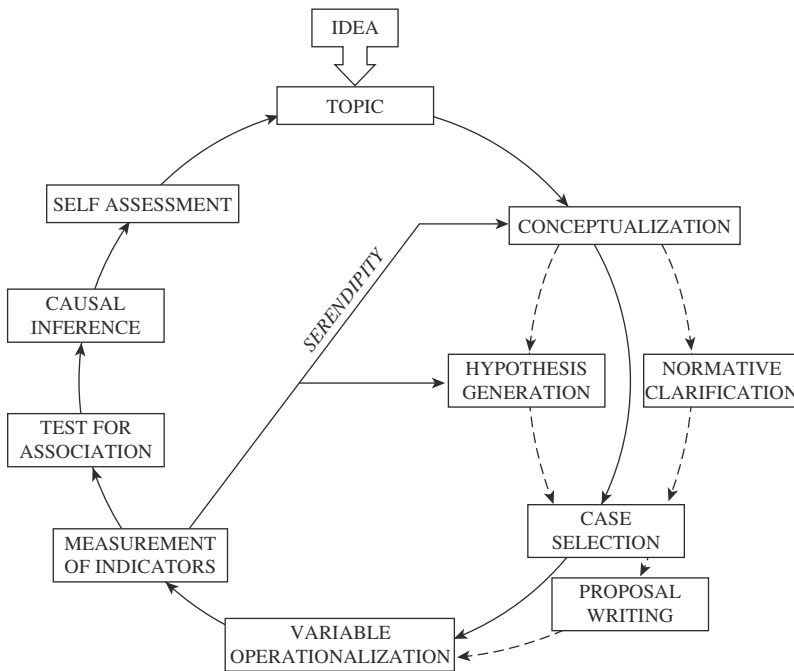# 14 The design of social and political research

Philippe Schmitter

Let us assume that you have an *idea* that has led you to identify a *topic* that you believe to be of sufficient importance and of feasible execution to conduct research on it. It may be a doctoral dissertation, or just a seminar exercise, but regardless of length and complexity no topic can 'research itself'. You will have to translate it – via a series of strategic choices – into a *project*. It is this process of translation from something problematic or puzzling into something on which you can gather valid data and about which you can make compelling inferences that constitutes your *research design*.

Granted, much social scientific research is not self-consciously designed – it is not subject to a deliberate and critical process of choosing its components and defending its overall configuration. In many areas of inquiry, the design is literally given along with the topic. So much research has already been conducted on it that adding yet another case or extending it to yet another time period does not seem to require a novel effort of translation. Indeed, the universal desire of all sciences to produce cumulative knowledge seems to militate against continuously challenging and changing the standard way of doing research. If you do propose a change in design – say, a reconceptualization of the topic, a revised instrument for measuring variation, a different way of selecting relevant cases, or a novel method of testing for association – you will risk confusing your reader-cum-critic. He or she may find it difficult to distinguish whether eventual differences in data or inference are 'really' due to the topic itself or 'merely' to your meddling with the established way of researching it.

Most young social scientists, however, will not be choosing topics whose research design is given. They will have to find or invent an apposite design – and they should be prepared to understand and defend the choices involved. Moreover, if their immediate or eventual intent is comparative – if they anticipate including more than one case or set of observations and drawing inferences across them – then their choice of design will be even more crucial. Making the right strategic choices will greatly enhance the value of the data

Fig. 14.1   The cycle of social and political research

they gather and the inferences they can draw from them; neglecting these choices or taking them for granted could result in idiosyncratic scraps of information and inferences rooted in exceptional circumstances that make no reliable or cumulative contribution to scientific knowledge.

Figure 14.1 is a schematic and idealized representation of the complete 'social and political research cycle'. Each of its boxes involves an important set of interrelated strategic choices, and its implication is that these should be made in the displayed sequence, beginning with an idea that defines a topic at 12 noon and proceeding clockwise until the researcher arrives at an evaluation of his or her findings that may or may not redefine the original topic at midnight. Inside the boxes lie a number of alternative courses of action. Choose among them wisely, and you will do better research. Ignore them or fail to grasp their significance, and you risk accepting serious fallacies at each stage.

The most important message to keep in mind while proceeding through the entire cycle is that there is no single best strategy or set of strategies for researching all topics. Everything depends on your point of departure, the initial substance you have decided to research. At the beginning of the cycle in Figure 14.1, the range of options tends to be most extensive – and, hence, most

confusing. Interesting topics clamour for equal attention; different theories and concepts can seem equally compelling. As one proceeds clockwise, the successive choices are increasingly related to each other and the options become more limited. At some point, you may well adopt or fall into an 'established disciplinary routine'. You can save yourself a lot of time and worry by doing this but this will only benefit you if your topic and, especially, its conceptualization is sufficiently isomorphic with the original – that is, it conforms to the basic characteristics of the topic that has already been successfully researched by others. Applying even the best established and/or most fashionable design to the wrong topic can be a formula for disaster, especially when it comes to drawing inferences.

Very few researchers really enter Figure 14.1 at noon and leave at midnight. Most take shortcuts to get started in the process. Many social scientists begin their research careers already knowing on which case or cases they intend to work. Not infrequently, it happens to be the country from which they come or in which they are trained. So-called 'area specialists' usually have some prior personal commitment involving their knowledge of history, culture or language, and this tends to affect the topics they select. Others may have picked up some novel statistical technique or measurement device that they wish to show off – and they search about for an apposite topic to which to apply it. Perhaps the most common (and, in my view, pernicious) point of departure concerns theories or approaches that are currently fashionable in sociology or political science. Imbued with the conviction that only those espousing such a 'paradigm' will find eventual employment, young researchers are prepared to take up any topic – no matter how trivial or obscure – if only to demonstrate their fidelity to its assumptions and postulates.

Do not presume that, once in the cycle, you will have to go all the way around. As we shall see in the conclusion, there are many points of exit that will still permit you to make an original and significant contribution to knowledge.

Wherever you have really begun your research and whatever your motives for doing so, I recommend that you at least pretend that you are beginning at the top of Figure 14.1, if only to help you clarify *ex post* the design choices you should have taken deliberately or have already taken implicitly. Try to imagine that it all began with an idea about a substantive topic that is important to you and that only later did you place it in an explicitly conceptual context, elaborate specific hypotheses about its occurrence, choose the cases to study, and so forth.

Now, we can proceed to look sequentially into the 'little black boxes' in Figure 14.1. Attached to each of them the reader will also find a list of 'possibly optimizing' choices and 'potentially damaging' fallacies.

Choice of topic

No one can predict where and when ideas will appear. With some knowledge of the researcher's personal and professional background, however, it may be a bit easier to predict the conditions under which an idea becomes a topic – that is, when someone will attach sufficient importance to a given thought and place significant boundaries around it to make it worthy of investing his or her energy to explain how it came about or what its consequences might be. This highly personal effort at selection can be an important source of distortion throughout the rest of the design and, especially, when it comes to drawing inferences from whatever data distributions or associations are generated. The very fact that you care enough to select some topic probably means that you also value what it contributes or the effect that it has. However subliminal the thought may be, your values become embedded in the topic and can exert a persistent influence on your choices as you make your way around the rest of the research cycle. They may have an even greater impact when you decide to make a 'premature' exit from the cycle.

It is often the case that one is attracted to a topic because the society or polity also cares about it. Never is this more evident than when the subject matter is in crisis or in fashion. As social scientists, we are attracted to phenomena that call attention to themselves – whether they do so by creating further problems or by providing novel solutions. Which is another way of saying that our topics tend to be either failed experiences at the end of their useful existence or recent successes that have yet to reveal their complete impact. Rarely does one come across designs explicitly focused on explaining social or political phenomena that are mediocre or inconsequential.

Grosso modo, topics of research come in two guises: (1) *projections*, where the researcher is confident that the existing approach and methods are adequate and deserve to be applied to units or time periods that have not already been covered or with greater precision to cases that only seem to be exceptional; and (2) *puzzles*, where the researcher begins with the assumption that something is deficient in the way that the topic has been previously handled and that the units or time periods to be examined will demonstrate the

existence of anomalies. Both projections and puzzles should be approached in the same 'critically rational' manner, but the perspective of the researcher differs. If the topic selected is regarded as a projection, he or she has the intent (at least, initially) of confirming established wisdom and will take more seriously the obligation to make a cumulative contribution to knowledge within a specific discipline or paradigm. The perspective when tackling a puzzle leads one to seize on anomalies that seem to expose deficiencies in how the topic has been conceptualized, measured or reported, and that is more likely to lead the researcher to alternative concepts and methods – frequently by drawing on other disciplines. Needless to say, both are capable of making valid contributions; both are needed by all social science disciplines.

Possibly optimizing choices

1 Choose a topic that you care enough about to be willing to spend the time to complete the project.
2 Choose a topic (and make an argument) that interests other social scientists (even those outside your field); the better it is, the more it will interest those working in adjacent fields and disciplines.
3 Specify the temporal, spatial and, if necessary, cultural boundaries of the topic in a way that makes the research feasible, but does not make it trivial or 'unique'.
4 Acknowledge your initial source of inspiration for the topic and your personal preference about its outcome, without apologizing for them.
5 Never justify your selection only on the grounds that it has been 'under-explored', and, especially, do not ignore, trivialize or dismiss what has already been written on the topic.
6 Try to reach as far back as possible in social and political theory to find grounds for the relevance of your topic and avoid being manipulated by academic fad and fashion.
7 By all means, listen to your advisor and your peers, but be absolutely certain that, regardless of who first suggested it, the topic 'belongs' to you.

Potentially damaging fallacies

1 'Fad-ism': Your topic (or method or theory) is being very much and very favourably discussed right now in your field, so that if you adopt it your work will be less criticized and you will be more likely to find a job.

2 'Wishful thinking': Your topic has already produced well-publicized and promising results for the society or polity; therefore, if you conduct research on it, your findings will be taken more seriously and favourably.

3 'Ambulance-chasing': Because the topic of your research is presently in crisis, you will have greater access to data and the public will be more interested in whatever you find out.

4 'Presentism': The assumption that whatever you find associated with some topic in the present must have been there in the past and will probably remain there in the future.

5 'Standing on the shoulders of the past giants': This might apparently allow you to see further and to avoid being distracted by the squabbles among contemporary pygmies – yet those giants might not have been looking at the same thing or in the same direction.

Conceptualization

Almost all substantive matters emerge 'pre-conceptualized' in the strict sense that they can only be recognized by the potential researcher and shared with others if they are expressed in some intelligible language. The idea may come initially as a shape or a colour or an emotion, but *words* are the indispensable way in which it acquires factual specificity and shared significance. The complication for research resides in the high probability that the words initially involved will be those of the social or political actors involved – which implies that their words could bear many different meanings and be attached to a wide range of contrasting assumptions.

Conceptualizing a topic involves translating the words that surround it in 'real-existing' societies or polities into *variables* (although see Della Porta, ch. 11, and Bray, ch. 15). These are not just fancy academic labels applied to a specific event or process. They should identify analogies, generic conditions that are shared by a distinctive set of events or processes and can take on different values over time – whether these are quantitatively or qualitatively observed. They acquire their peculiar status as causes or effects according to the way they are connected to other variables by theories. Once these variables have been assembled, whether from the same or varying theories, they constitute your provisional *argument* concerning the topic you have chosen to explain.

Which brings us to the 'Elephant-in-the-Room' that is so rarely mentioned but so frequently the source of confusion at each stage of designing research. Even the most elementary and frequently used concepts – such as class, status, gender, age, region and religiosity for explaining voting behaviour – derive

their meanings from being inserted into a more comprehensive (and presumably coherent) matrix of concepts (see Kratochwil, ch. 5, and Mair, ch. 10). Their definitions may sound the same and, as we shall see later, operationalization of these variables may even be identical, but their role depends on prior assumptions and contingent relations that differ according to the theory, paradigm, approach or framework that is being applied. And no single piece of research can possibly specify what these are. If you tried to do this, there would be no time or space left for your analysis. In other words, all social and political research is part and parcel of 'the state of theory' prevailing at the moment it is conducted. No research can be conceptualized *ex novo* without reference to what has been produced already on that and related topics. This applies just as much to those who are trying to solve puzzles as to those who are 'merely' trying to make projections.

Choosing one's concepts is only the first step. Making them into variables means assigning a status to them, and this is where their embeddedness in theory most saliently enters into the research design. The most important task is to distinguish between those that are regarded as *operative* with regard to the chosen topic and those that are *inoperative*. The former are expected to play some discernible role in the explanation of outcomes – either as an *explicans* (that which does the explaining) or as an *explicandum* (that which is to be explained). The more elaborate the prior theory and, hence, the conceptualization derived from it, the more it may be possible to assign different statuses to the operative variables, for example, by distinguishing between primary and secondary ones (according to their explanatory power), direct and intervening ones (according to how near the effect is to the cause), continuous and episodic ones (according to how constant in time their effect is), and so forth. Needless to say, all these initially assigned roles can be inverted, especially where and when the objective is to explain a relatively long-term sequence of social or political processes. *Inoperatives* are variables that are present and can be expected to take on different values during the subject matter being researched, but whose effect is not expected to produce a discernible or significant difference. Of course, when it comes to making eventual inferences, allegedly inoperative variables may turn out to be an important potential source of spuriousness. Even *constants*, variables that were present but not thought to vary during the research period and, hence, *a priori* considered not capable of contributing to variation in the outcome, may gain eventually in importance – especially when it becomes evident that the impact of operative variables was contingent on slight modifications or even simple reinterpretations of such background factors. Hopefully, *irrelevant* variables – those whose

variation cannot conceivably be logically or empirically associated with the topic under investigation – will remain that way.

Possibly optimizing choices

1 As much as possible, avoid references to specific persons, countries or cultures with 'upper-case' names by using only 'lower-case' variables to describe them and their prospective effects in your argument.

2 There is nothing wrong with using a 'hunch' as your starting point in conceptualization – the world surrounding most interesting topics is usually full of them – but try as soon as possible to identify the more generic theory in which this hunch is embedded, switch to its language, and explore its axioms or presumptions before going further.

3 Try to avoid 'multicollinearity' – clusters of variables that are closely associated with each other – and simplify by only using the dominant variable in such clusters or providing it with an ideal-type connotation that captures as precisely as possible the nature of the cluster.

4 Make as explicit as possible not only the operative but also the inoperative variables and the constants, those characteristics that do not vary, in your argument – and be prepared to change their status in the course of conducting the research.

5 When using classification systems (see Mair's chapter), make sure that the categories are both inclusive of all observations and exclusive in their assignment of every single observation – and that all of them are potentially relevant to explaining outcomes, including those that are vacant for the moment.

6 Specify as soon and as explicitly as possible the universe to which your conceptualization is intended to apply in both time and space.

7 Exercise caution when using concepts and variables across long periods of time or different cultural contexts, since their meaning to actors and, hence, their effect may change.

8 Strive for parsimony by eliminating double-dealing or superfluous variables, but without resorting to excessive simplification. One way of doing this is to restate your argument several times and to make it more concise each time.

Potentially damaging fallacies

1 'Obscurantism': If you cloak your conceptualization in highly abstract terms or fit all of your observations into some complicated classification

scheme, no one will notice that all you are doing is describing what hap-
pened.

2 'Attribute-ism': The more definitional attributes or analogous properties
you attach to a given concept, the more significant it is likely to be in
explaining the outcomes you want to understand.

3 'Concept stretching': A concept used successfully to identify an analogy
among events in one time and place must be equally valid when applied to
other times or places.

4 'Isolation': Your preferred variable plays such an important role in explain-
ing your topic that it can be conceptualized, measured and manipulated
alone, without regard for the network of other variables surrounding it and
the prior axioms upon which it rests.

5 'Novelty at any price': Because existing concepts are so embedded in (old)
theories, by inventing and using novel ones, you will be credited with
greater originality in your research.

6 'Arbitrariness': Since all concepts are basically arbitrary – a function of
unpredictable practical uses and/or theoretical fashions – it will make no
difference which ones you use, provided that your public and peers come to
accept them.

7 'Consensual-ism': If everyone in your discipline is using some concept and
seems to agree on its meaning, as well as its explanatory relevance, you
should feel safe to do so.

Formation of hypotheses

Not all research designs involve the formation (or the testing) of explicit
hypotheses. There exists a very broad range of social and political topics for
which it is possible to conceptualize the variables that may contribute to an
explication, but not to assign any sort of provisional 'if . . . then . . .' status to
their relationships. For these topics, the apposite research logic is one of *dis-
covery* and not of *proof.* The purpose is to improve one's conceptualization of
a topic, probe its plausibility against a range of data and eventually generate
hypotheses among its conclusions, but it would be premature to expect them
as a pre-condition for conducting the research itself.

The determining factor is again that 'Elephant-in-the-Room', the prevail-
ing state of theory on a given topic. Substantive matters that are of recent
occurrence, that are only characteristic of a small number of cases, that incite
strong emotions or political controversies, or that fall between different social
science disciplines are obvious candidates for 'discovery' status. The potential

researcher is reminded that this should not be taken as a sign of inferiority. Somewhere behind all social scientific research that today routinely follows the logic of proof, there must have been a glorious moment in the past when someone launched a voyage of discovery. Unfortunately, behind the facade of increased professionalism and standardization of techniques, this message has been suppressed. Only the most intrepid of young scholars will accept the challenge of trying to make sense out of alternative conceptualizations of the same topic; or piecing together potentially coherent and general arguments by 'process tracing' on the basis of specific cases; or admitting that, in instances of highly interdependent and complex social or political systems, it may never be possible to distinguish between independent and dependent variables, much less to express them in terms of a finite set of bivariate relationships.

Possibly optimizing choices

1 Ensure that the assumption of any 'if . . . then . . .' relationship is sufficiently precise that it specifies its 'micro-foundation', the functional dependence, structural mechanism or intentional logic that is supposed to connect its variables and, where possible, introduce an independent measure of its presence.

2 Do not assume *ex ante* that only individual human beings are capable of laying 'micro-foundations', when the 'real-existing', historical world is chock full of social and political units that have acquired the capacity to act collectively in ways that cannot be reduced to individual intentions and choices.

3 Ensure that the presumed cause is independent of the presumed effect, and not parallel or convergent manifestations of the same social or political process.

4 Where possible, specify explicitly the existence of intervening conditions or prevailing constants that must always be present for the hypothesized relation to produce its effect – even if these contextual factors do not vary during the research.

5 An ideal research situation can emerge when you find yourself in a 'two-ring circus' – when two rival versions of the same hypothesized relation are plausible and would explain diametrically different outcomes based on different theoretical assumptions.

6 Be prepared to recognize and deal with 'equifinalities', similar outcomes that are produced by different sequences or mechanisms, when they emerge, and therefore to test different sets or, better, 'strings' of hypotheses – not just isolated ones.

7   Remember that you always have three hypotheses to test, namely, the ones that suggest a positive or a negative relation, and the null hypothesis that no 'if . . . then . . . ' relationship exists. The latter should be regarded as the most probable in occurrence. Everything may be related to everything else in our complex environment, but not always in a predictable direction or to a significant degree.

8   Try *ex ante* when elaborating hypotheses to differentiate between variables that you think are 'necessary' (always likely to be present when the outcome is present), 'sufficient' (always and only present) and merely 'helpful' (sometimes present, sometimes not). Never assume that your set of variables is going to be both 'necessary and sufficient' and, therefore, make space for the inevitable 'error term'.

9   Since most research projects consist of 'clusters' and 'chains' of related hypotheses that contribute to explaining a selected outcome, it is often useful to draft a 'model' of these simultaneous and sequential relations using time and space as co-ordinates.

Potentially damaging fallacies

1   'Scientism': If your variables are not organized into hypotheses with clearly differentiated independent and dependent variables, your research will not be scientific.

2   'Fear of failure': If your hypothesis or hypotheses are disproved, you will have made no contribution to knowledge.

3   'Infinite regress': All hypotheses about variable relationships in the social sciences are preceded by a potentially infinite historical chain of causality and consequence, therefore, it makes no difference when you choose to break into that chain.

Selection of cases

For all but a few projects, the potential number of societies or polities affected by the chosen topic will exceed the researchers' capability for gathering data, testing for associations and drawing inferences. It is, therefore, normal that only some subset of these units will enter into your analysis. One of the most prominent of the strategic choices you will have to make involves the number and the identity of those to be included and the criteria you impose to select them. This can vary from one unit (the single case or person) to as many as

are apposite (the universe of those affected); but there is a fairly inescapable trade-off between the quantity of variables that have been included in your initial conception of the topic and the number of units for which you will be able to gather data. Including more cases probably also means poorer quality data, more missing observations and greater problems of conceptual equivalence. Inversely, the more narrowly you have defined and operationalized those variables – that is, the lower they are on the ladder of abstraction – the less likely they are to be relevant in a wide range of cases.

Case selection may have its practical side when it comes to gathering data and, especially, making one's own detailed observations; but its real payoff is analytical. Manipulating the identity of cases provides most sociologists and political scientists with their closest equivalent to experimentation. It 'simulates' the introduction of control variables. By 'holding constant' across the sample such potentially relevant conditions as cultural identity, geographic location, level of development and temporal proximity, the researcher can at least pretend that variation in them is unlikely to have produced the outcome one is looking at. Granted that the controls can be somewhat approximate and that there still will remain many potential sources of 'contaminating' differentiation among units in the sample – still, this is the best design instrument that he or she has available. It should, therefore, be wielded with deliberation – and caution.

Strictly speaking, the researcher does not select individual cases but 'configurations of variables' that co-habit the same unit and may even co-vary in a unique or distinctive fashion within that unit. But one cannot analyse 'France' as such and compare it with, say, 'Spain' or 'Italy'. There are simply too many different (and potentially relevant) conditions within each of these countries with regard to almost any topic you choose to work on. This holds even when comparing micro-units within the same country, where the number of variables can be more reliably controlled because of common constraints at the nation-state level. So-called 'holistic' research is, therefore, largely an illusion in social and political research and, when tried, it usually amounts to little more than a detailed or 'thick' description of one case (or of parallel ones if more units are covered) (but see Bray, ch. 15, for a different view).

This is not to say that there are not significant differences between designs that are driven by the effort to isolate a small number of variables and test exclusively for their association with other variables across a larger number of units, and designs that begin with a large number of interrelated variables (often combined via ideal-type constructs) within one country and then seek

to find significant and persistent connections across a few, carefully selected units of an allegedly comparable nature. But in either strategy, what you are usually comparing are variables – one or many, alone or in clusters – not units.

This brings us to the second aspect of case selection, which has long been taken for granted and yet has recently become of growing concern. For a unit of observation to be a valid case for analysis, it must possess identical or, at least, comparable degrees of freedom with regard to the topic under investigation. A design that drew inferences – descriptive or causal – from a sample of units composed of Brazilian municipalities, Mongolian provinces, Spanish *comunidades autónomas* and the permanent members of the United Nations Security Council about the efficacy of taxation systems would not attract much attention. Much as its author might (correctly) protest that this 'sample' embodies a 'most-different systems design', critics would (rightly) object that actors in these units did not have remotely equivalent powers to make or enforce their decisions on taxation.

The usual formula for getting around this problem was to select only units that were at the same level of aggregation and enjoyed the same formal status within the world social and political system. This presumably explains why so many comparative research projects have been based on nation-state units or, to a lesser degree, on relatively autonomous subnational units within federal or confederal systems. The *reductio ad absurdum* of this strategy has been reached with large-N comparisons containing all the members of the UN for which data can be obtained – despite the blatant fact that these so-called sovereign states have radically divergent capabilities for governing their respective populations or even satisfying their most elementary needs.

Since Donatella della Porta has contributed an entire chapter (ch. 11) to this volume that deals extensively with the issues involved in case selection, I have little more to add. I will, however, provide a pedagogic device that I have found useful in explaining to students what their options are at this point in the research cycle (Figure 14.2).

Where researchers are committed to producing scientific knowledge (defined here as causal inference), the preferred case selection strategy should usually be the experimental one, choosing the units of observation randomly and introducing some element of change in a subset of them while holding variation constant for the others. Unfortunately, most social and political scientists have to operate in 'real-existing' settings, where this is not possible. And even when they are permitted to engage in experimentation, the topics tend to be so trivial and the settings so artificial that projecting inferences based on such findings to more 'realistic' contexts is very hazardous.
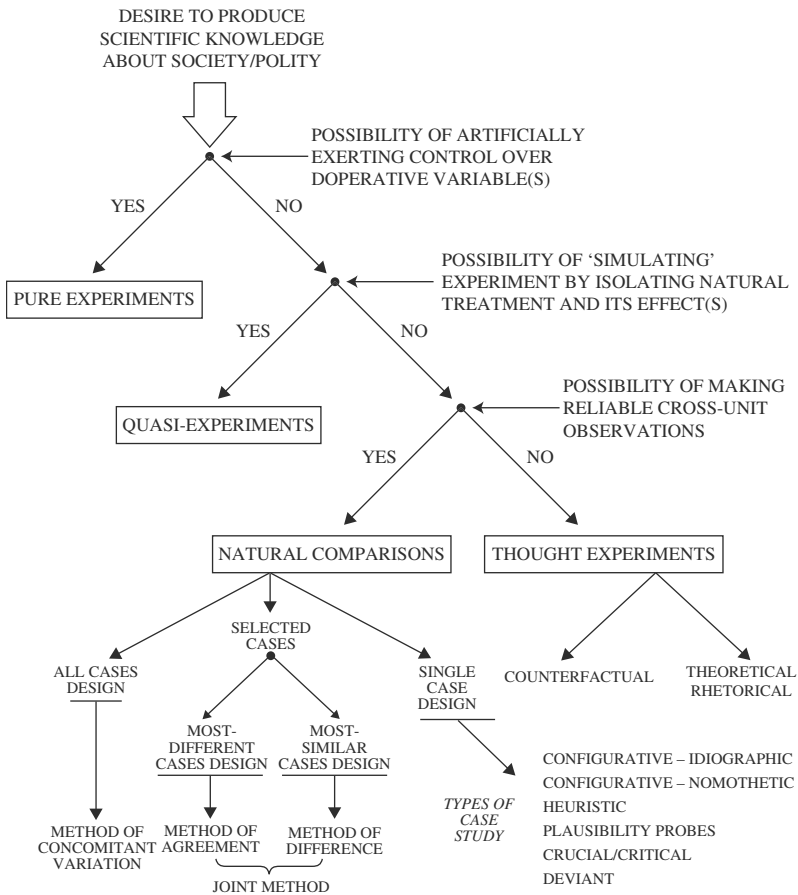
Fig. 14.2   A schematic for the selection of cases

Quasi-experiments may be second-best, but they offer some interesting advantages, with regard to both the efficiency of research and the credibility of inferences. The case-base can be as low as one, although it is better to replicate the quasi-experiment in several other settings, if possible, within the same time frame. They are, however, limited to real-world situations where the independent variable is highly discrete and temporally circumscribed and where data-gathering over a sufficient period of time has been consistent and reliable. Assessing the effect of a new public policy or the impact of some unexpected social or natural event tends to fit this narrow bill of particulars, but only if nothing else is happening to the unit or units at the same time. This is also a strategy of case selection that is especially vulnerable to diffusion or contagion effects, if the units involved know of each other's behaviour.

Most sociologists and political scientists will have to settle for the study of variations in their subject matter that appear 'naturally', whether within a single case or across different numbers of them. Della Porta (ch. 11) explores the implications of making these choices.

There are, I would add, a number of other alternative strategies that are not usually included in texts on research methods or design – presumably because their scientific status is dubious. They typically arise in contexts in which it is risky or impossible to observe and record the behaviour of 'real-existing' social or political units. All involve what Max Weber once called 'thought experiments'. The best-known goes under the rubric of *counter-factualism* and involves the researcher in an effort to imagine what would have happened to the topic if some condition, person, event or process had not been present. Usually, this focuses on a single country – for example, 'How would Germany have evolved politically if Hitler had not been "available" in the early 1930s?' It can also be applied to a sample or even to the universe of cases – for example, 'What would be today the level of international insecurity in Europe if the EU did not exist?' Or, 'How many people in the world would know how to speak English if the Americans had lost their Revolutionary War?' If this sounds 'exotic' and somewhat 'flaky', you should remember that every time that you invoke the famous and indispensable Latin phrase, *ceteris paribus,* before advancing a hypothesis, you are being a counterfactualist.

Moving even further from social scientific orthodoxy, one finds a vast number of seminar exercises, MA papers and PhD dissertations that are essentially rhetorical, theoretical or normative. These certainly deal with topics – often more important ones for 'real-existing' societies and polities than those chosen by empiricist-cum-positivists – but their purpose is to follow the development of concepts or discourses over time, or to examine the logical consistency of particular arguments, or to promote the ethical acceptance of specific forms of human behaviour. Such projects do indeed involve research. In a 'heuristic' and 'nomothetic' way they have influenced empirical inquiry. Just think of the impact of recent works by John Rawls, Jürgen Habermas and Jon Elster upon how even the most 'hardcore' empiricists select and conceptualize a wide range of topics (see Bauböck, ch. 3).

Possibly optimizing choices

1 If you are not trying to cover the entire universe, consider the possibility of selecting a sample of cases randomly and how that would affect your project.

2 As a rule, when randomization is excluded and you have to be purposively selective, choose your cases based on their relation to the independent variable or variables rather than the dependent variable or variables.

3 And when you make this choice, try to ensure that the cases chosen 'represent' as wide a range as possible of scores on those independent variables.

4 When your topic will not permit this, when you are motivated to research something precisely because it involves a compelling, arresting or extreme outcome and you therefore have to select on the dependent variable, remember this as a potential source of bias when it comes to drawing inferences.

5 Keep in mind that you do not have to use only one strategy of case selection and that so-called 'nested strategies', where you start with a large N of cases and relatively few crudely measured variables and, subsequently, shift to a small N with a much more detailed 'battery' of variables, can give you the advantages of both strategies when it comes to drawing inferences.

6 Always prefer the lowest level of spatial or functional aggregation that is compatible with the actor behaviour presumed by your conceptualization, since you can subsequently reassemble your research upwards – but not downwards – in scale.

7 No matter which or how many cases you initially select, some may prove to be 'decomposable', in that you may be able to generate additional cases by dividing up the initial ones, but only provided these subunits possess some and the same degree of autonomy.

8 Before selecting the number and identity of the cases for which you intend to gather data, make sure that you are aware of the criteria that you originally used for classifying your topic and ask first: 'What is this a case of?' Only after satisfying that demand will you know what units are 'eligible' for inclusion, and you can proceed to exclude some of them for good reason.

Potentially damaging fallacies

1 'Notoriety': Just because a particular case has been prominent in public discussion, it will be more interesting to research, and others will pay more attention to your research.

2 'Numbers': It is always advantageous to have a larger number of cases, even when, by adding them, you are compelled to attenuate their relation to the topic or to use less valid indicators.

3 'Cruciality': Because a given unit is an outlier according to your criteria of case selection, it will be a crucial case whose conformity or non-conformity provides a definitively significant test of causal association.

4 'The illusion of control': Selecting cases because they seem to share certain general cultural, locational or structural characteristics necessarily controls for their relevance – when it is still possible that minor or qualitative variations in 'controlled variables' could be affecting variation in what you are trying to explain.

5 'Contemporaneousness': In units chosen for comparison within the same time frame, the actors must have similar (or at least sufficient) awareness of the relevance of common variables and be capable of acting upon them simultaneously – when these units may be at different points in longer cycles or simply on different time schedules.

6 'Imitation': When actors in the selected units are acutely aware of having to deal with some topic within the same time frame, they will also be sensitive to what others are doing about it and will learn from each other's successes and failures – in fact they may be quite ignorant of what the others are doing.

Writing the proposal

This stage in the research cycle is 'optional', although highly desirable. Different graduate programmes place greatly different emphasis on the importance of defending a formal proposal. Some require it before allowing the candidate to 'go into the field'. My personal experience suggests that the greater the plurality of approaches or paradigms surrounding a given topic and present in a particular institution, the greater will be the emphasis on writing and defending your proposal. In scholastic contexts dominated by a single theoretical or disciplinary orientation, the effort may be eschewed completely. The reigning orthodoxy favours problems rather than puzzles and may even dictate in considerable detail how topics should be conceptualized and operationalized. At the extreme, there is no 'field' to go into, no specific cases to select and no measurement details to discuss. What matters at this stage is the normative or logical consistency of the 'argument', of one's conceptualization of the topic and how well it conforms to prevailing orthodoxy. The number and identity of cases are relatively unimportant, if not irrelevant, to the extent that both prior axioms and subsequent expectations are believed to be universal. The data can be simulated or assembled from the usual sources for illustrative purposes. The eventual inferences are usually predictable and in line with original expectations. The fellow members of your 'research club' will enthusiastically congratulate you on your cumulative contribution to knowledge. Practitioners of other disciplines and members of other clubs

within your discipline will yawn and tell you that you have 'rediscovered the wheel' or produced something utterly trivial. In other words, there are costs as well as benefits in belonging to an established research tradition.

Another condition affecting the utility of proposal-writing is its potentially critical role in obtaining research funding. Where such support is assured or not subject to competitive pressures, the researcher may content him- or herself with a brief statement of intention. Otherwise, your ability to summarize coherently and justify convincingly the design choices that you have made up to this point could make all of the difference in determining whether you will be able to carry out your project at all. Although it is not frequently discussed openly, this 'commercial' aspect of proposal-writing can also be a source of distortion when the preferences of the sponsor come to be anticipated in the proposal itself and the researcher finds him- or herself pandering to them by modifying the topic, changing its conceptualization, restricting the range of hypotheses and even selecting different cases in an effort to please the prospective sponsor. More experienced researchers soon learn how to 'fine-tune' their proposals to get support from donors and then go on to follow the course of inquiry they think will lead to the most compelling inferences. Fortunately, national or supranational sponsors rarely control for conformity between proposals and the research actually performed. At most, they may be interested in whether or not the policy implications drawn from such research conform to their preferences.

The 'real' purpose of writing a proposal should be to give the researcher a chance to sit back and reflect critically on the strategic choices he or she has made – and to exchange these reflections with supervisors and peers before plunging into the inevitably messy and absorbing process of gathering data and trying to make sense out of them. There may be subsequent moments for self-criticism and changes – see the remarks below on the importance of *serendipity* – but writing and defending a proposal at this stage offers a unique opportunity to 'rewrite' and 'resubmit' before becoming irrevocably locked into a course of action.

Operationalization of variables

In principle, the conceptualization of variables should be carried out beforehand and without regard for how they will be converted into indicators and eventually measured. There is a good reason for this. What is of paramount theoretical importance is to specify clearly the condition or factor that is

supposed to be present in order to produce some anticipated effect – alone or in conjunction with other variables. Having previously and independently conceptualized the projection or puzzle in such a fashion should provide a strong incentive subsequently to specify the observations that need to be made in order to verify the presence, magnitude, direction, or persistence of that variable. During the early stages of research, this means that you should adopt the attitude that all social and political variables can potentially be operationalized – and later be prepared to compromise when you start looking for indicators in the real world.

In practice, unfortunately, anticipations of such difficulties do tend to intrude and can even inhibit scholars from using concepts that are known to be 'impossible' to operationalize. Just think of such indispensable political properties as power, authority and legitimacy; or of such social ones as esteem, respect and trust. For none of them is there a standard and easily accessible set of measures. Even elaborate (and expensive) attempts to operationalize them based on 'reputational' criteria from public opinion surveys have been problematic. And criticisms of these efforts become more insistent the more such indicators are stretched across countries and over time.

Another way of putting this dilemma is that there are bound to be *trade-offs* that have to be made at this stage in the research cycle. The higher one's concepts are on the ladder of abstraction – and, presumably, the wider their prospective range of application – the more difficult it is going to be to make convincing observations about their presence in a specific case or set of cases. Increase the number of units in your study – either of persons or of organizations – and you are almost bound to run into problems with missing data and misleading indicators. Do not be afraid to make these trade-offs, but do so self-consciously. Tell your reader-cum-critic when you are settling for a less satisfactory indicator or a less specific level of observation. Be prepared when necessary even to eliminate cases, but also be sensitive to how this may distort your eventual capacity to draw inferences. Those research sites where operational requirements are most difficult to satisfy are usually places where social and political behaviour is the least 'normal' and their exclusion from the design will probably narrow the range of variation and reduce the eventual strength of association.

The theme that haunts all aspects of this stage of the research cycle is *validity*. Do the observations you propose to make accurately reflect and, hence, capture the meaning of the concepts you have chosen to bear the burden of explanation? No matter how accurate the observations, how comparable they are across units, how replicable they turn out to be when another scholar makes them, if they are

not valid, your research will have broken down at one of its most vulnerable points. You may well have discovered something important and the associations revealed by your indicators might be incontrovertible, but you have not proved (or disproved) what you started out with. Your findings are irrelevant in the strictest sense. They have told you nothing about the topic you announced that you intended to work on – unless you are prepared to rely on serendipity (see below) and reconceptualize your entire project from its very origins.

Possibly optimizing choices

1 Pay close and critical attention to the correspondence between your initial concepts and their proposed indicators or assessments by comparing them to research by others on the same or related topics.
2 Be wary of variable specifications and of empirical indicators that have been applied routinely over time and across units to measure different concepts.
3 Make sure that the concept and its indicator(s) are applied to the same level of analysis and are as close as possible in level of abstraction.
4 When available, use alternative operationalizations and multiple potential indicators and, where necessary, rely on 'triangulation' among them to resolve disparities and to improve validity.
5 All things being equal (although they never are), you are better off using unobtrusive rather than obtrusive indicators, since the actors whose behaviour is being observed will have less of an opportunity to respond strategically, ethically or emotionally to your request for information.
6 Remember that there are various ways of assessing the validity of indicators, ranging from consensus among independent respondents to co-variation between different 'internal' measures and, least reliably, correlation with other hypothesized 'external' outcomes.

Potentially damaging fallacies

1 'Availability': This indicator exists and has been used successfully by others; therefore, it must be valid when applied to your topic.
2 'Operationalism': You decide to include in your analysis only variables for which you know that a valid (or consensually accepted) indicator already exists.
3 'Mimetism': 'X' got away with using data on this to indicate a concept similar to yours, even when drawing upon a different theory; therefore, you can safely use it for the same purpose.

4 'Ignorance of the uncertainty principle': If you operationalize a variable by intruding on the 'real-existing' world of your respondent, you can nonetheless ignore the possibility that his or her answer will be contaminated by prevailing norms of correctness or strategic calculations of interest, or that you will be creating rather than measuring variation.

Measurement

At this point in the cycle, your choices will be more or less dictated by the ones you have already made – whether you did so consciously in relation to the specificity of your problem or puzzle (as I hope was the case) or whether you settled into an established research tradition – whether quantitative or qualitative – and obediently followed its dictates. Moreover, there is a good reason why you should let yourself 'go with the flow' at this point. Using existing techniques of observation and indicators for variables not only saves you a lot of time and anxiety, but can also provide you with an element of internal 'quality control' – provided that the measures used are valid, that is they capture the characteristics of the variable that you are relying upon for an eventual explanation. When it occurs, successful replication of previous research is a very desirable result – and one that can be personally very reassuring. Should you decide to invent and apply a new indicator or, worse, battery of indicators – especially to measure some frequently used variable – you will have to make an especially strong effort at justification. Otherwise, you will run the risk at the inference stage of confounding the reader: 'Is this seemingly compelling finding really novel, or is it only due to some change in measurement?'

The discussion on measurement tends to be dominated by the distinction between quantitative and qualitative indicators – with a marked bias in favour of the former. There is no reason to be surprised by this, since most methods texts are written by quantifiers and they have convincing arguments in their favour. Numerical data are said to be more *reliable*, i.e. more likely to provide agreement among independent observers, more *accurate*, i.e. more likely to produce agreement across units, and more *useful*, i.e. more compatible with different ways of testing for association. Certainly, the social science disciplines have tended to assign greater 'scientific status' to quantitative than to qualitative research – and to reward its practitioners accordingly.

This is unfortunate for at least three good reasons: (1) it has encouraged researchers to attach numbers to variables when the validity of their connection with the designated concept was dubious; (2) it has resulted in the

exploitation of standard numerical indicators whose multiple components are often theoretically disputable and whose weighted combinations are poorly understood by those who use them; and (3) it has discouraged the innovative use of more direct and imaginative techniques of observation – precisely to capture qualities inherent in complex and contingent relations. You can assign a number to anyone and anything; but nothing guarantees that the assignment will produce relevant information. If these qualities are differences in kind (nominal) rather than in magnitude (cardinal or ordinal), then – whatever the rule of their assignment – the number could well be a worthless piece of disinformation. What matters is how you have conceptualized your topic, not the allegedly superior virtues of one over another form of measurement.

Of all of the stages in the cycle, this is probably the one that is best suited for *serendipity*, for learning from the research process itself in ways that can feed back to your previous choices and lead you to introduce improvements in them before 'path dependence' has completely taken over. At last, you are back in touch with the 'real-existing' subjects/agents of your topic – having spent much time wandering around making abstract 'disciplinary' decisions. If you are lucky, they will talk to you directly about their intentions and perceptions, and they may even have some opinions about what you are asking them and intend to do with their answers. Even if your research relies exclusively on secondary or publicly available sources, there can be 'voices' in such documents that can speak in ways you have not anticipated. Of course, there will be a lot of sheer 'noise' generated by the data you are gathering, and that can be very confusing when juxtaposed to the relatively parsimonious approach you have been applying to the topic. Nevertheless, keep your eyes, ears and mind open for subtleties and surprises, and be amenable to introducing 'course corrections' – even some that go all the way back to the boundaries you initially placed around the topic or key aspects of your original argument.

Possibly optimizing choices

1 Routinely test for the reliability of indicators, if possible by using alternative sources of data and/or alternative persons to score the data.
2 If validity requirements can be satisfied, opt for quantitative over qualitative measurement, since the technical advantages are considerable and because you can more easily move from the former to the latter.
3 Always opt for the highest, most informative level of measurement possible (given the nature of the variable), since it will later be possible to shift to a

lower level. Cardinal data can always be made ordinal, and virtually anything can later be dichotomized or filed away in nominal categories – but you cannot move in the reverse direction.

4 Make your instructions – even if only for your own use – concerning the assignment of quantitative scores or qualitative labels as transparent and complete as possible so that the measurement operations can be replicated by you or someone else in the future.

5 Especially when working on the macro level of a complex society or polity, most variables will contain multiple components and be indicated by composite measures – which should obligate the researcher to devote concerted attention to how such 'scales' are aggregated.

6 Especially when gathering information over time about social or political processes, make sure to check that changes are not due to modifications of the instruments of observation rather than to changes in actual behaviour.

7 Many measurement devices are calibrated to pick up only relatively large-scale and consequential changes in variables, which means that they may systematically fail to capture more modest and gradual ones. Social and political 'revolutions' are always recognized; 'reforms' are more often underreported, until their effects have accumulated sufficiently to draw attention to them.

8 Try to estimate before actually gathering the data where the error sources are most likely to come from and how they will affect your findings. Worry less about random errors (they will attenuate possible associations) than about systematic ones (they will bias the direction of your findings).

9 Try to catch yourself before adjusting the data or correcting for errors in them in ways that make these data fit better the general expectations or specific hypotheses with which you started.

Potentially damaging fallacies

1 'Composite-ness': Many concepts are complex and multidimensional in nature and, therefore, can only be measured by similarly complex and multidimensional indicators – regardless of variation in their internal structures and, hence, the probability that identical scores will be assigned to quite different clusters of variation.

2 'Longevity': It is always better to use an indicator that has been around for some time, used in a variety of research settings, and can provide the researcher with a longer time perspective – despite the likelihood that

during this period the techniques for measurement will have changed and the meanings of items for actors may not be the same.

3 'Clarity': It is always preferable that each variable be given a specific and unambiguous score – even if the nature of its conceptualization and theoretical status is calculatedly 'fuzzy' or 'radial'.

4 'Reification': What you are measuring is identical to what you have conceptualized which, in turn, is identical to the way in which actors perceive 'it' – regardless of how much is lost in translation as the researcher moves from one realm to another.

Test for association

By now, the researcher may have momentarily lost almost all strategic control over his or her project and, at best, should consult one among many texts on methodology to discover which among all of the verbal or mathematical, symbolic or numerical, parametric or non-parametric, deterministic or probabilistic devices available for testing for association best fits the data that he or she has gathered.

Variables can be associated with each other in different ways. Typically, the social scientist will be interested in *direction*, or whether the fit is positive, negative or null; *strength*, or how much one variable affects another; and *significance*, or the likelihood that the fit could simply have been due to chance. Since his or her research will almost inevitably be 'historical', the *time*, *timing* and *sequence* of how they fit to each other should also be important – indeed, these chronological dimensions often provide the basic orientation to how one's findings are presented and defended.

The reason for this is that the most powerful means of testing for the fit among variables and, therefore, for presenting one's findings has long been to tell a believable story in chronological order. Perhaps, within some highly professionalized niches in sociology and political science, storytelling is no longer regarded as acceptable. The occupants of these niches – not infrequently, Americans or those trained in America – have forgotten that their disciplines are profoundly and irrevocably historical. What counts is not just what happens, but when it does and in relation to what else has already happened or is simultaneously happening. Moreover, the actors themselves are not just passive recipients of scores, but active and reflexive keepers of the score. They remember what they and their ancestors did in the past, and their preferences in the present are conditioned by this knowledge. In my opinion, no means of

testing for such associations has yet been invented that can supplant or even surpass the chronological narrative in capturing these subtleties of time and timing, and in bringing simultaneously into focus the multitude of variables involved in the sheer complexity of most social and political phenomena. The narration of your findings can, no doubt, be considerably bolstered in credibility by inserting quantitative tests about specific associations into the basic narrative. Cross-tabulations, rank-orderings, regression equations, factor- or small-space analyses, even mathematical models, can often be helpful, but primarily when analysing topics that are heavily circumscribed in time and space and that can be separated into relative simple and repetitive components.

Even social and political scientists relying exclusively on quantitative data may find it occasionally useful to tell a plausible story that places the associations they calculate and the inferences they draw in some chronological order. Narration can also serve to fill in the gaps between cause and effect by providing a verbal description of the mechanisms involved – especially when mathematical formulae and formal models typically treat such exchanges as taking place within impenetrable 'black boxes' (see Héritier, ch. 4, and Vennesson, ch. 12). The findings of hard-core quantifiers often circulate only among small groups of *cognoscenti* and are incomprehensible to outsiders; but whenever sociologists or political scientists aspire to enlighten and influence wider publics, they will either have to learn how to narrate their findings or hire someone else to translate the esoteric results of their tests into more intelligible stories.

Possibly optimizing choices

1 Never forget the 'inter-ocular impact test' that consists in simply eyeballing the data – scatterplots are especially useful for this – and forming your own visual impression of what is going on among the variables and across the cases.
2 Always try to apply different tests of fit and only try the more demanding ones once you have experimented with simpler ones.
3 If possible (and it will be much more possible with quantitative designs), manipulate the number by eliminating one or two, and/or by dividing the sample into subsamples – say, by size or location – and do not be discouraged if this shakes up their fit, but try to discover what variables may have intervened to produce such different results.
4 Remember that most tests for association – quantitative for sure, qualitative for some – are exceedingly sensitive to extreme cases, so that you may be well advised to eliminate them in order to find out how persistent or

significant is the association among variables when only more 'normal' units are included in the analysis.

5   Remind yourself of the time dimension and test whether successive cross-sections through the data – say, at ten-year intervals – produce equally strong associations. If they do not, reflect on what intervening or contextual variables might be responsible for the new findings.

6   Your tests for association will be all the more convincing, the more effort you put into falsifying initial hypotheses, rather than merely seeming to verify them by grasping at all favourable distributions of data.

7   The treatment of 'deviant' cases that do not fit the general pattern of association is often taken as an indicator of how seriously the researcher accepts the task of falsification. Ignoring them (or transforming their scores) suggests that you are excessively concerned with verification; embrace them, exploit their contrariness and try to determine the extent to which they call into question the hypothesis and you will gain favour as a 'falsificationist'.

Potentially damaging fallacies

1   'Spuriousness': You have found a close association between two variables and you report this finding – without considering that if you were to introduce a third variable, it might explain variation in both of the original ones.

2   'Contingency': The associations you find are strong and significant, but only if and when certain, usually unspecified, contextual variables are present.

3   'Curve-fitting': Since there is always 'noise' and 'error' in the data, it is permissible to 'smooth' distributions by transforming the raw data or eliminating outliers and this will usually result in a more 'satisfactory' fit.

4   'Anachronism': Whatever are the associations that satisfy your test criteria and the time period covered by your research, the findings they generate will be valid whenever.

5   'Ad-hocracy': At some level of abstraction and measurement, each case can be uniquely identified and used to 'explain away' any and all observed deviations from the outcome predicted by those variables included in the study.

Causal inference

This is by far the most hazardous – and the most rewarding – of the stages in the research cycle. It is the one in which you will have the least disciplinary

or academic guidance and, hence, the widest range of discretionary choices to make.

Many social researchers will have exited the process before arriving here. They will have made their accurate observations, published their empirical descriptions and gone home. Others will have stopped even earlier, before having gathered any data, and left satisfied that they have advanced further the plausibility of an argument or helped to specify the universe to which it can be applied. Some will have gone further and proffered tests – numerical and narrative – illustrating how frequently and strongly variables have been associated with each other. But they will have prudently refrained from trying to answer two further questions: (1) the retrospective one of *why* and *how* these variables combined to produce the outcome that was the topic of the research in the first place; and (2) the prospective one of *what* the consequences of this will be in the future and *when* these consequences will happen.

Consider, as an example, the current controversies over climate research. Do you think that if climatologists and other scientists had merely filed reports demonstrating that temperatures were rising across the planet and that various chemical substances have been accumulating in its atmosphere, there would have been much of a reaction? As far as I know, these facts were accepted by all as uncontroversial. It was only when these researchers correlated these indicators and drew the inference that increases in them masked a causal relation that could not be due to chance or fate that things became controversial. When they attributed primary causation to factors related to human intervention and, even more, when they began to advance threatening projections about what will happen in the future, then all hell broke loose!

Without even hinting that all social scientists have a responsibility for generating such controversy, they should feel a more modest responsibility for exploiting their data to the fullest extent possible; that almost inevitably commits them to drawing retrospective and (sometimes) prospective inferences. Just think back to the number of occasions when you have read a report on extensive and expensive research and still found yourself asking the 'why' and 'how' question at the end. This could be regarded as favourable by younger researchers, since it means that there is a very considerable amount of unexploited data out there just waiting for 'secondary analysis' at low cost. Nevertheless, it is lamentable when the scholars who initially chose the topic, conceptualized it, selected the cases and gathered the data do not go as far as they could in drawing 'grounded' inferences about the causality it might reveal. Manuals for sociology and political science are full of sage advice concerning the limits of doing this. Not infrequently, teachers of graduate courses

and dissertation advisors will revel in providing the student with egregious examples of researchers who exceeded the confines of their data or ignored the contribution of other variables, and made what proved to be erroneous statements about causality or consequence.

The controversy that tends to dog most discussions about inference is *generalizability*. A cautious researcher who draws inferences from his or her findings that are restricted to the cases investigated and the time period covered is unlikely to face much criticism – or to generate much attention. Specialists on the topic will, no doubt, have something to say about the validity of indicators, the accuracy of measurements and the appropriateness of tests for association – but it is not until you dare to generalize across temporal, spatial or cultural contexts, until you trample on someone else's turf, that you will be seriously challenged. No one likes to be told that his or her topic can be differently explained by someone intervening from another theoretical or disciplinary perspective.

And there are good reasons for this. Although they may seem arbitrary or anachronistic (and some no doubt are), the lines of specialization built into different social science disciplines have served to enforce professional standards and preside over the accumulation of knowledge. Generalizations that are based on alternative conceptualizations and/or novel methods should be especially carefully scrutinized. Nevertheless, this is where the real scholarly excitement lies – this is where 'seminal' contributions are to be made – provided the researcher is well prepared to face his or her critics.

The strategy of case selection will play an especially significant role. Single-case studies are rarely a convincing basis for generalization – even the so-called 'crucial' ones. Large-*N* studies should be less objectionable, were it not for the fact that many of their cases are dubious in terms of their (alleged) common capacity to act and the probability that behind any associations found in the whole universe there are bound to be subsets of cases where the fit differs considerably – and may even reverse itself. Middle-size samples based on controlling for the 'usual suspects' (geographic location, development, size, religion, cultural area) by their very nature inhibit generalization, unless they are replicated for different samples. Indeed, replication can be a powerful weapon – and not just to the extent that other cases or periods produce the same direction, magnitude and significance of association. If you can show that a reliable pattern holds at different levels of aggregation within the same sample, you will have made the inference that it is more likely to hold elsewhere considerably more compelling.

The other critical factor will come from accusations of researcher bias, often alleged to be the product of the national or disciplinary context in which the

researcher operates. It is only human to prefer to discover what you thought was there in the first place and, then, to extend that finding to other places about which you know less. Most often this can be attributed to a natural tendency to 'over-observe' what you expected to see and to 'under-observe' variation that you were less prepared to encounter. Along with this 'type I confirmation bias', one has to mention that type II errors also exist. In this instance, for some perverse reason, the researcher prefers to reject his or her original hypothesis and, thereby, underestimates the degree of association that actually exists. Whether the peculiarities of national cultures or academic disciplines have anything to do with either of these typical errors seems dubious to me, but there is no doubt that both exist.

The most secure way of guaranteeing enduring respect for the inferences you have drawn from your research – and of securing your place in the Pantheon of Notable Social Scientists – is to place them under the protection of a covering law. Such a law offers an explanation for a much broader range of social or political phenomena, for example the Darwinian 'Law of the Fittest'. It should be widely, if not universally, accepted by the Notables who have preceded you and, ideally, it should not be derived from the theory you started with. But do not worry if you do not make it to the Pantheon. Your contribution to knowledge can still be significant and your career as a social scientist still very rewarding.

Possibly optimizing choices

1 Add alternative explanatory variables suggested by other cases or experiences (if available without conducting an entirely new piece of research) to discover whether the original fit within your sample is maintained.

2 Probe your data by subtracting subsets of cases from the initial sample to see how robust the findings based on it were, especially when you think you are dealing with the entire universe but have reason to suspect 'regional' variations.

3 Be careful not to 'anchor' your inferences by relying too much on a single prominent association among variables at the expense of lesser (and less expected) ones.

4 When assembling a batch of inferences from a research project, do not privilege or attach greater significance to findings that were easier to document or closer to your own experience.

5 It will be risky, but try on the basis of your inferences from a given sample to predict what analogous behaviours have been in a different sample of

persons or places that you know nothing about; and (even more risky) to apply the inferences you have drawn to predicting the future performance of the units you have studied.

Potentially damaging fallacies

1 'Triumphalism': You have made a significant finding; therefore, your work is over – even though it could be the result of some variable you forgot to include and that may be very prominent in other cases or samples.

2 'Pago-Pago-ism': Whenever you think you have found something that applies everywhere, there will always be some place that you do not know (or have not even heard of) where the finding does not fit – and there will always be a scholar who knows the place and will inform you of your error.

3 'Exceptionalism': You chose to study a particular topic only in a particular country because you considered that the context was exceptional and, then, you turn around and claim that your findings are universal.

4 'Cross-level replicability': Associations among variables that have been found to be consistent in direction, strong in magnitude and significant at one level of analysis will replicate themselves at other – lower or higher – levels of aggregation within the same sample.

5 'Cognitive dissidence': If variables that simply 'should' not go together still seem to be associated, this must be due either to some unidentified measurement error or conceptual confusion, so that you are justified when drawing any inference by excluding the case or withdrawing the variable from your analysis.

6 'Temporal proximity': You choose to give greater prominence and to attach greater importance to associations of variables that have occurred more recently and to presume that earlier associations (or dissociations) should be 'discounted'.

Self-assessment

Once you have arrived at whatever stage in the research cycle you have chosen as your point of exit, your objective should be quite simple: make yourself into the best possible critic of your own work. Anticipate all of the potential objections at each of the previous stages. Where possible, return and enter appropriate corrections. Since this is often impossible, given the numerous and irreversible 'path dependencies' built into the research cycle, signal to your

reader-cum-critic that you are aware of the defect and have tried your best not to be misled by it or to magnify its impact. Above all, remind yourself right from the start that no research is perfect and all researchers make mistakes. Inscribe above your desk (or on your screen saver) the Latin phrase *Errare est humanum* – 'to make mistakes is to be human'– and recognize that to be a human being studying human behaviour is to be doubly vulnerable to this maxim.

My overarching purpose in writing this chapter has been to help you to become your own best critic.

Conclusion

Social and political research is characterized by the diversity of its concepts, theories, designs – and logics. Only a few will work 'around the clock' in Figure 14.1 and conclude with empirically grounded inferences about causal relations among variables. Many will choose a topic for which this would be premature or inappropriate, given the existing state of his or her discipline or his or her purpose in selecting a particular topic. They may exit the cycle relatively early, sometime between 1 and 3 p.m. – hopefully, with an improved understanding of the generic relations involved and, possibly, with a more elaborate set of hypotheses for future research. Still others will be interested in drawing out the ethical and normative implications of these relationships, perhaps by exploring analogies with previous experiences or prior philosophic assumptions. In Figure 14.3, I have labelled this point of exit as the 'logic of discovery', the idea being that those who take it will have made their original contribution by discovering empirical or normative relationships previously ignored or distorted by existing wisdom. The chapters in this volume by Zoe Bray, Alessandro Pizzorno, Sven Steinmo and Rainer Bauböck should be especially useful to those who choose to leave the cycle at this point.

From 3 to 6 p.m. fewer social and political researchers will be leaving the cycle.[1] Their distinctive contribution will have been to identify the apposite universe surrounding the topics selected, to select cases that represent specified distributions of key variables and to have invented new ways of defining these variables and embedding them in more comprehensive theories. Most importantly, they will have carried further and in greater detail the existing conceptualization of the relationships surrounding their topic – hence, the notion that they have followed a 'logic of explication'. Donatella della Porta, Peter Mair, Christine Chwaszcza and Friedrich Kratochwil have
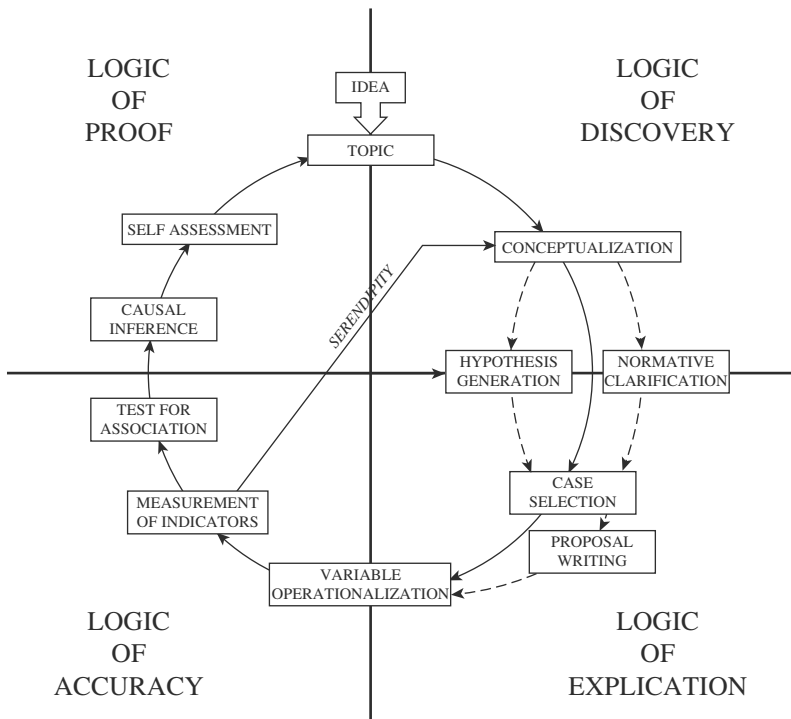
Fig. 14.3   The cycle of social and political research and its four logics

contributed chapters that should be of particular interest to them, but all of them presuppose that explication is not an end in itself, but only a necessary pre-condition for passing to the next stage which involves the specification of indicators and the gathering of data.

Many more social and political scientists will exit after 6 p.m. and before 9 p.m. They will have produced research that is fundamentally descriptive in nature. Here, the preoccupation is with the validity of their measurements and the accuracy of their observations. They will have gone into the field – even if it is in their own backyard – and generated new data about social and political phenomena. They are also most likely to have contributed to the development of better instruments of observation and more reliable indicators. Mark Franklin's chapter deals primarily with this 'logic of description'.

The chapter by Adrienne Héritier is the one that comes closest to tackling head-on the issues involved in the 'logic of proof', although virtually all of the authors touch at least peripherally on the very controversial objective of making empirically grounded inferences about causal relations in the political and social sciences. This may be where the ultimate payoff lies – and certainly

where the highest disciplinary status is usually awarded – but only a select few make it to this stage in the cycle, and even their conclusions are always contingent upon eventual replication by other scholars.

The reader should not be discouraged by this. To do original research on a topic about which you care is an adventure. It can take you in different directions and end in different places. A lot will depend on your point of departure, but you will also be influenced at every turn by your professors and peers – not to mention the fads and fashions of your discipline. The most important thing is to be conscious and confident of the choices you will be making, and then to know when and where to exit from the cycle. Hopefully, this and the other chapters in this volume will help you to make the voyage easier and, ultimately, more rewarding.

NOTE

1 Unfortunately, many of them will be so-called ABDs (all but dissertations) who come up with a design for a previously conceptualized piece of research, could have written a proposal and may even have given some thought to operationalizing its variables, but never managed to actually find the time, resources or energy to gather the relevant data – much less to write them up.