3rd. International Conference on Public Policy (ICPP3)
June 28 - June 30, 2017
Singapore

Systematic Case Selection in the "Context of Discovery": The Concept of Positive Instrumental Case Studies*

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Panel T10P05: Methodological Advances in Policy Studies and Comparative Public Policy
June 29, 10:30-12:30

Abstract: The paper develops an approach to systematic case selection in the context of discovery, i.e., early phases of research when concepts and theories are formed, for which no guidelines currently exist. Current literature advises on case selection based on well-specified concepts and hypotheses, but is silent on case selection during concept formulation and theory building. The paper introduces the concept of positive instrumental case studies and illustrates the case selection strategy empirically. The contribution lies in improving our ability to develop new concepts and theories by demanding, facilitating and supporting self-awareness about consequential decisions made in early phases of research.

Keywords: Case selection, case study research, positive instrumental case study, theory development, context of discovery, abduction, research process

Word count: 8.661 incl. tables, footnotes, references; excl. title page

*The paper is a condensed version of the preliminary methodological framework of my dissertation, which is to be handed in the summer of 2018.

1. Introduction

When political science undergraduates step foot in their first course on research design, the teacher will most likely show them an idealized depiction of a good research process. This depiction will tell students that their research process consists of different, interlinked, yet clearly identifiable stages and corresponding decisions that logically follow and build upon each other. In one way or another, we tell students to start their process by formulating a research question or problem statement. Based on this, they should review relevant literature and existing theory and formulate clear concepts and specific, testable hypotheses. Based on these concepts and hypotheses, they are then expected to set up a suitable research design that allows them to test their hypotheses. They should choose relevant cases, collect necessary data, analyze their material, and generalize their results in order to contribute to the broader research in the field[1].

For sure, we tell students that the different stages of the research process are closely interrelated, that they will have to jump back and forth between them, that, simply put, the real research process is much more chaotic and messy than such an idealized depiction suggests. And yet, most methodological literature we recommend to our students is based on the premise of such a clear, sequential, idealized research process. For example, in case study research, we find plenty of advice on how to select "good" cases for our research, but this advice almost exclusively follows a deductive logic that necessitates precise concepts and clear hypotheses to start with[2]. But on which basis do we choose cases when we engage in research for which no ready-

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[1] Cf. Piekkari et al. 2010: 110, in Dubois and Gadde 2014
[2] Cf. on case study research i.a.: Eckstein 1975, Gerring 2004, Gerring and McDermott 2007, Levy 2008, Seawright and Gerring 2008, Blatter and Haverland 2012, Rohlfing 2012, Yin 2014

made concepts are available in the existing literature, when we are in the *context of discovery*, that is, the process of forming concepts and defining the phenomenon we want to investigate?

Neither the methodological literature on case study research, nor the growing body of work on abduction and theorizing can answer this question. This shortcoming is particularly concerning since we do not only look at empirical material once our concepts and hypotheses are carved in stone, but we engage with empirics from the very beginning of our research. And more importantly, the early confrontation with potential cases crucially shapes the concepts, hypotheses and theories we later propose and test. Since many of us have the ambition to make theoretical and conceptual contributions to our research community, it is important to raise awareness of and reflect on the consequential decisions we make in the context of discovery, to communicate them better in the presentation of our research, and to develop systematic guidelines for it.

In this paper, I develop an approach to systematic case selection in the context of discovery. I draw on the growing literature on abductive reasoning and suggest that abduction is not a radically new, but a more apt description of how we do research than the ideal-typical dichotomy of deductive and inductive research processes. Additionally, I bring in and develop further the concept of instrumental case studies and suggest the theory-guided identification of *positive instrumental cases* as an approach to systematic case selection in the context of discovery. In practice, this means that the researcher selects *positive* cases that likely show the phenomenon she is interested in, as well as potential causes, mechanisms, or affinitive phenomena she has hunches about. First, the researcher formulates an initial, vague concept of the phenomenon of interest. Based on this, she develops a list of indicators with which likely positive cases can be identified and their degree of *instrumentality* for empirically investigating the phenomenon of interest be evaluated. Based on this evaluation, cases can be categorized as suitable, promising, or ideal for further

empirical investigation of the phenomenon of interest and for developing theoretical implications and more precise concepts and hypotheses about it.

My approach does not contest that the teaching of idealized, "best practice" research processes is important for setting commonly agreed standards for how we conduct research. However, the focus on teaching of best practice and the retrospective communication of our research as following ideal-typical, best practice research processes comes at the price of lacking awareness, reflection, and methodological guidance for the important choices we make in the context of discovery. The contribution of my approach is therefore twofold: First, I provide a strategy for systematic case selection in the context of discovery, when concept are vague and theories under development, and for which no methodological advice is currently available. Second, I improve our ability to develop new theories, frameworks, and concepts. By demanding, facilitating, and supporting more self-awareness about the consequential decisions we make in the context of discovery, opportunities are created for the development of systematic guidelines that can help us advance the conceptual and theoretical contributions to the political and social science community.

The structure of the paper is as follows: In section 2, I evaluate the methodological literature on case selection concerning its usefulness in the context of discovery. In section 3, I sketch out an abductive approach to case selection, develop the concept of positive instrumental case studies, and suggest guidelines for how to select such cases. In section 4, I illustrate the application of these guidelines in the context of my own research on the strategic design of policy feedback. In section 5, I carve out the contributions of my approach to the methodological literature on case study research and to our ability to develop new theories and concepts.

2. Types of Case Studies and Case Selection Strategies in the Existing Literature on Case Study Research

In this section, I do not to provide a full review of the literature on case studies, but give a coarse depiction of common types of case studies and related case selection procedures from the perspective of theory development. I use Levy's discussion of different types of case studies and case study designs as a guideline[3]. Levi, like others, distinguishes between idiographic (inductive or theory-guided) case studies, hypothesis-testing case studies, and hypothesis-generating case studies[4].

Idiographic Case Studies

Idiographic case studies come in two different forms. Idiographic, inductive case studies are for example common in historical research. They lack an explicit theoretical framework that guides the investigation, but focus on a specific case they aim to explain in its totality, presenting all possible aspects of the particular case and their interconnections. The analytical value of these descriptions is limited since no attempts are made to analytically abstract from the particular case to more general patterns of causation or co-constitution. This type of case study therefore offers no explicit lessons for research that aims to build theory and concepts. Grounded theory, more common in sociology and ethnology, also follows an inductive approach. The researcher starts the investigation without theoretical preconceptions, but deliberately builds theory through deep immersion in the data 'from the ground up' through systematic conceptualization and constant

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[3] Levy 2008;

[4] As a fourth type, Levi also discusses plausibility probes, which are similar to pilot studies in experimental research designs. They help sharpen hypotheses, refine the operationalization or measurement of variables, or test a case's suitability for research before starting costly fieldwork or quantitative data collection. They are nomothetic in their orientation since the purpose of the probe of a particular case is to advance a broader theoretical argument. However, the ambiguous position of plausibility probes in-between hypothesis-testing and hypothesis-generating case studies renders them a residual category, and the label is often loosely used as a legitimizing device in reaction to growing demands for theoretically and methodologically self-conscious research practices (cf. Eckstein 1975, Levy 2008). Literature on plausibility probes is furthermore often silent on how to systematically select cases for plausibility probes and limits their utility to probes of singular propositions (Beach and Pedersen 2016: 288-9).

comparison with similar and distinct research areas[5]. Huge emphasis is placed on coding techniques, memoir practices, and similar tools for fieldwork and the handling of large amounts of qualitative data. However, grounded theory does not give advice on how to select cases in the first place and the idea of entering the empirical field without theoretical preconceptions hinders the conceptualization of an adequate process for theory building. Tavory and Timmermans, for example, argue that denying or suppressing prior theoretical knowledge disables the researcher from relating different theoretical concepts to each other or from identifying tensions between existing theories and empirical observations from which new insights can arise. Grounded theory therefore sidelines theory and amplifies existing notions of the world by broadening the database without telling the researchers which objects to focus on and how to link them to each other[6]. Inductive idiographic case studies therefore offer little advice on how to systematically select cases in the context of discovery.

The second type of idiographic case studies, theory-guided case studies, does not renounce theoretical preconceptions but uses these as guidelines in the investigation of a case. As Levy points out, "social scientists' explicit and structured use of theory to explain discrete cases often provides better explanations and understandings of the key aspects of those cases than do less structured historical analyses"[7]. However, two caveats remain: First, the process of theorizing and coming up with theoretical explanations is usually not explicated and communicated analytically. While it is "the constant dialogue between theory and evidence that constitutes the comparative advantage"[8] of such studies, it is often the reader's tasks to retrospectively recreate what took place in the context of discovery. Second, such studies typically start from a particular case

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[5] Tavory and Timmermans 2014: 9-19; cf. Glaser and Strauss 1967, Glaser 1978, 1992, 1995
[6] Tavory and Timmermans 2014: 9-19
[7] Levy 2008: 4-5
[8] Rueschemeyer 2003: 312

that is worth studying due to its historical importance, and the literature therefore gives no advice on how to systematically select cases when the researcher's motivation lies not in the case, but in tensions between theoretical arguments, an unsatisfactory review, or a problematization of existing research[9].

Hypothesis-Testing Case Studies

Hypothesis-testing case studies are increasingly common in qualitative research that aims to perform valid, reliable tests of theoretical arguments. Research designs focus particularly on case selection strategies and issues like selection bias, the number of cases, and the choice of good cases for comparison[10]. The generalizability of causal claims is often the central concern in such studies, and causal inferences follow a statistical reasoning that infers from a representative case or sample to a population of cases delimited by scope conditions[11]. The methodological literature therefore gives advice, e.g., on how to define scope conditions as narrow as necessary (in order to make valid inferences) and as broad as possible (in order to make good, broad generalizations). However, this advice requires definiteness we do not have in the context of discovery. When the cause(s) of the phenomenon of interest is not identified yet, when no clear hypotheses are formulated, we cannot pick cases according to rules that require such preconditions.

Nevertheless, hypothesis-testing case studies do offer some lessons for research in the context of discovery. A common strategy for case selection is the choice of most-likely cases which "show a relatively high probability of confirming the proposition under scrutiny"[12]. Even with-

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[9] Cf. Alvesson and Sandberg 2011a, b on problematization as methodology.
[10] Cf. i.a. Blatter and Haverland 2012, Rohlfing 2012
[11] Cf. for detailed discussions of different case selection procedures: Rohlfing 2012; Levy 2008, Seawright and Gerring 2008
[12] Cf. i.a. Rohlfing 2012: 84

out clear propositions in the form of hypotheses, but "only" with mere hunches or vague assumptions, these theoretical expectations can guide the selection of cases we look at early on in our research. Hence, we ought to select positive cases in which the phenomenon of interest and assumed causes or related phenomena are present, since these cases would most-likely confirm our hunch or assumption as compared to cases in which only one of the two or neither are present.

Hypothesis-Generating Case Studies

Hypothesis-testing case studies suggest, according to Levy, "additional explanatory and contextual variables, causal mechanisms, interaction effects, and scope conditions"[13]. Often, they are deviant cases that do not conform to an existing theory but help "refine and sharpen existing hypotheses in any research strategy involving an ongoing dialogue between theory and evidence. A theory guides an empirical analysis of a case, which is then used to suggest refinements in the theory, which can then be tested on other cases […]."[14] The starting point of research lies in an already established theory with clear propositions that can be refined, sharpened, or amended. Since they follow deductive research designs that necessitate clear concepts and hypotheses before engagement with the empirics, they do not offer much advice for researchers in the context of discovery. Other interpretations of hypothesis-generating case studies fall back on inductive research designs. For Rohlfing, e.g., hypothesis-generating case studies build hypotheses "from scratch"[15] when the researcher develops a hypothesis "only after exploratory process tracing"[16] and without drawing on elaborated theory. However, the inductive approach to theory building

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[13] Levy 2008: 5
[14] ibd.: 5
[15] Rohlfing 2012: 9
[16] ibid.

or hypothesis-generation is ill-suited to provide an adequate conceptualization of the early phases of research and theory building.

Summary

The review shows that the existing literature does not give adequate guidance for case selection in the context of discovery. It follows either the logic of scientific confirmation in deductive research designs, or the inductive building of "grounded theories" from empirical observations. Deductive research designs reduce the emergence of new theoretical expectations to spontaneous flashes of wit or unpredictable inspiration. They help researchers on how to conduct an investigation once concepts are formed and hypotheses formulated, but not when these preconditions are not fulfilled. Inductive research designs either do not even aim at developing theory beyond the particular case, or they fall short in giving advice on how to select cases and in conceptualizing an adequate process of scientific discovery that facilitates theory building. For case study researchers, this means that there is ample advice for systematic case selection in ideal-typical deductive research designs, but no guidance on how to select cases in the context of discovery, which we all go through early on in our research when we start engaging with empirical material while developing concepts and theoretical expectations. Nevertheless, two lessons can be learned from the existing literature: First, theory-guided, idiographic case studies describe a dynamic research process that oscillates between theory and evidence and that has the potential to build novel theory, but the process is not made analytically explicit. Second, case selection in early phases of research can be informed by a relaxed version of most-likely case studies.

3. Developing an Abductive Approach to Case Selection: The Concept of Positive Instrumental Case Studies

Many of us have the ambition to not only perform empirical tests or applications of established theories, but to participate in the research community with theoretical and conceptual contributions. However, in recent years, many social scientists have become increasingly critical of our ability to produce new theories and of how we communicate research in the context of discovery[17]. A dynamic debate has developed around these issues under the label of abduction, which, as I argue, we should understand as a more apt description of how we conduct research and which thus helps us reflect upon our own doing and develop systematic guidelines for case selection in the context of discovery[18].

Abduction as an Apt Description of Research Processes

The concept of abduction originally goes back to the Charles Peirce, for whom it "is the process of forming an explanatory hypothesis"[19]. The emphasis is here not only on the explanation itself, but "on the process of coming up with an explanation or how to get there"[20]. Abduction might be hard to grasp initially because we are used to the ideal-typical dichotomy of induction and deduction that seems to describe all possible ways of doing research and relating theory and evidence to each other. In this dichotomy, the researcher either moves inductively from evidence to theory, i.e., from the particular case to the general law, or she moves deductively in clear and identifiable steps from theoretical reasoning to empirical tests of hypotheses. Abduction, in contrast, means a constant oscillation between theory and evidence. Research moves "in an iterative-

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[17] Cf. i.a. Dubois and Gadde 2002, Kilduff 2006, LePine and Wilcox-King 2010, Alvesson and Sandberg 2011a, b, Swedberg 2012, Dubois and Gadde 2014, Swedberg 2014b, a, Tavory and Timmermans 2014, Carleheden 2016, Swedberg 2016.
[18] Cf. i.a. Dubois and Gadde 1999, 2002, McKaughan 2008, Friedrichs and Kratochwil 2009, Schwartz-Shea and Yanow 2012, Swedberg 2012, Tavory and Timmermans 2014, Swedberg 2016
[19] Pierce 1934a:171-21, in Swedberg 2014a: 101
[20] Swedberg 2014a: 101

recursive fashion between what is puzzling and possible explanations for it"[21]. It is these iterative-recursive movements that we know from our daily work, that we struggle with, and that make the real research process so much more messy and chaotic than the ideal-typical depictions we show to our students suggest. Hence, abduction is a much more apt conceptualization of real research processes than ideal-typical inductive and deductive models of research. It is not a new best-practice prescription for how we ought to do research, but helps us to reflect upon how we conduct research in our daily work, develop systematic methodological guidelines that we can follow in the context of discovery, and improve our ability to participate in our research community with theoretical and conceptual contributions.

First, abduction emphasizes that research starts from a puzzle, a surprise, or a tension that the researcher seeks to explicate and make "less surprising[22]. Such surprises can emerge from a misfit between what we expect to find in a case or data and what we actually observe. What we then typically do is to model the existing literature in a way so that we can identify and close a corresponding gap. An alternative that better reflects what often triggers our research is that of problematization. Problematization means that the researcher identifies and challenges assumptions underlying existing research[23]. Often, we do not make these problematizations explicit but instead choose to communicate our research as closing a gap[24]. The concept of abduction helps highlight that research processes neither have to start from an atheoretical point rooted solely in

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[21] Schwartz-Shea and Yanow 2012: 27

[22] ibd.: 27-28

[23] We can, for example, challenge "in-house assumptions that exist within a specific school of thought", "root metaphors, [i.e.] broader images of a particular subject matter underlying existing literature", or "paradigms, [i.e.] ontological, epistemological and methodological assumptions underlying existing literature." (Alvesson and Sandberg 2011a: 256-260, cf. Alvesson and Sandberg 2011b)

[24] For a variety of reasons, "gap-spotting" and is a more attractive way of communicating one's research than problematization. For example, it acknowledges others, seemingly contributes to knowledge accumulation, and it is required and recognized by funding agencies and journals and publishers (cf. Alvesson and Sandberg 2011a, b).

the empirics (as in inductive research) nor from logical reasoning on the theoretical level discon-nected from empirical observation (as in deductive research processes), and that corresponding case selection strategies are therefore not helpful in early phases of our research.

Second, abduction emphasizes that research does not follow pre-given steps, that it is not linear and not leading towards an ex-ante known goal. Dubois and Gadde, for example, describe research as a "nonlinear, path-dependent process of combining efforts with the ultimate objec-tive of matching theory and reality"[25]. This constant matching effort is not a series of discrete inductive and deductive steps, but "the researcher is simultaneously puzzling over empirical ma-terials and theoretical literature."[26] Both theory and empirics develop throughout this process, which highlights that our research is particularly in its early phases rarely based on clear-cut concepts and hypotheses. Hence, we frequently have to choose cases while we develop, modify, and adapt our concepts and theoretical expectations, but, again, for these choices we need to develop systematic guidelines to follow.

Third, abduction emphasizes that research processes are unpredictable and open-ended. They are path-dependent and depending on which pieces you add for solving a puzzle, you see different patterns or solutions forming. At the same time, research processes have "no obvious patterns. Our efforts to match theory and reality can take us in various directions. There is never one single way of matching. On the other hand, it can be argued that some ways turn out to be better than others are. This is a result of the process and cannot be known in advance.[27] Hence, abduction highlights that decisions we make early on in our research, e.g. case selection, greatly influence the later outcomes and the implications we draw[28].

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[25] Dubois and Gadde 2002: 556
[26] Schwartz-Shea and Yanow 2012: 27
[27] Dubois and Gadde 2002: 556
[28] Schwartz-Shea and Yanow 2012: 30-31

A Strategy for Case Selection in the Context of Discovery

Since existing methodological case study literature does not take an abductive perspective on the research process but follows ideal-typical deductive or inductive models, I now develop guidelines for systematic case selection in the context of discovery from an abductive perspective[29]. First, I introduce the concept of instrumental case studies, then, I propose the *theory-guided selection of positive instrumental cases* as a strategy for case selection. In the next section, I illustrate how this strategy can be applied empirically by drawing on my own research on the intentional design of policy feedback.

The Concept of Instrumental Case Studies

The concept of instrumental case studies was originally proposed by Stake[30]. According to Stake, a case study is instrumental when the case or cases studied are vehicles, i.e. instruments, for a different purpose. They serve to "provide insight into a particular issue, redraw generalizations, or build theory. In instrumental case studies the case facilitates understanding of something else."[31] It plays a supportive role in addressing the puzzle, tension, or surprise that motivated one's research or in problematizing the existing literature. The investigation does not aim to deductively test a priori defined hypotheses, but to produce novel theoretical implications, develop new hypotheses, and yield new theoretical and empirical insights during the course of the research. Cases and their contexts are therefore looked at in depth and described in rich detail in order to create opportunities for a better understanding of the phenomenon of interest better. The focus of the case study is known beforehand and does not emerge inductively from the case, but an evolving theoretical framework and the empirical investigation guide each other.

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[29] Cf. for exceptions: Dubois and Gadde 2002, 2014
[30] Stake 1994, 1995, Grandy 2010
[31] Grandy 2010:474

Hence, the case is not chosen by its historical significance or a deductive logic of hypothesis-testing, but through careful theoretical consideration of learning opportunities about the phenomenon of interest. Similar to the way that abduction is not a prescription of how to do research, but a more apt description of real research processes, the concept of instrumental case studies is a more apt description of how researchers think about the case(s) they study.

Reflecting about case studies with the concept of the instrumental case in mind, we can more clearly think about which role the case plays in our research and how we can systematically select cases. The concept highlights that the motivation for our research rests neither within one particular case (as in idiographic case studies and Rohlfing's understanding of hypothesis-generating case studies), nor is it based purely in theoretical reasoning (as in hypothesis-testing case studies or Levi's understanding of hypothesis-generating case studies). It points out that the role of the case often lies in between those two extremes. The particular case is not elementary to motivate the inquiry, but it is a case of something bigger. It represents a conceptual interest, a puzzle or tension between theories and empirical observation, or a problematic assumption in the literature that the researcher challenges. Cases are, particularly in the context of discovery, not chosen deductively based on clear and precise concepts and hypotheses, but according to theoretical hunches and vague concepts that inspire our case selection. The early engagement with a case then shapes the subsequent research path and the coevolution of the theoretical framework and empirical database. In that sense, the case itself also shapes what it is a case of.

A Strategy for Systematic Case Selection in Theory-Building Research: Identifying and Selecting Positive Instrumental Cases

We should therefore base case selection on careful theoretical considerations of the learning opportunities the case might provide. In contrast to conventional views on case selection, we do

not select the instrumental case to represent an a priori defined population of cases, but to maximize learning opportunities about the phenomenon of interest. Case selection is therefore not a problem of correct sampling, representativeness, and generalizability. In order to maximize learning opportunities, we select most-likely cases, which show "a relatively high probability of confirming the proposition under scrutiny"[32]. Since we often do not have precise theoretical propositions in the context of discovery, but rather vague concepts and theoretical hunches, we can instead use the term positive case to avoid misunderstandings. A *positive* case is then a case in which the phenomenon of interest as well as assumed potential causes, affinitive phenomena, or mechanisms that we are interested in investigating or that we have hunches about, are present. Through early and cursory engagement with the literature, both empirical and theoretical/conceptual work prior to our own empirical investigation, we can increase our knowledge of the phenomenon of interest, develop an initial concept and collect lists of related phenomena or potential causes. The concept and lists help us refine our theoretical expectations and narrow the number of potential cases to investigate. We can continue this iterative process of refining theoretical conceptions and engaging in the literature until we feel confident to summarize a list of theoretical indicators that helps us identify and evaluate positive cases. We can then evaluate potential positive cases for empirical investigation based on these indicators. This evaluation does not mean a definite measurement of a case on each indicator, but an evaluation of the case based on cursory reading of case-specific literature. The more indicators a case scores positively on, the more *instrumental* it is for learning about the phenomenon of interest. Hence, it is a *positive instrumental case.*

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[32] Rohlfing 2012: 84

4. An Illustration of the Selection of Positive Instrumental Cases

In my own research on the intentional design of policy feedback, I have utilized this case selection strategy. In the early stages of my project, I sought inspiration in a variety of different literatures on policy feedback, institutional change, structure and agency, and many more. Reading different theoretical work and empirical analyses, I eventually focused on the question of whether and how policy feedback effects might be the result of political actors' intentional and strategic action. Based on my understanding of the subject, I formulated an initial concept of "architectural policy design strategies" that suggested elective affinities between three interrelated elements: first, contextual factors, such as veto barriers and institutional discretion; second, policy design elements, such as compartmentalizing resource flows or enhancing or delimiting state capacities; and, third, policy effects, such as political learning or coordination effects. Then, I planned to investigate whether or how these affinities would play out in the real world, if I had missed or overlooked other elements or related phenomena, or if I was completely on the wrong track with my hunches and assumptions.

Step 1: Carving Out Underlying Assumptions of the Initial Concept

The methodological literature on case selection, however, left me in the dark about how to select cases in this early stage of my research, where my key concept that was rather vague and broad and not translated into one or two specific causal hypotheses. Due to this vagueness, almost all cases of public policymaking seemed, at first, like potential objects to study. In a first step, I therefore tried to theoretically demarcate the universe of potential cases by identifying two preconditions that where implicit in my concept: the intentionality and capability of political actors in policymaking. On one side, I was only interested in policy feedback that was the result of intentional action, but not policy feedback as a side-effect or unintended consequence. This was

both in contrast to most existing literature on policy feedback, but also posed a challenge since actors' intentionality cannot simply be read retrospectively from the resulting feedback effects. At the same time, the idea of architectural policy design necessitates that actors are capable of designing policies intentionally. They need to have the capability to invest time and effort in strategically designing policies and policy effects. This capability can, e.g., be reduced when policymaking happens in a "state of emergency" under extraordinary, unanticipated circumstances like natural disasters or abrupt economic crises, but in many cases we can assume that actors have such capabilities.

Step 2: Theorizing Indicators for Identifying Positive Instrumental Cases

While this demarcation helped me think more clearly about what I was interested in, it still allowed for many different incidents of public policymaking to be selected as a case. In a second step, I therefore theorized a number of indicators that would help me evaluate at a first cursory look whether a case of policymaking might show elements of architectural policy design. That is, the indicators were supposed to help me assess whether a case is a positive case and instrumental in studying architectural policy design. Each indicator represented a question that cases of policymaking could be confronted with in cursory investigations and literature reviews in order to determine whether or not they might show elements of architectural policy design. In total, I theorized 16 indicators, which I grouped into three dimensions: a resource dimension, a conflict dimension, and an impact dimension. The resource dimension related to political actors' capability for architectural policy design and its indicators help evaluate how capable and resourceful political actors are in regards to long-term, strategic policymaking. The conflict and impact dimension related to political actors intentionality and the indicators helped evaluate the likeliness of intentional, long-term policy-design. Figure 1, below, illustrates how these indicators

helped evaluate and categorize cases of policymaking. The square box comprised all *potential* cases of political architecture, that is, all policymaking that fulfills the two basic preconditions of intentionality and capability. Cases that I positively evaluated on several or all indicators from all dimensions constituted *ideal* cases. Cases that I positively evaluated on a majority or all indicators from two dimensions but negatively on all indicators from the third dimension constituted *promising* cases. Cases that I positively evaluated on only a few indicators from one dimension and none of the others constituted *suitable* cases. For each indicator, I formulated a guiding question that was to I in mind during the cursory reading of secondary literature on potential cases. I developed some of the indicators on the basis of the diverse literature I read at the beginning of my project, others I added only later on during my process, when I had started with cursory investigations of actual cases of public policymaking.
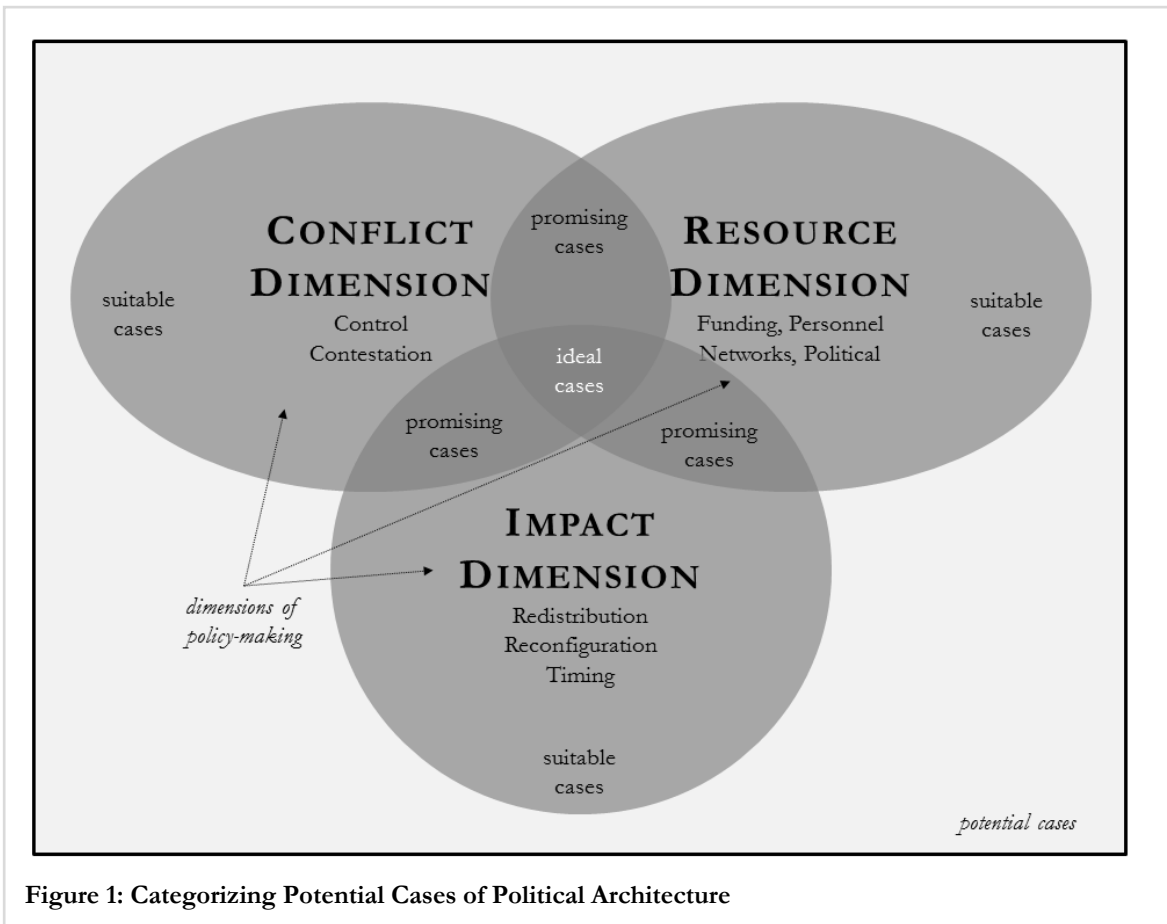

Figure 1: Categorizing Potential Cases of Political Architecture

For reasons of word limitation, I will only discuss the indicators in the resource dimension. Indicators in the resource dimension asked whether political actors possess resources that enable them to conduct long-term, strategic policy design. Specifically, it asked for four types of resources: (1) The first concerned actors' *funding.* The question was: *can actors financially afford to devise policies and/or evaluate policy drafts*? Since policymaking is a highly complex process, and policymakers face great uncertainty and complexity, the process of devising and evaluating policies or policy drafts requires substantial financial resources, amongst others in order to pay for qualified staff that evaluates previous policies, looks beyond national borders in attempts to learn from experiences, failures, and successes elsewhere, or consults with other experts or researcher. All these tasks require not just time, but also sufficient funding. Big unions or parties, for example, can run their own think tanks or foundations that develop and evaluate policy concepts, while small NGOs or newcomers in parliament will lack the means to be on par in this regard. (2) The second kind of resource concerned the *personnel,* with the question being *whether political actors are capable/qualified to devise and/or evaluate policy drafts.* As already suggested above, financial resources are not sufficient for long-term, strategic policy design, but collective political actors also need qualified staff that can carry out the complex task of policy design. The intricacies of the design process and the potential, anticipated, or intended effects of policies require great amounts of expertise, knowledge, and qualification that not all parties or interest groups can provide. (3) The third kind of resource concerned *networks,* or *whether or to what degree actors are included or heard in formal decision making processes.* Simply, good funding and qualified staff are helpful for drafting and evaluating policies, but political actors also need to be able to feed their ideas and suggestions into the formal political system. Sometimes, big unions might have well developed political concepts, possibly even ready-to-use policy drafts, but they can be shut out of decision-making if a

more employee-friendly government does not consider their position or objections. (4) In such cases, it can also be helpful to consider a fourth kind of resource that I called *political*, and that asks *whether or to what degree actors can create political pressure on formal decision makers*. Here, we can for example think of mobilization potential. Can a union, even if shut out of the decision making process, create political pressure by bringing its members and supporters on the streets? Or, are actors influential due to their long affiliation with established political actors, as one can for example assume in many countries for churches and conservative parties, or for unions and social-democratic parties. Taken together, these four indicators should give a good impression of the resources political actors possess regarding long-term, strategic policy design in concrete cases of policymaking. Partially, the indicators might overlap in certain cases, for example regarding funding and personnel. At the same time they allow for a differentiation between different kinds of resources that political actors might possess or not.

In a similar way, I developed the remaining 12 indicators in the impact and conflict dimension. Table 1, below, gives a summary of all 16 indicators. With each dimension, sub-dimension and indicator, the table gives a guiding question that potential cases of political architecture were confronted with in initial rounds of case evaluation. In sum, the 16 indicators served as a valuable guide in cursory investigations of potential cases. They were not meant to exactly measure cases on different dimensions or variables, but to help get a grip of "what might be going on in a case" and to evaluate and sort cases according to how promising they seemed for closer investigation in empirical studies that would support and facilitate theory development.

Table 1: Indicators for the Evaluation of Potential Cases of Political Architecture

Dimension & Guiding Question	Sub-dimension & Guiding Question	Element & Guiding Question	
Resources *Do the actors involved in policymaking possess resources for strategic, long-term policy design?*	-	funding — *Can actors financially afford devising policies and/or evaluating policy drafts?*	a
	-	personnel — *Are actors capable/qualified to devise and/or evaluate policy drafts?*	b
	-	networks — *Are actors included or heard in formal decision making processes?*	c
	-	politics — *Can actors create political pressure on formal decision makers?*	d
Impact *Is the policy likely to impact future developments in the policy field?*	re-distribution — *Does the policy redistribute resources among citizens?*	benefit access — *Does the reform affect or change citizens' access to benefits?*	e
		social rights — *Does the reform affect or change citizens' social rights?*	f
		benefit level — *Does the reform affect or change levels of benefits?*	g
	re-configuration — *Is the policy likely to re-configure the political landscape?*	funding — *Does the reform affect or change the financial base of an agency or organized interests?*	h
		personnel — *Does the reform affect or change bureaucratic or organizational capacities?*	i
		networks — *Does the reform affect or change decision making procedures?*	j
		politics — *Does the reform affect or change mobilization prospects or patterns?*	k
	timing — *Does a "window of opportunity" allow for unusually far-reaching policy reform?*	-	l
Conflict *Is the policy vulnerable to future withdrawal?*	control — *Do actors fear losing power over "their" policy?*	electoral — *Do actors fear being voted out of positions with formal decision-making power?*	m
		network — *Do actors fear being excluded from formal decision making processes?*	n
	contestation — *How contested is the policy issue?*	policy alternative — *Are there viable alternatives that could replace the policy?*	o
		issue salience — *Is the issue of high importance to voters?*	p

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Step 3: Evaluating and Selecting Potential Cases

In a third step, I then evaluated cases of policymaking with the help of the theorized indicators. For pragmatic reasons, I limited my universe of potential cases to Germany (for reasons of language proficiency) in the period from 1966 to 1985 (for reasons of access to parliamentary archives, the main data source). First, I inspected literature on the successes and failures of the different government coalitions during this period in order to determine which reforms were typically considered important achievements of a government and which political debates and conflicts characterized certain periods. For the evaluation of reforms that were repeatedly mentioned in the literature, I then added further literature and performed a cursory review on these cases in order to evaluate their instrumentality for an empirical investigation. In total, I reviewed 11 cases of policymaking. For each case, I prepared a brief description of the policy content and the political context and then continued with an indicator-by-indicator evaluation of the case. For reasons of space limitations, I only include one exemplary case evaluation of the 1972 Works Constitution Act I performed. Table 2, p.29, summarizes the results of all 11 case evaluations.

> The 1972 Works Constitution Act (WCA) reformed one of the central pieces of legislation regulating the cooperation and coordination between employers and workers, and their respective representative bodies. The WCA was first introduced in 1952, regulating the role of work councils in firms, their composition, election procedure and responsibilities, the cooperation between work councils and employers, the participation of workers' representatives in supervisory boards of companies, etc. Even though it granted workers substantial rights, the labor movement considered the 1952 WCA a step back as compared to regulation in force earlier in the 20th century. Twenty years after the passage of the WCA, the Social Democrats led, for the first time in post-war Germany, a coalition government with the Liberal Party and placed huge emphasis on a "politics of inner reforms" and "daring more democracy", thus, fueling unions' hopes to achieve major improvements work place regulation. Furthermore, both Social Democrats as well as Christian Democrats acknowledged the need for a reform of the law. However, despite this shared acknowledgement

and extensive inter-party negotiations, the final act was not passed unanimously but opposed by the Christian Democrats in parliament.

I now evaluate the reform along the list of theorized indicators. Overall, the cursory investigation of the case suggests it to be a *promising to ideal* case since it can be evaluated positively on most indicators. In the *resource dimension,* which concerns the resources political actors possess for long-term, strategic policy design, I evaluate the case positively on all four indicators. The main actors in policymaking in this case, as in labor market politics in general, are the government, formed by Social Democrats and Liberals, the opposition, formed by Christian Democrats, and unions and employers and their respective collective organizations. I expect all of these actors to possess the financial means to engage in policy design and evaluation (a), to be staffed with qualified, experienced personnel (b), to be or have access to formal decision makers (c), and to be able to create political pressure on formal decision makers (d). The three parties, Social Democrats, Christian Democrats, and Liberals, are well-established political players with consolidated membership bases (a), parliamentary experience (a, b, c), varying degrees of governing experience (b) and affiliated party foundations that engage in political education, consulting, and policy design and development offside daily politics (b). Even though the coalition government (1969-1972) under Chancellor Brandt was the first headed by the Social Democrats, the party is one of the two *Volksparteien,* it consistently won more than 30 percent of parliament seats since 1961, and gained experience in the previous Grand Coalition from 1965 to 1969 (b). While the Christian Democrats were for the first time not in government, they still formed the biggest group in parliament with substantial policymaking experience and political influence (b, d). Overall, none of the involved parties was a newcomer to the political business lacking prior experience, political expertise, influence, or organizational resources (a, b, c, d), and I therefore expect them to possess the resources to engage in long-term strategic policy design. The main political actors outside legislative and executive are the unions and employers, as well as their respective federations. They are backed by millions of workers, respectively thousands of well-heeled member companies (a), and are traditionally considered to be important actors in the field of labor market policy (b) with a good standing that enables them to pressure political parties both directly in policymaking as well as publicly through protests or media campaigns (c, d). They have close ties to each of the main parties, with unions being more closely affiliated to Social Democrats, and employers more closely to

Christian Democrats and Liberals (c). I therefore consider them to have the financial resources, the organizational experience and expertise, and the relevant access and political influence to engage in long-term, strategic policy design.

In the *impact dimension*, I evaluate most indicators positively as well. While the WCA reform does not concern citizens' access to certain benefits or benefit levels (e, g), codetermination directly affects fundamental social rights for millions of workers, i.e. their representation and collaboration in firm management (f). Furthermore, the reform seems to likely impact the future development in the policy field. Regulations on codetermination directly affect unions' organizational and financial strength (h, i) because they affect their ability to influence managerial decisions and to organize, recruit and mobilize members. Consequently, they also impact unions' strength vis-à-vis employers and political decision makers (j, k). Employers, on the other side, fear a curtailment of managerial freedoms, harmed economic growth (h), a weakening of their position vis-à-vis unions (k), and subsequently a loss of influence and prestige in the policy field (j, k). Regarding the timing of the reform, I assume that the circumstances were rather friendly towards far-reaching reforms. As noted earlier, the need for reform was generally acknowledged by all big parties. Furthermore, the Social-Liberal coalition coincided with a general breaking open of the German society that was shaken up by student protests, and with a political climate that favored steps towards a democratization of workplace relations (l).

Finally, I evaluate the case positively on all indicators in the *conflict dimension*. The Social-Liberal coalition was the first of its kind, but while it did fit the zeitgeist, Social Democrats and Liberals won the election only by a slight margin. The Christian Democrats still formed the biggest group in parliament (m) and the Social-Liberal government and policies were far from unchallengeable both in the moment and in the future (n). Additionally, the reform was high on the political agenda, had been discussed for years, and was of direct relevance for millions of workers (p), and different policy options were proposed by government and opposition (o).

Overall, the cursory description and evaluation of the 1972 WCA reform suggest that it is a *promising to ideal* case of political architecture. All eight indicators in the resource and conflict dimension are evaluated positively, as well as a majority of the indicators in the impact dimension.

Based on the results of the evaluation of all 11 cases, I then selected the two of the best evaluated cases, the Works Constitution Act of 1972 and the Codetermination Act of 1976, for further empirical investigation of architectural policy design.

Table 2: Evaluation of Potential Cases of Political Architecture

Evaluation symbols: P = positive, P/A = positive to ambiguous, A = ambiguous, N/A = negative to ambiguous, N = negative, – = not applicable.

Dimension		Indicator	Pay Continuation Act of 1970	Works Constitution Act of 1972	Codetermination Act of 1976	Employment Promotion Act of 1985	Reform of the "Strike Paragraph"	Pension Reform of 1972	The Maternity Leave Act of 1979	The Child-Raising Allowance Act of 1985	The Reform of Paragraph 218 of 1974/76	22. Act to Change the Basic Law of	Framework Act for Higher Edu. of 1976
Resources	a	funding	P	P	P	P	–	P	–	P	A	P	P/A
	b	personnel	P	P	P	P	–	P	–	P	N/A	P	P/A
	c	networks	P	P	P	P	–	P	–	P	P/A	P	P
	d	political	P	P	P	P	–	P	–	P	P	P	P
Impact (redistribution, reconfiguration, timing)	e	benefit access	P	N	N	N	–	P	–	P	N	N	N
	f	social rights	P	P	P	P	–	P	–	P	P	N	P
	g	benefit level	P/A	N	N	N	–	P	–	P	N	N	N
	h	funding	P	N	P	P	–	P	–	N	N/A	P	P
	i	personnel	N	P	P	N/A	–	N	–	N/A	N/A	P	P
	j	networks	N	N	P	N/A	–	N	–	N	N	P	P
	k	mobilization	N	P	P	P	–	P/A	–	N/A	N/A	N	P
	l	timing	P	P/A	N/A	P	–	P	–	P	P	P	P/A
Conflict	m	electoral	A	P	A	P/A	–	P	–	P/A	P	N	P
	n	network	A	P	P	P/A	–	N	–	A	P	N	P
	o	policy alternative	P	P	P	A	–	P	–	A	P	A	P/A
	p	issue salience	P/A	P	P	P	–	P	–	A	P	N	A
Overall Evaluation			A	P/A	P/A	P/A	–	P/A	–	A	N/A	N/A	A

Notes: Evaluation of Indicators: ⦿ = positive, ⦿◐ = positive to ambiguous, ○ = ambiguous, ⊖○ = negative to ambiguous, ⊖ = negative /// Overall Evaluation of Case: ⊖ = ideal, ⦿◐ = promising to ideal, ○ = promising, ⊖◐ = suitable to promising, ⊖ = suitable

5. Conclusion

In this paper, I have developed a new strategy for systematic case selection in early phases of our research, when concepts are still vague and theories under development. In response to a critical evaluation of the existing methodological literature on case studies, and based on the growing literature on abductive reasoning, I have suggested a concept of positive instrumental case studies and developed guidelines for selecting such cases. In a nutshell, the guidelines advise researchers to select *positive* cases that likely show the phenomenon of interest as well as potential causes, mechanisms, or affinitive phenomena. The researcher can use an initial concept of the phenomenon of interest in order to develop a set of indicators that help identify likely positive cases and rate their *instrumentality* for empirically investigating said phenomenon. The indicators help quickly evaluate a medium number of cases in cursory literature reviews and to sort them into categories of suitable, promising or ideal case for empirical analysis of the phenomenon of interest and, hence, for gaining a deeper understanding of said phenomenon and developing more precise theoretical expectations.

The contribution of my approach lies in providing a strategy for systematic case selection in the context of discovery, for which no methodological advice is currently available. The current literature follows either the logic of scientific confirmation in deductive research designs, or the inductive building of "grounded theories" from empirical observation. For case study researchers, this means that there is ample advice for how to systematically select cases in ideal-typical deductive research designs, but barely any guidance on how to select cases in their in reality quite "messy" research and when they start engaging with empirical material while developing concepts and theoretical expectations. By developing new methodological advice for sys-

tematic case selection in early phases of our research, my approach improves our ability to develop new theories, frameworks, and concepts. It demands, facilitates and supports more self-awareness about consequential decisions made in early phases of research and advances the growing literature on abduction and research in the "context of discovery". Under the label of abduction, a growing body of literature has in recent years criticized our ability to develop new, interesting theories. My approach emphasizes that we can understand abduction not as a new, prescriptive approach but as a more apt description of how we already do research, and it adds to the literature systematic guidelines for case selection in research that aims to develop new theories and concepts.

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