World Political Science Review

Volume 4, *Issue* 1 2008 *Article* 5

Political Science: Witchcraft or Craftsmanship? Standards for Good Research

Asbjørn S. Nørgaard*

*Centre for Welfare State Studies, Department of Political Science, University of Southern Denmark and Politica, Denmark, ano@sam.sdu.dk

Originally published as Nørgaard, Asbjørn Sonne. 2007. "God statskundskab: - heksekunst eller håndv?", Politica 39(3):233-241. Reprinted with permission from Politica.

Produced by The Berkeley Electronic Press.

Political Science: Witchcraft or Craftsmanship? Standards for Good Research

Asbjørn S. Nørgaard

Abstract

Scientific debate requires a common understanding of what constitutes good research. The purpose of this article is to establish such an understanding. The purpose of political science is to uncover, understand and explain the conformist aspect of social behavior, well aware that not all behavior is systematically determined by society. Good political science ought to be grounded in two questions: What do we know, and what are we going to learn? Research question and theory are decisive, while all discussion about methodology and design is about subjecting our prejudices and expectations to the most difficult test possible. The binary opposites we are familiar with from the 'Methodenstreit' of the social sciences are unproductive.

KEYWORDS: political science, social science, research

> Sometimes the verbal substitutions masquerading as contributions to knowledge are so inept and gross that it is difficult to believe that the authors really think that they are revealing new truths (which must be the case), and that they are not laughing up their sleeves at the gullibility of their audience (Andreski, 1974: 64).

Much mediocre research is carried out in political science and the other social sciences. Fortunately, however, there is also quite a bit of good research. Perhaps the latter category is almost as large as the former? This article will not present evidence for such empirical claims; they remain postulates. Rather, the article will attempt to clarify what constitutes good political science.

Is it even possible to establish common standards for good political science? Do such decisions not merely become a positioning of certain perspectives on the philosophy of science, ontologies, and epistemologies (cf. Marsh and Furlong, 2002)? These questions relate to the standard debate for and against inductive and deductive research strategies; for and against quantitative analysis techniques in relation to qualitative techniques; depth vs. breadth; interpretation vs. explanation; the ideographic ideal vs. the nomothetic idea; for and against case studies; in other words, the unending and impossible *Methodenstreit* in one of its numerous manifestations. Via a discussion of these opposites, the aim of this article is to establish a capacious yet precise definition of what constitutes good political science. A shared understanding of the criteria for good science is a conceptual grammar that enables researchers to engage in productive theoretical debate and settle empirical disagreements, thereby bringing research forward. The costs of a "dichotomous methodological vision", in particular the seclusion of research debates, are far too great (Gerring and McDermott, 2007: 698).

This article claims that a discussion of what constitutes good political science does not necessarily need to begin or end in the vicinity of the red-hot debates concerning philosophy of science or methodology. The point of departure for good research requires addressing two questions: what do we know, and what are we going to learn? By implication research is always driven by puzzles and theory, and meta-theoretical disagreement does not stand in the way for genuine theoretical and empirical debate. Research then becomes about testing

Thanks to Ronald F. King, Thomas Pallesen, Lotte Bøgh Andersen and Robert Klemmensen for endless inspiring and clarifying discussions about method and for the fact that you also find great intellectual stimulation in modelling and playing out all sorts of arguments. Thanks to Casper Sylvest for challenging the ideal advocated in the article. Also thanks to the roughly 60 PhD students who participated in the craft courses that Ron, Thomas and I have held over the last 6-7 years. Without your involvement in real world subjects and theory, the discussion of research design would have been meaningless. The flaws and ambiguities that might still be found in the article are certainly not your fault.

prejudices, conceptions and hypotheses as rigorously as possible. Only then can we become as certain as possible that there is something new to learn. The perspective further implies that craftsmanship and methodological pluralism are the cornerstones of a prolific political science research community.

The article begins with a positive determination of what constitutes good political science and what a social science approach entails. This determination is perfectly ordinary; some would say banal. The next section argues that social scientists should focus on uncovering and investigating causal relationships in a broad and minimalistic sense and that interpretation and theorizing constitute the foundation of a political science that wants to learn and progress. The third main section investigates the implications of the preceding arguments for a number of the binary opposites dominating the *Methodenstreit* of political science and the social sciences in general. The point of departure is the two questions that ought to be posed in all good research; what do we know, and what do we have to learn? The answers to these questions determine the most appropriate research design. In conclusion, the article claims that the understanding of good political science advocated here also for normative reasons ought to be able to attract many followers.

1. Good political science *is* …

Good political science is defined in terms of method, not object of study. The object of political science is difficult to define. Despite whole-hearted attempts at defining the state, power, or politics as the privileged object of analysis which constitute the field of study in political science, there is little consensus about these central concepts amongst the practitioners within the discipline. Besides, other disciplines also address these issues. In reality, practitioners of political science cannot deal with all themes and questions simultaneously, and it may be possible to reconstruct the essence of political science by analyzing the history of the discipline (cf. Almond, 1996). That is not the purpose of this article; and regardless of the definition of the field of investigation, it offers little help in clarifying what constitutes good political science. For the same reason, however, the theme is not really good *political science*. This argument applies to the social sciences in general.

More than 150 years ago, Karl Marx claimed that mankind writes its own history, but does not do so under conditions of its own making: "circumstances make men just as much as men make circumstances" (*German Ideology* 1845-1846 in Bottomore, 1964: 55). We can easily subscribe to this claim – or axiom – without subscribing to the entire Marxist package about the dialectic between *Verhalten* and *Velhältnisse*. The insight is as deep as it is banal, and it touches upon the one of the central problems in social sciences: the relationship between

social action (voluntarism) and social structures (determinism), i.e. the structure-agency problem. We usually place Karl Marx and Emile Dürkheim on one side and Max Weber on the other when we demarcate the materialist and institutional collectivist/structuralist positions in contrast to methodological individualism. Over the years, many attempts have been made to overcome this dualism (e.g. Laclau and Mouffe, 1985; Giddens, 1984). The theories – or, rather, approaches – can be difficult to grasp theoretically, and they create just as many problems as they solve as regards the structure-agency relationship. The most certain conclusion is to maintain the position that both actors and structures have significance; with shifting weight, depending on what we are studying.

Human action can be perceived partly as acts of will and creativity, partly as a result of structurally conditioned limitations and incentives. Whether the structural incentives are socio-economic, institutional, cultural, discursive or something entirely different is not decisive here. How much of a *specific* type of behavior is decided in a *given context* at a *given point in time* by creative acts of will or different structural incentives can never be determined *a priori*. If we were to defy this common sense ontological point of departure and assert the opposite, we should turn towards philosophy, theology or another discipline in which metaphysical arguments can be unfolded fully.

The social sciences cannot work with anything that is genuinely creative. All good social science is about establishing systematic – in the societal or structural sense – understandings and explanations of human behavior. This is the case for the descriptive analysis of local discourses about problem perceptions of lacking commercial development; the small local study of school closings in Anytown; or when election research concludes that education and income have an impact on opinion formation and party choice. We can have different views about the extent to which the results of the analyses can be generalized in time and space and whether generalization is even worth striving for – more on this below. But the social sciences cannot capture or deal with anything unique, i.e. the genuinely creative. In that sense, the social sciences are boring. The social sciences always attempt to demonstrate that the actions of the individual can be understood and explained by a more or less proximate social context and that social, political, institutional, cultural, or economic conditions somehow matter for individual behavior.

In some contexts, the political scientist will encounter incredible difficulty establishing a general social understanding of behavior. For example, a prime minister with serious mental problems, whose mind is on something other than votes, future government power or political objectives. The social sciences cannot explain everything, even though we as social scientists can be tempted to engage in post hoc rationalization and claim the ability to understand even the most bizarre behavior based on various contextual societal conditions. Political science

is not plagued by an inferiority complex, and media appearances in which political scientists dole out knowledge with limited – if any – scientific basis are commonplace (Albæk et al., 2002).

Regardless of the magisterial ambitions of political science, people are rarely as creative or choose as freely as we give ourselves credit for. In the words of Professor Ronald F. King, "individuals choose separately but live with considerable conformity" (King, 2006: ch. 3, p. 27). Just exactly how conformist we live is an empirical question; but not only.

In political science, we seek some level of general understanding and explanation of concrete societal phenomena. There are no theories for and therefore no understanding of truly unique phenomena. In a concrete sense, two separate phenomena are always unique, if nothing else than in time and space (cf. Gerring, 2005: 185). As Wittgenstein already pointed out (Buckler, 2002: 176f-78), however, we cannot understand, describe or determine the unique. It is only in comparison with an expected regularity in action – a pattern – that we can see and articulate the unique. This has two consequences.

First, we must use words, categories, and concepts to describe which aspects of specific actions we are studying. 'Reality' itself does not tell stories or how it is to be classified or understood; our theories, theoretical concepts and normative interests do so. All phenomena can be studied in a number of ways. However, even the most inductive analysis must determine what it will observe, how it will classify with respect to which purpose. Anything else is nonsense. Myrdal calls this radical inductive position "naive empiricism", David Easton calls it "crude empiricism", while Danish Erik Rasmussen merely says that "it is not possible to allow data to speak for itself" (Rasmussen, 1971: 17, own translation). Research will always focus only on some aspects of an empirical field of study. Even "scientific description is not a simple linguistic or other symbolic identification of an observation" (ibid.: 19), but something requiring clear concepts and criteria for classification and grouping. Classification is a "kind of measurement. A phenomenon is described in terms of being more or less equal with the phenomena *a, b, c … n*." (ibid.: 20).

Only in a certain light are different – in the sense of distinct – phenomena identical or different. No claim about anything being particular, special or unique ought to be taken seriously unless the researcher has clearly defined and demarcated his concepts. If a classification is not correct and meticulous, neither the general and typical nor the specific and atypical can be identified. Most political science analyses that in the introduction and conclusion claim that a problem is very complicated have simply not focused their research interest and defined their concepts well enough. Research that employs unclear concepts and an interest in "wholes" that are not properly classified, usually leads

to poor analyses and vague, complex conclusions, because the research problem has not been thought out clearly to begin with.

Second, we must be aware that there will always be a set of boundary conditions for any and every pattern of behavior. This is the case, regardless of whether we have merely described a pattern without claiming anything about cause and effect. Every relationship, configuration, and pattern is temporary and conditional. Any claim to the contrary would make it necessary to allege the existence of laws akin to the natural sciences in the social sciences. Methodologically, taking Karl Popper in hand, we know that such a perspective is impossible to maintain (cf. King, 2006: ch. 2, pp. 22-24). We can never be 100 percent certain that a relationship and a pattern are true; and by no means forever.

However, the most important argument against the notion of law-like generalizations in the social sciences is our fundamental assumption that humans have their own will and in principle could choose differently; and sometimes do. All of the patterns and relationships we have established in the social sciences will therefore be temporary, probabilistic and conditioned by a number of acknowledged and unacknowledged factors. The objective for the social sciences is that as much as possible can be acknowledged; well aware that these efforts will never succeed. We should rarely place much faith in any analysis claiming to be able to explain everything – an R^2 of 100 or close to it. The perspective here is the same, as Stanislav Andreski asserts:

> Luckily, to pursue our studies we do not need to accept the doctrine of universal determinism. It suffices if we assume that many phenomena can be causally explained, that not all possible causal explanations are known, and that it is possible to discover new ones … (I)ndeterminism can be restated as the belief (which I personally hold) that mortals will never reach a stage when their knowledge will be complete and there will be nothing left to discover (Andreski, 1974: 22).

Much follows from this perspective. In this context the most controversial issue is Andreski's implicit assertion that the objective of the social sciences is to formulate causal propositions and to test their validity and probability. However, his statement is not as controversial as it might seem at a first glance. The greatest disagreement in social *cum* political science is about how easy or difficult it is to establish causal explanations and how theoretically and methodologically well equipped political science is with respect to demonstrating that a conjectured cause and effect relationship is theoretically sound and empirically plausible.

2. Causality or description – explanation or understanding?

As people in a modern, thoroughly regulated society, we cannot live together unless our social, political and economic interactions are largely predictable and can be influenced by known factors. Think about what would happen if the vast majority of motorists did not choose to follow the same rules and norms. How impossible teaching and education would be if the students' behavior could not be regulated and manipulated in a reasonably predictable manner. How inflation could totally spin out of control if households and companies did not consider interest rate levels when making financial decisions. Much behavior in modern society is predictable because we can understand and explain it.

What constitutes an explanation and what kinds of explanations are possible have been intensely debated since the days of Aristotle (King et al., 1994: 75-76, 85-91; Brady, 2004: 56-58; Gerring, 2005: 163-165; King, 2006: ch. 4). Distinctions have occasionally been made between functional, intentional and causal explanations, and some also talk about descriptive or classificatory explanations (e.g. Brady, 2004). Various typologies of causal explanations have also been developed. For example, distinctions are made between triggering/proximate and background/distant causes, necessary and sufficient causes, deterministic and non-deterministic causes, etc. A lengthy discussion of all these distinctions and their possible merit is not possible in this context. The contention here is (a) that with a sufficiently capacious definition of causality, we can see both intentional and functional explanations as a subset of causal explanations and (b) that classification and interpretation are necessary first steps in any analysis that makes a cause and effect argument and wants to demonstrate its plausibility.

2.1 Causal explanations

Following John Gerring a minimal definition of causality is that "causes may be said to refer to events or conditions that *raise the probability* of some outcome occurring (under ceteris paribus conditions)" (2005: 169). This definition can contain intentional/motivational explanations or "explanatory understanding" in the Weberian sense (cf. Weber, 1993: 29-55) as well as (in any case, certain versions of) functional explanations, regardless of whether we for other reasons reject functional explanations (cf. discussion in Sørensen, 1991: 25-28). Even such a minimalistic definition has several implications if we are to argue with plausibility that the causes in question "*generate*, *create* or *produce* the supposed effect" (Gerring, 2005: 170; emphasis in original): (a) we must clearly be able to distinguish between the two phenomena: cause and effect; (b) we must be able to determine what is cause and what is effect; (c) we must be able to describe the counterfactual situation – what would have happened if the cause had not been present; and (d) we must be able to compare what happened with what is

happening (or would have happened), if the cause is (was) not present. There is an extensive methodological jargon for these requirements, but it is not necessary to use jargon to make the argument.

The first requirement is about the need for precise concepts, categories, and indicators of what we are studying. Reality itself does not tell us how it is to be studied, and it cannot be described in its full complexity, but with the help of categories, which we for one reason or another find important. This point ought to be clear by now. Firstly, the requirement about being able to distinguish between cause and effect demands that we differentiate and describe various characteristics about an object of analysis. We can refer to these characteristics as variables, as we customarily do in the social sciences, but this designation is rather unimportant. The characteristics can be referred to as dimensions, attributes, factors or something completely different. Any descriptive typology acknowledges the need to be able to distinguish and describe characteristics systematically – gender is distinct from education, which is distinct from job and income. Some categorizations are qualitative and dichotomous (e.g. gender), others are qualitative but involve multiple categories (e.g. job types as white-collar employee, laborer or self-employed). Others can be ranked (low, medium or high education), while others yet can be set in interval scales (e.g. salary in euro). Taking his point of departure in the conceptual grammar guiding the research it is the researcher who classifies phenomena and characteristics as different and distinct. Description and interpretation is thus a part of and simultaneously a requirement for being able to establish causality; without interpretation and description, no causality. The opposite is logically not the case.

The next requirement is about how we see one phenomenon as causing another. It is not possible to observe causality. It is possible to observe covariation between two characteristics, and it is possible to make the direction of causality probable, e.g. when one phenomenon appears after another (e.g. as a rule, formal education comes before employment, and gender before education), but we must still have a well-reasoned argument – a theory – in order to claim that one phenomenon is the cause of another in the sense indicated by the definition above (cf. King, 2006: Chapter 4). Even the time sequence cannot always determine the direction of causality. In political science, we often talk about "anticipated reaction", i.e. we expect a specific reaction at a later point in time if we act in a specific manner. With experience and strategic insight, political actors try to predict other people's reaction and adjust their own behavior accordingly. The cause for the behavior thus comes after the action (Gerring, 2005: 175). But 'the expectation' – right or wrong – is present in the strategic considerations prior to the action and can very well be a cause for a certain behavior. We must always have a good argument for being able to claim that something is the cause of something else.

The third requirement fits closely together with the second requirement and also demands clear concepts and categories, but in addition it implies that we must be able to put words and concepts on what will happen if the cause is not present. In other words, we must create a theoretically meaningful categorization of the effect which can reasonably be impacted by a given cause. Much research fails to meet this requirement and lacks a clear conception about the counterfactual situation; that *something* has significance for *something else* is casual chit-chat devoid of precise meaning, not a causal claim worth debating and analyzing. Without a specific statement of the counterfactual situation, it is difficult to disagree that a strong Social Democratic Party (somehow) has had significance for the development of the welfare state. However, the statement is meaningless if we fail to describe the sense in which the Social Democratic Party has had significance: Is it in relation to the rising social spending, specific types of benefits, the level of redistribution or some entirely different aspect of the welfare state? Similarly, what constitutes a strong party? Is the party organization, relations to the trade union movement, the number of seats in parliament, or years in government decisive for the strength of the Social Democratic Party? In other words, the causal claim suggests that with a certain probability the absence of a strong Social Democratic Party (however defined) would have resulted in another development of the welfare state. When we describe the counterfactual situation, we simultaneously enter the realm of theory and the specification of causal mechanisms – why and how does a cause trigger a specific effect?

The fourth requirement concerns operationalization, evidence and falsification and suggests that we must be able to determine whether the relationship between cause and effect exists. Did the expected factual and counterfactual situations occur? The problem is that we cannot tell for sure. Because of "the fundamental problem with drawing causal conclusions", we cannot unambiguously determine a causal effect, as it is never possible to observe both a factual and counterfactual situation in exactly the same situation (King et al., 1994: 79-80; Brady, 2004: 79-81). A person cannot eat an apple and not eat it at the same time. With experiments, we can come closest to determining the causal effect (cf. Serritzlew, 2007), but many research questions in political science cannot be analyzed with the help of experiments (Lijphart, 1971; Frendreis, 1983: 256-58). In other words, less certain assessments and estimates of the causal effects have to suffice. To be able to make a qualified estimate of a causal effect the causal conditions must vary, at least hypothetically. A hypothetical variation can be entirely reasonable if the theory about the counterfactual situation is quite strong. We cannot draw any causal inference from a constant. Nevertheless, there are an endless number of studies that dare to do so.

Just think of the many analyses of Scandinavia that refer to the significance of the historical background for the development of the Scandinavian

model. Without a non-Scandinavian country to compare with, however, how can we then produce an estimate of the causal effect of the historical background? Or what about the vast array of studies of the US Congress and *The Logic of Congressional Action* (Arnold, 1990)? Nobody studying only Congress can tell whether there is a unique or even distinctive logic governing congressional action or whether the political strategies and processes so meticulously analyzed are general to a broader array of legislatures. Even purely descriptive studies have to compare with something else in order to determine what is distinctive. For instance, it goes without saying that one needs to have at least two observation points in order to be able to say anything about change. However, the mortal sin in relation to a lack of variation is bigger when making statements about cause and effect: "All empirical evidence of causal relationships is *co-variational* in nature … Conversely, the absence of such covariation is taken as disconfirming evidence. If all appearance and disappearance (waxing/waning et al.) of *X* and *Y* are *not* associated in any way that can be rationally explained, and hence predicted (or postdicted), then the empirical evidence suggests that a causal relationship does not exist (Gerring, 2004: 342; cf. also Gerring and McDermott, 2007).

All four requirements are about being able to theoretically specify and empirically test a conjectured cause and effect relationship. In particular, one must be able to tackle the counter-argument that the claimed causality does not exist, and that an observed covariation is spurious. Well aware that not all conceivable alternative explanations can be taken into account, we must to the highest extent possible control for other plausible causes. This is the case for large quantitative analyses and small qualitative studies alike, although the way we control for alternative explanations differs (cf. King et al., 1994; see also Lijphart, 1971; Frendreis, 1983).

2.2 Causal explanations and/or interpretive analyses

With the minimalistic definition of causality discussed above, most researchers would accept the claim that a number of phenomena in modern society are steered by partially known cause and effect relationships. In the words of Ron King, we are to some extent conforming to rules, habits, incentives and the like. Obviously, then, the question becomes partly how much behavior is conformist and steered by systematic causal factors and partly how easy or difficult it is for political science to comprehend and demonstrate these causal relations. The fact that in the social sciences the objects of investigation are themselves thinking and occasionally creative people, who are capable of learning and changing their behavior in light of new knowledge, only makes the challenge greater; that which was correct yesterday can be wrong tomorrow.

We can easily sympathize with the understanding of good political science presented in this article and agree that the forwarding of robust causal explanations and theories is our ambition while at the same time subscribing to the conviction that few and primarily trivial causal explanations are valid for very long and then only under restrictive conditions. Moreover, we can also hold the belief that political science has only laid bare a minority of these causal relations. The level of knowledge in political science can only be determined concretely on the basis of a given research question and the specific theoretical literature with which our own research is in dialogue (King, 2006: Chapter 1). In some research areas, e.g. voting or party behavior, the causal theories are so well documented, the conceptual grammar so well developed, and the understanding so profound that it hardly seems productive to start *de novo* with deep single-case studies with the purpose of describing and understanding a phenomenon. In other fields of research, the level of knowledge is far lower. This could be the question of determining which type of social interventions for socially disadvantaged children that provide the most positive conditions for child development. If our level of theoretical and empirical knowledge is low, it may be more productive to carry out a few explorative case studies aimed at describing and understanding social action than to elucidate and test causal theses that rest on shaky foundations.

The relative merit of deeply textured descriptive studies versus broad causal theory testing analyses cannot and should not be settled with reference to differences in ontology or epistemology. It is far too important to be determined *a priori* with reference to a certain *Weltanschauen*. Rather, the question of research design must be decided pragmatically and concretely, and it can only be answered when we have found out what we already know about the issue at hand. Good interpretations, credible descriptions and the development of solid concepts are necessary to be able to take the next step, which is to make coherent causal arguments and develop causal propositions and ultimately test their empirical implications. In many research areas, however, political science is not yet capable of taking the next step.

Causal explanations require interpretation and description; especially the subset of causal explanations concerning intentional and strategic action. Put bluntly, we cannot venture causal explanations of political and social behavior before carrying out solid discourse analyses and thick descriptions. As Max Weber advocates, we must understand human behavior and the subjective meaning actors ascribe to it in order to proceed and account for the causes of a specific type of behavior, its course and effects (Weber, 1993 [1925]: 29-36).

Rational choice assumes certain motives and ascribes certain preferences for individuals in comparable roles, and then contextual inducements – typically conceptualized as various institutional incentives or game situations – are decisive for the behavior. In such deductive systems, the conclusion ultimately equates

with the assumptions. Therefore, the assumptions must be defended and justified and ultimately tested as any other descriptive statement (cf. Tsebelis, 1990: ch. 2). The assumptions about vote-maximizing politicians (Downs, 1957) or budget-maximizing top bureaucrats (Niskanen, 1971) must be justified and substantiated.

Interpretation is part of the entire research process. The objects of study in the social sciences are thinking subjects themselves, but the researcher chooses his own perspective, constructs his own categories and organizes his analyses. Just like the dismal assumptions in rational choice have to be tested, no statement made by well-intentioned bureaucrats or idealistic politicians explaining their own motives can be taken at face value. Similarly, any professor's statements about his own excellence also ought to be scrutinized. Institutional roles, structural incentives, or shady personal motives can be important determinants of behavior, even if one personally does not fully acknowledge it and in any case would never admit it.

3. What do we know, and what are we going to learn?

There is no single formula for carrying out good social science. A single best design or a superior method does not exist, as opposed to what we often read (e.g. Lijphart, 1971; King et al., 1994). Some methods, e.g. experiments, give the researcher better opportunities for testing and drawing causal inference than others, e.g. a single case study. But our knowledge is not always at a level where it is reasonable to propose and test causal theories, let alone test them using experiments. The question about analytical approach, research design and method is determined by the research question, which on the other hand is inspired and bounded by the researcher's conceptualization and existing theoretical and empirical knowledge (Rogowski, 2004). Basically, all research ought to begin by raising two questions: what do we know, and what are we going to learn? Only then can we decide how, in the best possible way, we can acquire knowledge about our research question.

Our existing knowledge is always provisional and more or less uncertain; not least our knowledge about causal relationships. Nevertheless, our work should always be based on what we (think) we know. Partly because we always study a subject in a certain theoretical perspective, and if the concepts and definitions we are working with are sufficiently clear, there are always people who have worked with similar issues; partly because to figure out whether we have anything new to say we have to build upon the existing knowledge and engage the literature in the field. One cannot study everything about anything. It is not possible to contribute to research without knowing and relating to it.

The social sciences are inevitably theoretical because the ambition is to acquire systematic knowledge. Theory is key. As far as I know, nobody has

studied the social and political identities of Latin-Americans living on 10th Street between Avenue B and D in New York City after the turn of the millennium (a place I like very much because I lived there in the early 1990s). But the construction of political identities has been studied before. The issue of race and ethnicity in American politics has been analyzed in literally thousands of books and articles. The issue of inner-city problems in poor neighborhoods is not new either. Nor is grass root mobilization, social mobility, or the consequences of gentrification. The question any social scientist must ask is what is studying Latin-Americans living in the East Village going to teach us that we did not already know?

Issue and theory determine which aspects of a given phenomenon we are interested in, and indeed whether it is worth studying. The theory situates the study and commits the research. As opposed to what some political scientists, many historians and even more anthropologists believe, one cannot study an empirical phenomenon 'as it is'; no matter how deep we dig and how dispassionate we are.

3.1 Induction or deduction – empiricism or theory?

Many researchers and an even greater number of students proclaim that their interest is empirical and not theoretical. As a rule, this is nonsensical, and in any case it is unclear what they mean. If we are talking about abstract and formal models when thinking about theory, then the statement may make sense. But we can never study a social phenomenon without a theoretical perspective emphasizing only some aspects and ignoring others.

The slightly more sophisticated distinction between inductive and deductive analysis does not necessarily make more sense. Pure induction in the sense "to draw inference solely on the basis of experience" is impossible, as reality does not tell how it wants to be interpreted. Pure deduction without experience-based analysis only serves "to clarify the content of a statement" (Rasmussen, 1971: 22); it cannot provide the basis for formulating synthetic statements and empirical theses.

When Clifford Geertz claims that the purpose of anthropology is to provide "thick descriptions" of cultures and sort out the socially established structures of signification informing social action and interaction (1973: 24-28), he is advocating an inductive ideal which, according to this article, is not possible – or at least not productive. Geertz acknowledges that theories, concepts and past analyses have an impact on his own analysis and construction of the structures of meaning of other people. However, our understanding of cultures primarily improves and becomes more precise when the researcher delves deeper into the local culture being studied: "(T)he essential task of theory building here is not to codify abstract regularities but to make thick description possible, not to

generalize across cases but to generalize within them. (…) In ethnography, the office of theory is to provide a vocabulary in which what symbolic action has to say about itself … can be expressed." (1973: 26, 27). According to Geertz, the best scientific interpretations are those that are considered useful and survive intellectually. There are at least two problems with this perspective on the social sciences (anthropology).

First, symbolic actions cannot state anything about themselves. Regardless of how long we study a social phenomenon, we will only uncover certain aspects and not others. We cannot escape making theoretical and conceptual choices incessantly, and of course we should be as explicit and open about these choices as possible. The scientific debate and clarity suffer if these choices are unconscious, concealed or implicit. It is unclear what it means to delve deeper into the local culture and also what the purpose is. The perspective appears to be that the deeper we analyze, the clearer it becomes what the local culture really 'is'. The argument has a positivist tinge – reality presents itself to the researcher if we look hard enough. What does it mean that an understanding becomes more precise if we do not measure it against some scale or standard of our own choosing? This criticism can also be directed towards a number of versions of "grounded theory", which argue that we must avoid theoretical studies before beginning empirical analysis, as theory infects the analysis. Instead, via the coding of the material, we must discover concepts and contexts (Glaser, 1998). For some, this ideal may sound commendable, but in fact it is an impossible task that masks the process that precedes and accompanies the empirical analysis. Theory is with us all the way.

Second, the view on theory as an instrument for thick description implies that it is not possible to construct general categories and formulate broad theses, let alone causal theses. But how can we know how much local cultures and discourses mean without investigating them and without comparing across them? It is one thing to have a thesis about local structures of meaning being decisive for social interaction. Without comparative analyses and general concepts, it is not possible to figure out whether this proposition is valid. Geertz acknowledges that culture is one among many phenomena that can form and inform human action; but how is it determined whether it is more important and "will stand out against the other determinants of human behavior" (Geertz, 1973: 27)? To analyze the competing determinants of behavior they have to be conceptualized:

> It is false to believe that, if you compile enough facts, fundamental truths will jump up and seize you. All you would produce is a long list of unconnected sentences, useful at best for memorization skills but certainly not for understanding fundamentally how things

> function. Facts without theories are silent, just as theories without facts are empty (King, 2006: ch. 2, p. 31).

Deep, qualitative case studies are a well suited design for a number of research questions. The criticism of extreme induction and theoretically unguided interpretation is not an argument against case studies. In cases where the theory is weak we cannot do without explorative case studies which can help us to produce new theses (Gerring, 2004: 349-50). In many situations, new descriptions and classifications clearly drive the research forward. But this depends greatly on what is added and what we can learn from the new descriptions. An example illustrates this point.

More or less all comparative analyses of the Danish public sector emphasize its high level of decentralization (e.g. Cerniglia, 2003; Stegarescu, 2006). Local government is responsible for most welfare state services. They organize the provision of welfare services and levy a large share of the taxes needed to finance them. Carrying out yet another analysis making the claim that Denmark has one of the most decentralized public sectors in the world of unitary states does not deserve financing. However, an in-depth, descriptive analysis showing that the public sector is actually highly centralized and has been so for a long time could be extremely interesting and, if convincing, add to our knowledge not only of the character of the Danish public sector but perhaps also new forms of central regulation. Such a descriptive analysis would require considerable theoretical work on concepts such as decentralization, local autonomy, and central regulation followed by careful data selection and systematic comparative analysis. Otherwise the skeptics would, probably, remain unconvinced.

The ambition of political science is to formulate and study causal propositions. Still, a well-conceived descriptive study can be far more interesting and bring research much further than a mediocre analysis testing shaky causal explanations. Research is a collective venture. No individual researcher ever lays the final brick in the common building. Well-designed descriptive analyses and causal tests are both important blocks in building theory, that is, in acquiring knowledge about the systematic component of social and political life.

3.2 Theory and design – yin and yang

At this point in time it hardly comes as a surprise that following the argument in this article there is no single best design that *sub specie æternitatis* is superior. This is not an argument for the fact that *anything goes*. Methodological awareness and stringent analysis are prerequisites for producing good political science. The choice of design depends on the research question and our existing knowledge. Nevertheless, as political science/the social sciences are fundamentally interested in understanding and explaining the systematic component in social actions we

can point out a number of general rules of thumb for how we ought to design our research. Different types of questions call for different designs. In the work at hand, there is only space to discuss three central points regarding the relationship between theory and design.

- Variation is necessary to be able to identify a pattern, which in turn is a requirement for all empirically oriented theorizing.
- Large N-studies are normally preferable if we want to determine with the greatest certainty possible that a cause has a claimed effect and estimate the form of the effect.
- Comparative case studies are normally preferable if we want to investigate causal mechanisms rather than estimating effects. Case studies also have other advantages.

Variation: Variation is necessary to determine whether there is any pattern in a set of phenomena. Whether the aim is to generate or test theory, the analysis builds on the identification of patterns. Ceteris paribus, more cases/units of analysis provide us with better opportunities to observe a pattern and identify a probable cause and effect relationship. C*eteris paribus*, the larger the number of cases, the better. Unless a case contains sub-cases and allows for more observation, a single case study is a logical impossibility (Gerring, 2004; cf. Yin, 1994). It is not possible to draw descriptive or causal conclusions from a constant (though it is possible to create variation via comparison with counterfactuals, i.e. a hypothetical second case).

Say, for the sake of the argument, that a researcher is doing a study of three local governments over a period of two decades and concludes that local democracy has weakened (less participation, less congruence between mass opinion and elite decisions, etc.). One should immediately ask how it is possible to draw inference and generalize from three cases. There are large and small municipalities, rich and poor, more or less bureaucratic, more or less democratically organized local councils, different types of politicians. These and other conditions can theoretically have an impact on how local democracy works. How has the research design accounted for these factors? In this case, a "most different systems design" will be the most appropriate (Frendreis, 1983; cf. Przeworski and Teune, 1970). Nevertheless, there are reasons to remain skeptical with respect to drawing too firm conclusions on the background of only three cases.

The simplest causal thesis we can imagine claims a cause and effect relation between two dichotomous variables. A plausible thesis is that people with high salaries (X) are inclined to have right-leaning political attitudes (Y), while those with low wages do not, cf. Figure 1.

Figure 1: Causal thesis for categorical, dichotomous cause and effect variables

	Right-leaning	Not right-leaning
High income	(H, R)	
Low income		(L, not-R)

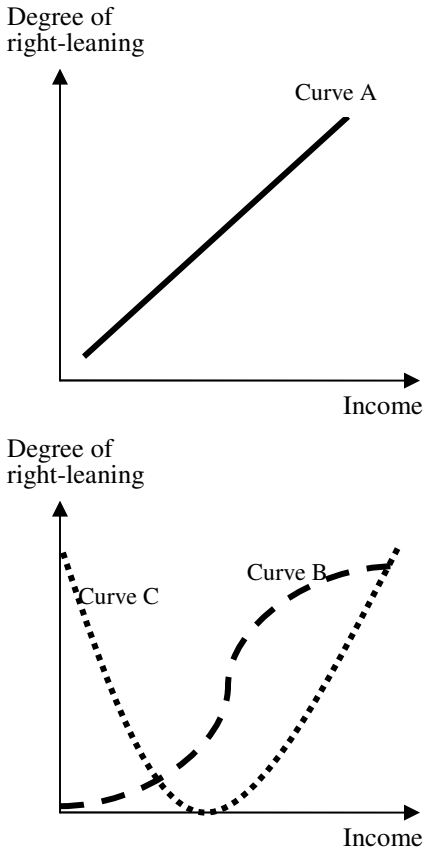
The minimal test of causality requires that the researcher analyzes at least one person earning a high income and one person receiving a low income. We will not be very confident with such a test no matter what it shows, because other, non-studied persons could subscribe to other opinions and because opinions are formed by many other and perhaps more important factors. Moreover, we may conjecture that the causal relation is not deterministic. Instead, we may hold that if a person has a high income, then the probability that they are right-leaning is greater than if the person had a low income. In other words: if X=high income, then P(Y=right-leaning) increases.

If the causal thesis concerns the relationship between the individual's income and political opinions, then most will find an analysis consisting of only two individuals insufficient. But if the individual is replaced with local governments, parties, reforms, or welfare states which are immensely more complex than one single individual, then – for some reason – we tend to accept causal generalization more readily. However, our skepticism against drawing too firm conclusions from only two cases would be equally justified. Why should two particular localities, two parties, or two reforms say anything about local government, political parties, or reform politics more generally?

Causal effects, control and large N-studies: Few causal theses have the form illustrated in Figure 1 above. The thesis that highly educated persons are right-leaning is very simple; and in its deterministic form, merely one anomaly is enough to reject it. With everything we know the probability that it is correct is not particularly great. Most researchers in the field will probably argue that the causal model is inadequate, incorrectly specified, and fails to make use of the existing knowledge. A more plausible, alternative causal model would be: the higher the income, the more right-leaning the individual. This necessitates measuring income and the tendency to lean to the right on at least a rank order level, but we will typically analyze the variables as if it were an interval-scale level. The question then becomes whether we expect the relationship to be linear (curve A in Figure 2) or that only over a certain level will increasing income lead to an greater tendency to lean to the right (curve B). It is also possible that the most impoverished people are just as right-leaning as the wealthy and that the

tendency to lean to the right first falls with rising income, but then increases again for very high incomes (curve C). Theoretically, all three theses can be justified.

Figure 2: Different causal theses for interval-scaled cause and effect variables.



With two cases, it is not possible to determine which of the three theses is most correct. With two cases, e.g. one with a mid-level income and one with high income, none of the three theses could be rejected. In order to estimate which form the causal effect has, more observations are necessary (King et al., 1994: 75-82). Because most causal relations are probabilistic we can only estimate average causal effects. Therefore many observations are necessary before we can estimate with an acceptable degree of certainty which thesis is the most plausible.

Again, most people are likely to accept the reasonableness of these considerations when the issue is the impact of income on political attitudes. However, considerations about the form, strength and character of causal effects are equally pertinent for other fields of research and other objects of analysis. Which form of causal relationship is there e.g. between (a) the strength of the Social Democratic Party (e.g. measured in terms of the number of years in government) and the increase in welfare expenditures; (b) the prosperity of

municipalities and the degree of contracting-out; (c) economic growth and democratization; (d) the degree of corporatism and equality?

The relationship between cause and effect can be more or less complicated, and the causal relationship can be conditioned by other interacting factors. But the relationship can also be spurious, meaning that both cause and effect are affected by a common third variable. In the example between income and right-leaning political attitudes, the relationship could for instance depend on whether one is employed in the public or private sector. The thesis could be that income means less to the tendency to lean right for employees in the public sector than for those employed in the private sector. Similarly, it is possible that the relationship between income and the tendency to lean right is neutralized when taking education into account; those with a higher education typically have a higher income and lean further to the right.

New Danish research indicates that education rather than income is decisive for the degree to which an individual leans to the right (Stubager, 2006, 2007). In other words, all of the theses about how income affects the degree to which an individual leans to the right can be rejected. In addition, the relationship between education and right-leaning political attitudes is negative, not positive; i.e. the higher the education, the less the right-leaning. It should be added, however, that the left-right dimension we are talking about here is the post-materialist, new politics scale (values) not the economic, old politics of redistribution. If we consider the old dimension – measured in terms of opinions on the state intervention and welfare spending – it appears as though high income continues to bring about increased right-leaning attitudes, but it is not tested equally thoroughly as the new politics dimension. It seems, however, that the tendency to lean to the right only increases with income at a rather high level of household income (Goul Andersen, 2003: 302-305). Curve B in Figure 2 thus provides the best image of the relationship when the effect variable is the old left-right scale.

Causal effects or causal mechanisms: Large N-studies are preferable when studying complex multivariate causal models, and when the interest is in the shape and strength of the causal effects. Multiple units of analysis also increase the opportunity to control for third variables. However, ultimately no causal claim is better than the theoretical argument on which the claim rests. With large N-studies, there are often partially theorized – but not empirically demonstrated – links between the cause and effect variables (cf. e.g. Ragin, 2004). Well-designed large N-studies can also contribute to the investigation of causal mechanisms, but well-designed comparative case studies are often better (Gerring, 2004: 348-49).

The literature addressing small N-studies emphasizes with various words and concepts that intensive case studies are better than large N-studies, if we are

interested in studying how a cause leads to a certain effect (Yin, 1994; Ragin, 2004; McKeown, 1999; Munck, 2004; Gerring, 2004). In large N-studies we rarely have good comparable indicators for all the links (or intermediate variables) that a causal claim normally includes. If the causal theory is strong, perhaps because well-crafted case studies have already demonstrated the causal links, we do not have to demonstrate them again. Large N-studies then make good sense, because of the reasons already discussed above. But political science does not always have strong and well-documented causal theories, and studies of causal mechanisms can then be extraordinarily productive. In fact, it can be reasonable to reject theories if the causal mechanisms cannot be specified and demonstrated. Let us consider an example.

One of the most canonized and studied theses in Danish political science, is Ole P. Kristensen's thesis about the asymmetrical decision-making process and the growth in public spending (1987; cf. Buchanan and Tullock, 1962). One of the implications of his thesis is that the expenses for consumption where the employees have a concentrated producer interest in large budgets will grow more rapidly than the expenses to transfers where there is no such interest. The thesis has not found unconditional empirical support (Christiansen, 1990; Green-Pedersen, 1998), but that is not decisive here. The thesis is normally tested by investigating the growth in expenditures for consumption and transfers over time and by controlling for other causes for the changes in spending, cf. Figure 3.a. This is all well and good, but the thesis rests upon a number of causal links and less-specified mechanisms that are not studied very closely. How does the alleged pressure for increasing expenses proceed?

The pressure can be exerted in different ways. One possibility is that the producer interest organizations (e.g. childcare professionals, teachers, and doctors) are more active lobbyists in the political decision-making process than interest organizations representing clients who benefit from cash payments (e.g. The DaneAge Association, The National Association of Schoolparents, various patient associations), cf. Figure 3.b. Furthermore, producer interests may have more resources and expertise and therefore be better lobbyists. We could then also expect that producer interest organizations have closer and better networks to politicians and civil servants, and that their views are referred to more often in parliamentary debates and in the media. In other words, the thesis that the growth in service is greater than the growth in transfer payments can be studied in two different ways, as illustrated in Figure 3.

Figure 3: Causal models: (a) control for third variables or (b) causal mechanisms

Figure 3.a.: Many cases, control for third variables:

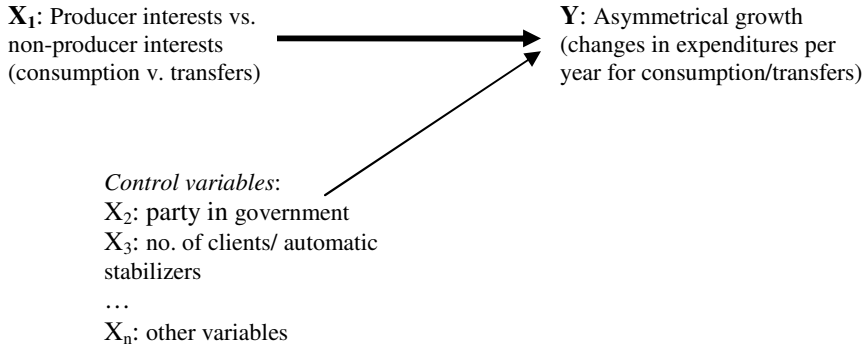
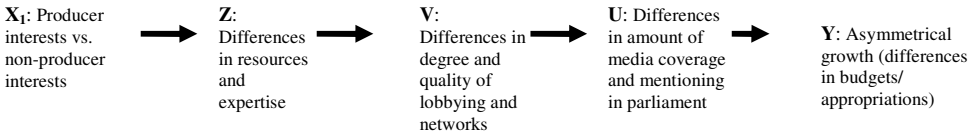


Figure 3.b.: Few cases, causal mechanisms (intermediary variables):



What is decisive in this context is not whether the causal mechanisms are entirely correct or whether there are other, more plausible mechanisms (which should then be specified so that they can be made the object of empirical investigation). An analysis of causal mechanism does not necessarily imply a comparative case study design, though in practice it will often do so. The proposed theory concerning the relation between type of interests and asymmetrical growth could in fact be tested in a large N-study with the help of statistical analysis.

The opportunity to study processes and causal mechanisms is not the only advantage of case studies of relatively few units of analysis compared with large N-studies (Yin, 1994; Gerring, 2004: Ragin, 2004; Munck, 2004). In fact, it may even be argued that the inference logic in well-designed cross-time and cross-sectional case studies is not that different from the experiment (Gerring and McDermott, 2007). When the focus is on the role of case studies in causal analysis, in particular two strengths deserves to be mentioned.

If we want to identify patterns and demonstrate cause and effect we must study strictly comparable phenomena (in statistics we refer to 'unit homogeneity') (Munck, 2004; Gerring, 2004). Two cases are never identical; they are only comparable and identical in a certain theoretical light. Many large N-studies implicitly assume that phenomena that are called the same are the same –

semantic equivalents are treated as causal or functional equivalents. For example, that the Social Democratic Party in the year 1945 is the same as the Social Democratic Party in 1975 and 2005, and that this is also true across countries. A conventional study of the impact of Social Democratic government (as opposed to a right-of-centre government) on welfare would for instance compare increases in welfare spending since 1945 across type of government and control for a range of third variables (see e.g. Swank, 2001 for this type of analysis). But is it reasonable to assume that the Social Democratic Party of 1945 is a causal equivalent for the Social Democratic Party of 2007? Does the party have the same type of voters (do they still exist?)? Is the relationship to the trade union movement the same (does it still exist?)? Is the ideology the same (does it still exist?)? Behind the theory on the impact of Social Democratic incumbency on welfare spending, there are a lot of assumptions about what a Social Democratic party is and what incentives it faces. Comparative case-studies of the development of Social Democratic parties could clarify whether it is reasonable to assume this sameness in time and space. A lot of concept stretching could be avoided if we did more good descriptive case studies. Without good description we cannot understand and, therefore, not make valid causal tests.

Further along these lines, comparative case studies are more useful, the weaker our concepts and causal theories about the phenomenon in question. If we only have a weak conception of what can be the cause of a certain phenomenon, it can be a good idea to study the phenomena more closely in order to establish clearer causal theses for comparable cases (see e.g. McKeown, 1999). This view is a partial accommodation of Geertz's argument for "thick description"; but only partial. Our deep case studies are colored by our theoretical interest and perspective, and our aim is to build theory and generate hypotheses. We seek to understand and explain the general and conform; not the unique.

> There is no analysis of a single event apart from the subject category that defines it, the theory that grants it significance, and the method that establishes its location in context (King, 2006: 16)

4. The social sciences and democracy – the truth up for debate

Offhand, the relationship between democracy and the social sciences seems strained. Informed discussion in a democracy is about realizing collective acts of will and having an impact on the development of society. Informed discussion in a research community is about uncovering the conformist aspect of social action and to demonstrate the predictability of the development of society (or at least explicability). However, this opposition is not real.

Despite the hopes expressed by August Comte, the social sciences can never attain definitive knowledge about universal laws or regularities. All theories and general causal relationships depend on known and, as a rule, also unknown factors. This is inevitably so primarily because people are creative and equipped with individual wills, which interact and sometimes produce a collective, i.e. political, action. People can learn and change society. The basic conditions in the social sciences thus also hold true for political life. Unpredictability poses challenges for the social sciences, while in the political life it is a precondition for politics to make a difference. If politicians cannot make a difference, the conception of democratic accountability is absurd and uncalled for.

The greatest honor that can be bestowed upon solid and critical social science research is for politicians to acknowledge the research results and use them to change the status quo. The analysis that was spot-on yesterday can therefore be wrong tomorrow. Perverse incentives are changed; concealed motives are re-considered; and policy failures may be addressed and remedied. Our existing knowledge of cause and effect becomes obsolete and existing theories must be revised.

The argument here is that good political science ought to start by asking two questions: what do we know, and what are we going to learn? It is only when these two questions have been answered that we can ask the third question, namely *how can we learn?* This is the central question to the philosophy of science and social science methodology. It is about conceptions of man and society and how we can acquire the knowledge we seek with the greatest possible certainty. It is about how, on the background of self-reflection and good craftsmanship, we account for how we draw inference about empirical phenomena – how do we know that we know what we claim to know? But the question does not become meaningful until we have responded to the first two and more fundamental questions. If, as claimed in this article, there is a relationship between question, theory and method, then researchers must be good craftsmen. Otherwise we are limited in terms of the questions we can raise and try to answer.

Both in democracy and research there are rules for how we can substantiate and validate knowledge. As knowledge in the social sciences by definition is provisional, the rules of the game are basically not that different. The free exchange of opinion, freedom of ideas and speech, and a pluralism that lends room to good arguments and justification are ground rules in both spheres. Also apparently odd and eccentric arguments have to be heard. Assuming that we can distinguish one from the other, neither the genius nor the moron should be oppressed. What John Stuart Mill, one of the sternest advocates of liberty and pluralism, argues for society at large is an equally valid standard for the social sciences. To be right you must be able to be proven wrong.

> But the peculiar evil of silencing the expression of an opinion is that it is robbing the human race; posterity as well as the existing generation; those who dissent from the opinion, still more than those who hold it. If the opinion is right, they are deprived of the opportunity of exchanging error for truth: if wrong, they lose, what is almost as great a benefit, the clearer perception and livelier impression of truth, produced by its collision with error (Stuart Mill, 1989 [1859]: 20)

References:

Albæk, Erik, Peter Munk Christiansen & Lise Togeby (2002). *Eksperter i medierne. Dagspressens brug af forskere 1961-2001* (Experts in the media. The use of researchers in daily newspapers 1961-2001), Aarhus: Aarhus University Press.

Almond, Gabriel A. (1996). "Political science: The history of the discipline", pp. 50-89 in Robert E. Goodin & Hans-Dieter Klingemann (eds.), *A new handbook of political science*, Oxford: Oxford University Press.

Andreski, Stanislav (1974). *Social Sciences as Sorcery*, Middlesex: Penguin Books.

Arnold, R. Douglas (1990). *The Logic of Congressional Action*, New Haven: Yale University Press.

Bottomore, T.B. (1964). *Karl Marx: Selected Writings in Sociology & Social Philosophy*, New York: McGraw-Hill Book Company.

Brady, Henry E. (2004). "Doing Good Things Better: How Far Does the Quantitative Template Get Us?", pp. 53-67 in Henry E. Brady & David Collier (eds.), *Rethinking Social Inquiry: Diverse Tools, Shared Standards*, Lanham, MD: Rowman & Littlefield.

Buchanan, James M. and Gordon Tullock (1962). *The Calculus of Consent*, Ann Arbor: University of Michigan Press.

Buckler, Steve (2002). "Normative Theory", pp. 172-194 in David Marsh & Gerry Stoker (eds.), *Theory and Methods in Political Science*, New York: Palgrave.

Cerniglia, Floriana (2003). "Decentralization in the public sector: quantitative aspects in federal and unitary countries", *Journal of Policy Modeling*, vol. 25, pp. 749-776.

Christiansen, Peter Munk (1990). "Udgiftspolitikken i 1980'erne. Fra asymmetri til asymmetrisk
Tilpasning" (Budget policy in the 1980s. From asymmetry to asymmetrical adaptation), *Politica*, vol. 22, no. 4, pp. 442-456.

Downs, Anthony (1957). *An Economic Theory of Democracy*, New York: Harper and Row.

Frendreis, John P. (1983). "Explanation of Variation and Detection of Covariation: The Purpose and Logic of Comparative Analysis", *Comparative Political Studies*, vol. 16, pp. 255-272.

Geertz, Clifford (1973). "Thick Description: Toward an Interpretive Theory of Culture", pp. 3-30 in *The Interpretation of Cultures*, New York: Basic Books.

Gerring, John (2004). "What Is a Case Study and What Is It Good for?", *American Political Science Review*, vol. 98, no. 2, pp. 341-354.

Gerring, John (2005). "Causation: A Unified Framework for the Social Sciences", *Journal of Theoretical Politics*, vol. 17, no. 2, pp. 163-198.

Gerring, John & Rose McDermott (2007). "An Experimental Template for Case Study Research", *American Journal of Political Science*, vol. 51, no. 3, pp. 688-701.

Giddens, Anthony (1984). *The Constitution of Society*, Cambridge: Polity Press.

Glaser, Barney G. (1998). *Doing Grounded Theory: Issues and Discussions*, Mill Valley, CA: Sociology Press.

Goul Andersen, Jørgen (2003). "Farligt farvand: Vælgernes holdninger til velfærdspolitik and skatter" (Dangerous waters: the opinions in the electorate to welfare policy and taxation), pp. 293-314 in Jørgen Goul Andersen & Ole Borre (eds.), *Politisk forandring. Værdipolitik og ny skillelinjer ved folketingsvalget 2001* (Political change. Value politics and new cleavages at the 2001 Folketing election), Aarhus: Systime Academic.

Green-Pedersen, Christoffer (1998). "Udviklingen i udgifterne til overførsler og konsum i Danmark fra 1982 til 1992. Hvorfor holdt asymmmetri-tesen ikke stik?" (The development in spending for transfers and consumption in Denmark from 1892 to 1992. Why did the asymmetry-thesis not hold?), *Økonomi og Politik*, 2, pp. 25-37.

King, Gary, Robert O. Keohane & Sidney Verba (1994). *Designing Social Inquiry: Scientific Inference in Qualitative Research*, Princeton: Princeton University Press.

King, Ronald F. (2006). "The Strategy of Research: Thirteen Lessons on the Elements of Social Science", San Diego: San Diego State University (unpublished manuscript. Published in Romanian).

Kristensen, Ole P. (1987). *Væksten i den offentlige sektor* (The growth in the public sector), Copenhagen: DJØF Publishing.

Laclau, Ernesto & Chantal Mouffe (1985). *Hegemony and Socialist Strategy*, London: Verso.

Lijphart, Arend (1971). "Comparative Politics and Comparative Method", *American Political Science Review*, vol. 65, pp. 682-693.

Marsh, David & Paul Furlong (2002). "A Skin, not a Sweater: Ontology and Epistemology in Political Science", pp. 17-41 in David Marsh & Gerry Stoker (eds.), *Theory and Methods in Political Science*, New York: Palgrave.

McKeown, Timothy J. (1999). "Case Studies and the Statistical Worldview", *International Organization*, vol. 53, no. 1, pp. 161-190.

Munck, Gerardo L. (2004). "Tools for Qualitative Reserach", pp. 105-122 in Henry E. Brady & David Collier (eds.), *Rethinking Social Inquiry: Diverse Tools, Shared Standards*, Lanham, MD: Rowman & Littlefield.

Niskanen, William A. (1971). *Bureaucracy and Representative Government*, Chicago, IL: Aldine-Atherton.

Przeworski, Adam & Henry Teune (1970). *The Logic of Comparative Social Inquiry*, New York: John Wiley.

Ragin, Charles E. (2004). "Turning the Tables: How Case-Oriented Research Challenges Variable-Oriented Research", pp. 123-138 in Henry E. Brady & David Collier (eds.). *Rethinking Social Inquiry: Diverse Tools, Shared Standards*, Lanham, MD: Rowman & Littlefield.

Rasmussen, Erik (1971). *Komparativ politik 1*, 2nd edition, Copenhagen: Gyldendal.

Rogowski, Ronald (2004). "How Inference in the Social (but Not the Physical) Sciences Neglect Theoretical Anomaly", pp. 75-85 in Henry E. Brady & David Collier (eds.), *Rethinking Social Inquiry: Diverse Tools, Shared Standards*, Lanham, MD: Rowman & Littlefield.

Serritzlew, Søren (2007). "Det politologiske eksperiment: hvorfor, hvornår og hvordan?", *Politica*, vol. 39 nr. 3, pp. 275-293.

Sørensen, Curt (1991). "Den historiske materialisme i lyset af nyere diskussion om social handlen og social objektivitet" (Historical materialism in the light of recent debate about social action and social objectivity), *Politica* 23(1), pp. 20-36.

Stegarescu, Dan (2006). *Public Sector Decentralisation: Measurement and International Trends*. ZEW Economic Studies, vol. 34, Physica-Verlag HD.

Stuart Mill, John (1989) [1859]. *On Liberty and other writings*, ed. by Stefan Collini, Cambridge: Cambridge University Press.

Stubager, Rune (2006). *The Education Cleavage: New Politics in Denmark*, Aarhus: Politica.

Stubager, Rune (2007). "Uddannelse og ny politik: En dekomponering af uddannelseseffekten på nypolitiske holdninger (Education and new politics: deconstructing the effect of education in new politics opinions)" pp. 89-108 in Jørgen Goul Andersen et al. (eds.), *Det nye politiske landskab: Folketingsvalget i 2005 i perspektiv* (The new political landscape: The 2005 Folketing election in perspective), Aarhus: Academica.

Swank, Duane (2001). "Political Institutions and Welfare State Restructuring: The Impact of Institutions on Social Policy Change in Developed Democracies", pp. 197-237 in Paul Pierson (ed.), *The New Politics of the Welfare State*, Oxford: Oxford University Press.

Tsebelis, George (1990). *Nested Games: Rational Choice in Comparative Politics*, Berkeley: University of California Press.

Weber, Max (1993) [1913/1925]. *Basic Concepts in Sociology*, New York: Carol Publishing Group/Citadel Press.

Yin, Robert K. (1994). *Case Study Research: Design and Methods*, Newbury Park, CA: Sage.