are no instances of any other strategy of case-selection. Insofar as these most-cited case studies exemplify the broader population of case studies we are forced to conclude that case study researchers do not exploit the full palette of case-selection options.

Table 2: Survey of Case-Selection Strategies Among Most-cited Case Studies

<table>
<thead>
<tr>
<th></th>
<th>N</th>
<th>%</th>
</tr>
</thead>
<tbody>
<tr>
<td>Representative</td>
<td>25</td>
<td>67.5</td>
</tr>
<tr>
<td>Anomalous</td>
<td>6</td>
<td>16.2</td>
</tr>
<tr>
<td>Most-Different</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>Crucial</td>
<td>6</td>
<td>16.2</td>
</tr>
<tr>
<td>Most-Similar</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td><strong>TOTAL</strong></td>
<td>37</td>
<td>100</td>
</tr>
</tbody>
</table>

Most-cited case studies and mixed-method studies coded into case-selection typology (Table 1).

Of course, this is a small sample, and perhaps an unrepresentative one. It could be that the most-cited studies are different in certain respects relative to the larger population of extant case studies. In any case, our goal is not simply to represent the universe of extant case studies but also to
represent the full toolbox of case study methods, summarized in Table 1. For this purpose, we need to cast a broader net, and one that is devised to catch certain species of fish.

It is important to cast a wide net so that a variety of disciplines, subjects, and approaches can be represented. If “case study” means many things to different people we need to gather a large number of examples in order to capture that diversity and to provide examples that readers will be familiar with.

Table 3 includes 153 case studies, each of which is classified into the typology, including both case study types and sub-types. The survey begins (roughly) at the turn of the twentieth century, coincident with the professionalization of the core disciplines – anthropology, economics, history, political science, and sociology. We include both “pure” case studies and mixed-method studies (where a case study is combined with a cross-case analysis).

In selecting studies, we privilege those with demonstrable influence in a field or subfield (“classics”), those that have become touchstones in methodological discussion of the case study method, and those that provide diversity in topic, method of case-selection, method of analysis, theory, or disciplinary background. Chosen studies are referred to as exemplars.

Inclusion in Table 3 is not contingent on inferential goodness, so placement in this table should not be interpreted to mean that a study has strong claims to internal or external validity. These matters are difficult to evaluate and would require extensive discussion. In any case, the contribution of a case study is often exploratory (e.g., the elaboration of a new theory) rather than confirmatory (to test an extant hypothesis). As such, claims to validity may not be the most important feature.

To code each exemplar within the typology we try to determine which factors were most important in the author’s choice of cases. In making these judgments we rely on explicit statements by the author, wherever possible. Unfortunately, explication of case-selection procedures is often opaque. This is especially true for older studies, where methodological issues are generally not front-and-center. Additionally, researchers sometimes mean different things when they invoke case-study terms, and rarely do authors differentiate between versions of the same generic design. In classifying studies we are therefore forced to apply our own intuition about the method of case-selection that best accounts for the author’s choices.

Several features enhance ambiguity in these judgments. First, it is sometimes difficult to differentiate studies whose main purpose is descriptive from those whose main purpose is causal. This is because arguments are often rather loosely framed and may include a mixture of both elements, and also because many case study researchers adopt a rather diffuse vision of causality. Second, it is often difficult to tell which features of the cases were known to the author prior to case-selection. For example, one cannot say for sure when a researcher selected on X and when she selected on X and Y, unless she has clarified this issue in an explicit fashion. For this reason, classifications of case-study types (representative, anomalous, most-different, crucial, most-similar) are probably more accurate than classifications of case-study sub-types. Third, researchers sometimes combine different case study methods. To accommodate instances where several case-selection desiderata seem to exert equal force a final category – “Multiple” – is added to Table 3.

It follows that a degree of guesswork is involved in classifying case studies within the typology, and the fit between Tables 1 and 3 is not perfect. Even so, the chosen exemplars provide important reference-points, helping to establish our topic and paving the way for further discussion. What is it that we mean when we invoke various case study types and sub-types?

Readers will notice that the studies chosen for inclusion in Table 3 are not equally distributed across case-study types. Some categories are well-populated, and a few are entirely empty – signifying that they are hardy, if ever, enlisted (or we are not aware of their use). This may be a sign
that case study researchers have new tools to explore. Or it may be a sign that these strategies are not fruitful.

In column 2, we list the main author’s primary disciplinary field, categorized as anthropology (AN), demography (DE), economics, business, and management (EC), history (HI), linguistics (LI), public health (PH), political science (PS), psychology (PY), or sociology (SO). All disciplines are represented, though by no means equally.

In the column 3, we provide a crude signal of scholarly influence – the number of hits a study obtains in Google Scholar. Bear in mind that some studies are older than others, and thus benefit from a longer time-period over which to accrue citations. It is clear in any case that our assemblage includes some extremely influential studies, including several that have helped to establish new research paradigms.

Columns 4 and 5 note whether case-selection was carried out ex ante and/or ex post. Ex ante case-selection involves a formal procedure by which a case is chosen from a sample of possible cases based on formula specified in Table 1 (or something resembling those formula). Ex post refers to an informal (“qualitative”) procedure in which the researcher knows the values of the case on relevant parameters prior to his/her selection of the case. Among our exemplars, almost all were chosen in an ex post fashion. (This point is taken up in the concluding section of the paper.)

Column 6 lists the principal cases in the study, i.e., the units of analysis as defined by the main argument or theory. Readers will find a diversity of case-types, though a large number are comprised of nations (by which we mean nation-states or countries).

The final column records the number of cases (N) that are submitted to intensive analysis. If there are additional “shadow” cases that can be easily identified these are listed after the slash. (Typically, shadow cases enter the narrative in an ad hoc manner and are thus not easy to identify.) Most studies incorporate just one or several cases, though a few have several dozen. One medical study, implemented by a large team of researchers headed by Rosenbaum & Silber, integrates seventy-six cases. Our definition of “case study” regards the number of cases as a continuum, for which Rosenbaum and Silber’s study constitutes what might be regarded as an extreme upper bound.

Before concluding this summary review, we should remind ourselves that Table 3 includes studies that are exemplary in some fashion and is thus quite different from a sample of most-cited studies (shown in Table 2) or a sample of all case studies (assuming a suitable sampling frame could be identified). Even so, it provides a good sample for answering questions such as: What are we talking about when we use the term “case study”? and What do we mean when we invoke different case-study types and sub-types (as listed in Table 1)?

---

### Table 3: Exemplars

<table>
<thead>
<tr>
<th>Field</th>
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<th>Ex post</th>
<th>Cases</th>
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</tr>
</thead>
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<td>Fearon &amp; Laitin (2014) Civil War Non-Onsets</td>
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<td><strong>Typical (N=26)</strong></td>
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<td></td>
<td></td>
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<tr>
<td>Allen (1965) Nazi Seizure of Power: Experience of German Town</td>
<td>HI</td>
<td>397</td>
<td></td>
<td>Towns</td>
<td>1</td>
</tr>
<tr>
<td>Banfield (1958) Moral Basis of Backward Society</td>
<td>PS</td>
<td>3,882</td>
<td></td>
<td>Villages</td>
<td>1</td>
</tr>
<tr>
<td>Becker (1961) Boys in White</td>
<td>SO</td>
<td>3,142</td>
<td></td>
<td>Medical schools</td>
<td>1</td>
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<td>Benedict (1934) Patterns of Culture</td>
<td>AN</td>
<td>5,571</td>
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<td>Cultures</td>
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<tr>
<td>Geertz (1978) Bazaar Economy</td>
<td>AN</td>
<td>892</td>
<td></td>
<td>Communities</td>
<td>1</td>
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<tr>
<td>Gouldner (1954) Patterns of Industrial Bureaucracy</td>
<td>SO</td>
<td>3075</td>
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<td>Factories</td>
<td>1</td>
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<td>Handlin (1941) Boston's Immigrants</td>
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<td>488</td>
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<td>Cities</td>
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<td>Homans (1951) Human Group</td>
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<td>Groups</td>
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<td>Hunter (1953) Community Power Structure</td>
<td>SO</td>
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<td>Kanter (1977) Men and Women of the Corporation</td>
<td>SO</td>
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<td>Corporations</td>
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<td>Kaufman (1960) Forest Ranger</td>
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<td>1,073</td>
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<td>Lane (1962) Political Ideology</td>
<td>PS</td>
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<td>Workers</td>
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<td>Lerner (1958) Passing of Traditional Society</td>
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<td>Le Roy Ladurie (1978) Montaillou: Promised Land of Error</td>
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<td>Lewis (1959) Fire Families: Case Studies in Culture of Poverty</td>
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<td>Lynd &amp; Lynd (1929) Middletown: Study in American Culture</td>
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<td>Riker (1958) Paradox of Voting &amp; Congressional Rules</td>
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<td>Schattschneider (1935) Politics, Pressures and the Tariff</td>
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<td>Schmidt (1983) Interaction, Acculturation, Acquisition</td>
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<td>Selznick (1949) TVA and the Grass Roots</td>
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<td>Shaw (1930) The Jack Roller</td>
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<td>Taylor (1911) Principles of Scientific Management</td>
<td>EC</td>
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<td>Plants</td>
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<td>Warner &amp; Lunt (1941) Yankee City</td>
<td>AN</td>
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<td>Whyte (1943) Street Corner Society</td>
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<td>Swank (2002) Global Capital, Political Institutions &amp; Policy</td>
<td>PS</td>
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<td>Almond &amp; Verba (1963) Civic Culture</td>
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<td>Angell (1936) Family Encounters the Depression</td>
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<td>Bunce (1981) Do New Leaders Make a Difference</td>
<td>PS</td>
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<td>Chandler (1962) Strategy and Structure</td>
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<td>Collier &amp; Collier (1991) Shaping the Political Arena</td>
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<td>Fenno (1978) Home Style: House Members in their Districts</td>
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<td>Legislators/districts</td>
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<td>George &amp; Smoke (1974) Deterrence in US Foreign Policy</td>
<td>PS</td>
<td>894</td>
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<td>Crises</td>
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<td>Gourevitch (1986) Politics in Hard Times</td>
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<td>Nations in crises</td>
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<td>Kohli (2004) State-Directed Development</td>
<td>PS</td>
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<td>Levi (1988) Of Ruin and Revenue</td>
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<td>Rueschemeyer et al. (1992) Capitalist Development &amp; Demo.</td>
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<td>Wilson (1889) The State</td>
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<td>Ertman (1997) Birth of the Leviathan</td>
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<td>Hall (2011) Nature of Supreme Court Power</td>
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<td>Issue-areas</td>
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<td>Key (1949) Southern Politics in State and Nation</td>
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<td>3238</td>
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<td>Luebbert (1991) Liberalism, Fascism, or Social Democracy</td>
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### ANOMALOUS (N=39)

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### Idiographic (N=19)

- Alperovitz (1996) Decision to Use Atomic Bomb
- Beard (1913) Economic Interpretation of Constitution of the US
- Bennett et al. (1994) Burden-sharing in the Persian Gulf War
- Clark (2013) Shopkeepers How Exempts Went to War in 1914
- Coase (2000) Acquisition of Fisher Body by General Motors
- David (1985) Civic and the Economics of QWERTY
- Derthick (1979) Policymaking for Social Security
- Fogel (1992) Political Realignment of 1850
- Geertz (1979a) Deep Play: Notes on the Balinese Cockfight
- Hartz (1955) Liberal Tradition in America
- Kurzman (2005) Unthinkable Revolution in Iran
- McAdam (1982) Political Process & Black Insurgency
- North & Weingast (1989) Constitutions & Commitment
- Thompson (1963) Making of English Working Class
- Weber (1979) Peasants into Frenchmen

### Outcome (N=13)

- Caldwell (1986) Routes to Low Mortality in Poor Countries
- Curtiss (1977) Psycholinguistic Study of "Wild Child"
- Goldstone (1991) Revolution and Rebellion
- Greif (1998) Self-Enforcing Political Systems
- Harding et al. (2002) Study of Rampage School Shootings
- Johnson (1983) MTT and the Japanese Miracle
- Linz & Stepan (1978b) Breakdown of Democratic Regimes
- Peters & Waterman (1982) In Search of Excellence
- Porter (1990) Competitive Advantage of Nations
- Sagan (1993) Limits of Safety
- Skocpol (1979) States and Social Revolutions
- Tilly (1964) The Vendée

### Deviant (N=7)

- Aymard (1982) From Feudalism to Capitalism in Italy
- Liphart (1968) Politics of Accommodation
- Lipset et al. (1956) Union Democracy: Internal Politics of ITU
- Pearce (2002) Integrating Survey and Ethnographic Methods
- Sombart (1906) Economic Interpretation of Constitution of the US

### Influential (N=0)

### MOST-DIFFERENT (N=4)

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### Exploratory (N=2)

- Childs et al. (2005) Tibetan Fertility Transitions

### Pathway (N=2)

- Karl (1997) Paradox of Plenty

### CRUCIAL (N=21)

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### Testing (N=6)

- Friedman & Schwartz (1963) Monetary History of US
- Martin (1992) Coercive Cooperation
- Romer & Romer (2010) Macroeconomic Effects of Tax Changes
- Simmons (1994) Who Adjusts
- Tannenwald (1999) Nuclear Taboo
### Summary

#### Exploratory (N=14)
- Alston et al. (1996) *Determinants and Impact of Property Rights* (EC 390) - Brazilian states 2
- Cornell (2002) *Autonomy as a Source of Conflict* (PS 256) - Ethnic groups 9
- Dreze & Sen (1989) *India and India* (EC 2,936) - National Famine 2
- Epstein (1964) *A Comparative Study of Canadian Parties* (PS 113) - Party Systems 2
- Fiorina (1977) *Congress: Keystone of Washington Establishment* (PS 1,866) - House districts 2
- Geertz (1963) *Peddlers and Princes* (AN 977) - Towns 2
- Madrigal et al. (2011) *Community-Based Drinking Water Ours* (EC 20) - Water provision agencies 4
- Rosenbaum & Silber (2001) *Matching & Thick Description* (PH 50) - Patients 76

#### Testing (N=10)
- Abadie & Garcez (2003) *Costs of Conflict* (EC 867) - Spanish regions 1/17
- Immengut (1992) *Health Politics* (PS 1,008) - National Health Policy 3
- Kitchell (1986) *Political Opportunity Structures & Protest* (PS 1,760) - Social Movements 4
- Mondak (1995) *Newspapers and Political Awareness* (PS 95) - Cities 2
- Posner (2004) *Political Silence of Cultural Difference* (PS 376) - Nation/ethnic groups 4
- Skendja (2014) *International Insulation from Politics* (PS 0) - State bureaucracies 2
- Weinstein (2007) *Inside Rebellion* (PS 771) - Rebel groups 4

#### Pathway (N=7)
- Dunlevy (1994) *Politics and Industrialisation* (HI 125) - National Industrial Policy 2
- Haber (2010) *Politics, Banking, and Economic Development* (HI 3) - Nations 3
- Hebelre (1943) *Political Movements in Schleswig-Holstein* (HI 13) - Zones w/in a region 3
- Lutfey & Freese (2005) *NES & Health in Routine Clinic* (SO 144) - Clinics 2
- Wade (1997) *How Infrastructure Agencies Motivate Staff* (PS 71) - National irrigation agency 2

#### Multiple (N=3)
- Ostrom (1990) *Governing the Commons* (PS 20,073) - Common pool resources 14
- Pinjari (2012) *Peac Negotiations and Time* (PS 0) - Negotiations 4

#### Summary (N=153)

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19
III. Ex Ante Case-selection

All case-selection methods except the idiographic are practicable with ex ante methods and one method, the randomly chosen case, must use ex ante methods. By this we mean that the researcher chooses cases from a set of potential cases according to an algorithm (set-theoretic or statistical), remaining ignorant of the chosen case(s) until after its selection. This may also be referred to as systematic case-selection.  

The appeal of ex ante case selection, relative to informal (“qualitative”) case selection is severalfold. First, it allows for the separation of theory generation and theory testing. Only in this fashion can an honest test be administered and “curve-fitting” avoided. Second, it ensures that evidence is chosen in a neutral fashion, avoiding the problem of “cherry picking” (choosing cases that fit the researcher’s theory or pre-conceptions). Finally, it assists researchers in identifying the best case(s) from a large set of potential cases wherever evaluations of the various case-selection criteria are complicated and it might be difficult to render a summary judgment without a formal method of analysis.

The prima facie case for ex ante case selection seems compelling. However, to evaluate its utility we must take a close look at the protocol, which presumably goes something like this.

1. Define the research question and the population of theoretical interest. (This eliminates idiographic cases, which are chosen by virtue of their intrinsic or theoretical importance and often do not have a clearly defined population.)

2. Identify a large sample of potential cases in a preliminary fashion.

3. If random sampling is advisable draw a case(s) randomly from the sample. If not, proceed.

4. Measure relevant features of the cases – e.g., D, X, Y, and/or Z (as specified in Table 1) – across the sample.

5. Construct a causal model, if required.

6. Apply the case-selection criteria summarized in Table 1 to identify the case, or cases, eligible for investigation.

7. If more than one case satisfies the criteria, select cases randomly from within the subset of eligible cases (stratified random sampling).

Where it makes sense to do so, researchers are strongly advised to follow this exacting protocol, for reasons adumbrated above. The question is, when is it possible to do so? And when does it make sense to do so?

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One difficulty arises when the population is not well understood or the research question is not well-defined. Of course, one could struggle to define these goals. But doing so may require an encounter with the cases, which of course defeats the purpose. One may set these cases aside, leaving others as testing grounds for the theory. Yet, this constitutes a considerable waste of scholarly resources and is practicable only in situations where useful cases are plentiful. Unfortunately, useful cases are often rare in social science. This is the second difficulty. A third difficulty arises when key variables cannot be measured across a sizeable sample of cases, or when statistical models are highly implausible, either because they don’t provide a good fit for the data or because they strain credulity.

In all of these circumstances the researcher must fall back on informal (“qualitative”) methods of case-selection. However, even when these demanding criteria are met a formal protocol may not always optimize case-selection criteria, summarized in Table 1. Note that in order to be optimized a criterion must be contained in the formula used to select cases. Yet, some criteria such as case-independence and data-availability are not easily encapsulated in a formal model. Generally, one does not become aware of how interdependent cases might be or how much data is available for a given case until one has explored the cases in question, at which point the goal of separating theory-generation and theory-testing has been sacrificed.

With respect to representativeness, one must bear in mind that methods of random or stratified random sampling are developed for use with large samples. With extremely small samples (e.g., one or several) their utility is mitigated, for variance across randomly drawn samples will be large (Seawright & Gerring 2008). Sometimes, detailed knowledge of a case is sufficient to call into question its representativeness. “On-liers” in the model may be “outliers” when one is privy to the facts. If, after examination, a case seems unrepresentative it may make more sense to jettison ex ante case selection rather than persist with a case that is obviously flawed.

With respect to other features of case-serviceability the problem is similar. A case that looks good from the perspective of a statistical model may not look so appealing when one becomes familiar with the intricacies of the setting. This is an especially important issue in the context of crucial and most-similar designs, where the goal is to replicate the virtues of an experiment in a natural setting. Consider the care required to select a good natural experiment. Researchers must invest an enormous amount of time looking into the fine details of a site before it can be ascertained whether – or in what respects – it satisfies the methodological criteria of a natural experiment, i.e., as-if random assignment and the absence of post-treatment confounders. If the notion of choosing a natural experiment randomly from a sample of potential natural experiments seems daft, the notion of choosing a case study that is supposed to mimic the virtues of an experiment may seem equally daft.

For all these reasons, the formal ex ante protocol outlined above should probably be regarded as a point of departure – but not necessarily a point of arrival – for the selection of case studies. It is telling that only 9 of the 153 exemplars listed in Table 3 utilize ex ante case-selection procedures, and in one of these studies (Lange 2009) the initial ex ante analysis is supplemented by further deliberation (ex post) based on features of the cases prior to final case-selection.

Finally, we must consider the employment of ex ante methods of case-selection in the context of the value that an in-depth case analysis might add to a study. Ex ante case-selection usually requires knowing a good deal about the cases in a population so that an informed choice can be made about which case, or cases, to devote special attention to. (The exception is random case-selection, which requires no information at all about cases except but which is very seldom employed.) The paradox is that the more one knows about the population the less informative an

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30 Dunning (2012).
intensive study of a particular case is likely to be. Consider the typical case, where one attempts to choose a case that is descriptively representative of a population. Let us assume that there are a number of factors of theoretical interest and that the researcher’s goal is to describe those features based on features of the chosen case. Now, if one could measure all of these features, ex ante, in the population of interest it is not clear what might be accomplished by an in-depth study of a single case. To take a second example, let us consider the conforming case, which is alleged to be causally representative of a population. Insofar as one can answer this question in a definitive fashion – presupposing a valid and precise measurement of a causal relationship in the population of interest – the value of any subsequent case study is mitigated. Or rather, it is apt to be re-focused from causal effects to causal mechanisms.

To conclude, ex ante case selection is not useful where cross-case analysis is impossible or obviously flawed. Here, cross-case models are likely to be worse than informal, qualitative approaches. Nor is it useful in situations where cross-case analysis is definitive. Here, case selection may be carried out with assurance but there is scarcely any point to it. Ex ante case selection is useful in those relatively rare situations where some feature of a problem may be measured across cases but (a) that feature (descriptive or causal) is still somewhat uncertain or (b) the object of the case study is some other feature of the data (e.g., cases are chosen according to causal effects measured across cases but the goal of the case study is to inform us about causal mechanisms).

IV. Conclusions

In this study, we have laid out a typology that elucidates a diverse array of case-selection methods; we have classified wellknown exemplars and influential (well-cited) case studies into the typology; and we have commented on the viability of ex ante case-selection methods.

We hope to have shown that case-selection is a more complicated venture than it has generally been made out to be. These complications matter for any attempt to assess the utility of different case-selection methods. Before we can judge whether most-similar designs are superior to crucial-case designs we must have a clear idea of what they are, and their different sub-species. Likewise, we must have a fuller sense of their purposes, which are not restricted to causal effects and causal mechanisms. This should contribute to a more selfconscious and transparent approach to case-selection, employing ex ante formulas where advisable.

By way of conclusion, let us return to our point of departure. This essay began by observing the central position of case-selection in discussion of case study research. We pointed out several reasons why methods of case-selection seem more consequential, and more fraught, in case study research than they are in cross-case research.

At the same time, there may be a sense in which case selection has been over-emphasized in the methodological literature. Indeed, the attention given to this subject by methodologists is in stark contrast to the relative indifference of case study researchers, who prefer to focus on within-case evidence – the case itself, one might say. Perhaps it is time for methodologists to follow their lead, turning their attention to issues of internal validity and away from issues of external validity (that are, arguably, impossible to resolve when the sample is so small). An encouraging movement in
this direction is the recent efflorescence of work on process tracing, causal-process observations, pattern matching, and adjoining subjects.\textsuperscript{31}

V. References


Meckstroth, Theodore. 1975. “‘Most Different Systems’ and ‘Most Similar Systems’: A Study in the Logic of Comparative Inquiry.” Comparative Political Studies 8:2 (July) 133-177.


Symposium: Qualitative Comparative Analysis (QCA).” 2004. *Qualitative Methods: Newsletter of the American Political Science Association Organized Section on Qualitative Methods* 1:2 (Fall).


