



## Institute of Political Science Finding a Topic for the Bachelor's Dissertation Handout for Students

### The problem

You have earned all your credits, and now your bachelor's dissertation is suddenly due. But you still lack a topic for this big and important project. At least there are some areas that you find interesting and would like to discover more about. Just what exactly? General curiosity and academic interest are not the same thing – in the same way that a tourist who visits a city and is led by their spontaneous interests takes a different approach to a researcher who is systematically studying a certain historical development in the old quarters. What you need, therefore, is a precise research question. Only in this way can you narrow down your work and concentrate on a single *scientific* aspect of the subject matter that interests you.

Even experienced academics rarely find it easy to condense a topic into a manageable question, for example when looking for a good theme for their doctoral or postdoctoral thesis (habilitation). It is a time-consuming process because the search for promising questions already requires some prior knowledge. It calls for goal-oriented work and not just the "right inspiration". Only someone with a good overview of the current state of research can gauge which questions are still open. This also applies, with some reservations, to bachelor's dissertations. Although no real scientific innovation is expected of these, the research question should nonetheless not be so banal that it could be answered after reading two decent scientific essays. Thus, here too, finding a topic requires that you familiarise yourself with the pertinent debate in your subject at an early stage. But how can I find a topic for myself in this "mound of literature"?

## Possible solutions

First of all, please note that the different subareas of political science also pursue different methodological approaches. For example, political science theories in general and political theory as a part of the overall subject differ regarding the role that empirical and/or normative statements play in 'explaining' or 'understanding' reality. Epistemic interest can lie more in the 'explanation' of facts or contexts or else in the hermeneutic 'understanding' and normative justification of standards and values. The search for a suitable supervisor for your dissertation especially depends on how you intend to plan your topic in terms of methodology.

Thorough reading of the available literature can provide you with a number of starting points for finding a topic:

The 'puzzle': In the literature, phenomena are addressed directly that cannot be easily explained by means of the pertinent theories. Or you yourself can think of empirical examples that seem to contradict the prevailing opinion. Looking for a possible explanation that starts with factors mostly neglected so far can then prove worthwhile. Please note: Only someone with a little existing knowledge can spot such a puzzle. You should therefore be surprised if you have concrete expectations of how it "ought to" be. It is, however, also possible that the explanations offered do not deliver sufficient information about the normative meaning of an empirically verifiable fact. The 'puzzle' might then possibly lie in the lack of clarity – against the background of implicitly or explicitly named values – about what constitutes a problem in the finding in question in the first place.

The research controversy: In the literature, there is open dissent (e.g. between two established schools of thought) on a specific question that you also find interesting. A research question can then be developed from the confrontation of different positions. In a paper with predominantly empirical observations, you must narrow it down to a suitable example (or an interesting sub-question in the controversy) so that it is still manageable for a bachelor's dissertation (this similarly applies for the puzzle too). No one demands from you that you settle this controversy once and for all. But perhaps you can at least answer the question for a certain relevant example (for instance a country) about which side is more likely to be right. A paper with predominantly theoretical-normative observations necessitates a mapping out of the terminological and methodical/conceptual principles on the basis of which the positions diverge.

The question of generalisability: In the literature, a certain explanation (or normative assessment) is widely favoured which is supported by a large number of studies. But is it also applicable to cases that have not yet been examined at all? Perhaps you can analyse another case where the outcome as yet seems unforeseeable. Sometimes it is even worth analysing a previously studied case a second time to see if the original result stands up, for example when there are meanwhile better data or new theoretical approaches. A further question can

be the extent to which generalisability also reaches its limits or fails for methodological reasons, for example because different problem definitions and values are rooted in different historico-cultural contexts.

Unclear development trends: In the literature, assertions about supposed trends are made again and again (e.g. the increasing ideologisation of French parties), but no one so far has really proven them. Examining this question systematically might then be worthwhile, that is, "measuring" whether this purported trend actually exists. Such "measurements" can sometimes also be of academic interest as simple "snapshots". In 'theory-based' papers, such analyses would refer to shifts in attention in academic debates, the concepts and problems on which they are based (such as the diagnostic term 'post-democracy') and corresponding discourse analyses.

Unclear literature situation: You establish that the literature on your subject is untransparent because there is no decent overview and the relevant debates are often taking place in parallel (e.g. because they are being conducted within different disciplines that do not otherwise have close points of contact). In this case, compiling a systematic literature review that pinpoints common questions, compares the respective findings, addresses the strengths and weaknesses of previous contributions and identifies open questions can be a notable academic accomplishment.

## Avoidable errors and useful tips

*Please avoid the 'topicality trap'!* "Why are the sanctions against Russia not working?" sounds like a very interesting research question. But the disadvantage is that your result might be obsolete by the time you submit your dissertation, for example if Russia should suddenly make greater concessions after all. (Apart from which, the data situation is naturally better if the topic relates to the past.)

*Please be careful with controversial issues in day-to-day politics!* "Should Germany advocate a new policy of détente?" is without doubt an important question. But it is more suitable for foreign policy editorials than for academic papers because the answer depends far too much on personal values and attitudes and unverifiable prognoses (regarding the effect of détente policy and its alternatives).

*Please do not attempt to verify your favourite political hypothesis!* If what you really want is just to find further arguments or evidence that your current estimation is correct, you will not be able to conduct systematic and well-balanced research. Instead, look for a question that is completely open from your point of view. You will then also find your research work more exciting!

*Please do not be afraid of a "banal" or ambivalent result!* Real research is an

open process. No one knows beforehand what the end result will be. And the world itself is not always unambiguous either (although this can often only be said after studying it in depth). Treating the question thoroughly is much more important than an "exciting result" or identifying *the one* cause or *the right* analysis. When your work is assessed, the focus will be on your academic approach.

*Please do not take on too much!* Most research questions are much more complex and profound than we tend to think at first. It is better to explore a partial aspect thoroughly than many aspects superficially!

*Obtain an overview early on of the literature and empirical data you might need!* Having a nice research question and a methodologically sophisticated plan of how to proceed are of little help if you are forced to establish that the materials you require are inaccessible (or do not exist at all).

*Think about possible topics well in advance!* You can already make a note of questions that might interest you in the first semesters of your degree programme! If possible, discuss these ideas with your peers too. In this way, you can gain some experience in gauging potential topics a little sooner, and you will not end up in the situation that – having earned all your credits – the "vast mountain" in the shape of the bachelor's dissertation "suddenly" looms up ahead of you. It is sometimes also wise to follow up a successful seminar paper that you completed in an advanced semester. When discussing your term paper with your lecturer, you can also ask them, without any obligation, what they think of this idea. Apart from that and if possible, please bear in mind the need to prepare a topic for your bachelor's dissertation when choosing a seminar in the specialisation module.

*Please also take a look at the following websites!*

- <http://www.uni-frankfurt.de/43403430/Schreibzentrum>
- <https://studi-lektor.de/tipps/schreiben-bachelorarbeit/bachelorarbeit-thema.html>
- <https://studi-lektor.de/tipps/masterarbeit-schreiben/masterarbeit-thema.html>
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