Formulating Research Questions & Designing Research Projects in International Relations[1]

Adam McCauley, University of Oxford
Andrea Ruggeri, University of Oxford

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Nam placuisse nocet
(Perfection brings destruction)
"The Metamorphoses", Ovid

Every researcher must pick a research question. The researcher must turn to her draft, sharpen this question, making it clear and explicit, before polishing it and elaborating on the subsequent research project. Regardless of perspective, philosophy, approach, or assumptions, strong research begins with a clear question. Surely, readers will find all chapters in this handbook extremely useful, for the lucky ones some of these chapters on different methods will be eye-openers. However, not all chapters will be necessary for every research project and researcher. Some will introduce and elaborate on case studies and process tracing (see chapters 59 and 62), others on formal theory (see chapters 3 and 11) and others on specific estimator in multivariate statistics (chapters 33-57). Though, for sure, a scholar will have to face the core issues discussed in this and its companion chapter (see chapter 1). After all, what are you going to study?

In part, this is the most vital aspect of the academic life. Any research, reviewer, or assessor will wonder: i.) what main question does this research is aiming to answer? ii.) What is the relevance of the inquiry? iii.) What contribution will the potential answers make to the wider field? iv.) How rigorous can the answer be? The first three interrogations pertain to the generation of research questions and how they can be phrased most cogently. While researcher demonstrate their rigour in the process of investigation, it is vital to consider whether our

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question lends itself to such an investigation. This chapter explores how we can approach, imagine, and generate research questions in International Relations (IR) and how they open pathways to related research projects. Our mission is to explore the craft of formulating research questions. And as a craft, beyond the exclusive realm of science, this chapter provides wisdom from published works and offers "rules of thumbs" to think systematically about the puzzles that motivate the academic process. Readers more conscious (or concerned) than us about ontological and epistemological issues will notice that we do not explicitly discuss different philosophies of social science and how they relate to the discipline. For the inclined, there are excellent works on the epistemology of IR (Hollis and Smith 1990; J. Fearon and Wendt 2002; Jackson 2016). This decision does not discredit or cheapen a host of "critical" or " normative" approaches to IR research, simply that we are limited by space to include samples instructive and helpful to sharpen our approach to discovering the discoverable, at the frontiers of our discipline[2].

Good ideas, or interesting queries, emerge sometimes at random, or seemingly so, and often as a process of communing with colleagues or peers. The research question is a prerequisite for any academic project, and for all the time spent on methodological training, theoretical instruction, and historical exploration in our field, we devote few formal resources to teaching the art of question development. In handbooks and courses on research design "there is rarely any mention, however, of the specific challenges students face when devising a research question (Bachner 2012, 2)". The chapter is structured as follows: in the first section we discuss where research questions might come from. In the second section, we provide a heuristic trick to move from a specific research question to a more general inquiry—and vice versa. In the third section, we discuss the key conceptual pillars of effective research questions: puzzles, gaps, real world problems, methodological rigour and enjoyment. These categories can guide researchers to craft and develop more relevant and interesting questions for study. In the fourth section, we present some ideal types for research questions and explore how a researcher might develop or deepen them. In the fifth section, we sketch some lessons learned from two important IR research agendas, Democratic Peace and Civil Wars. The chapter concludes with a set of questions scholars often tackle when facing a new research project.

[2] As a Rob Franzese's saying goes: we summarize and report the best we know without implying that is the best out there.

1) Where Research Questions Might Come From

Research questions, like their subsequent answers, are a product of development. Post-facto, it is difficult to explain the origins of scholars' interests in topics such as sanctions, international organizations, military occupation, or civil wars. Even if these roots remain unknown, we can think systematically about how research questions are developed, and the steps we can take to refine, specify, and clarify a research question. Bertrand Russell (2009) claimed: "I do not pretend to start with precise questions. I do not think you can start with anything precise. You have to achieve such precision as you can, as you go along." In fact, the same holds true for the formulation of research questions—insights comes from crafting (and re-drafting) one's inquiries, not merely communing in hope of fruitful epiphany, and the intellectual journey is as important as the destination. What might appear to be driven by intuition or creative accomplishment (at first cut) is the fruit of a systematic commitment to scholarly review. Here, the "literature review" is the tangible "product" of this systematic approach, and aids the scholar to identify extant claims within the literature and, in practice, posit their own question to "explore under what conditions" a potential cause and effect might be identified.

Patience and commitment[3] remain vital for any scholar eager to generate knowledge. Steven Johnson, author of *Where Good Ideas Come From*, writes that "World-changing ideas generally evolve over time – slow hunches that develop, opposed to sudden breakthroughs" (Johnson 2010). Today's researchers can, and should, wield the abundant assets of our digital age. Johnson argues that ideas may well be aided by our better, stronger communication platforms, which allow us access to real-time information in ways never previously thought possible. Our age of communication also facilitates connections between researchers—and these epistemic networks have intellectual weight to be leveraged in search of evocative questions. Studying the complexities of innovation, Steven Johnson illustrates the importance of collaboration, while stressing the importance of preparation. Scholars must study, 'do the work' if they want to be in position for good fortune to strike. Finally, Johnson knows that route to understanding is not carved by successes alone, but the failures that lead to innovations, and refashioning something old to create something new. By knowing as much as we can about what works and what does not, or how things are thought to work and might not, allows us to tackle pressing and potentially catalysing research questions most effectively.

[3] Whereas procrastination and perfectionism are the most dangerous for knowledge generation.

Similarly, Scott Berkun in *The Myths of Innovation*, challenges the very notion of an "epiphany"—a word that suggested all inspiration comes from God—and suggests that ideas emerge from a lifetime of hard work and personal sacrifice. Real innovation, he wrote, emerges from "an infinite number of previous, smaller ideas" (Berkun 2010). Beyond agency (i.e. the "hard work" and "sacrifice") evidence from Alex Pentland's *Social Physics* illustrates the important of structure (or social context) for idea generation. Pentland finds strength in numbers, particularly when they involve free-flowing ideas and a community engaged in the process. These characteristics are essential for innovative and productive societies—and these insights presumably hold for smaller epistemic communities as well (Pentland 2014). Further, Burkus argues that creativity as a condition emerges when an individual can connect different types of knowledge (Burkus 2014). Extending the metaphor to the academic context, creativity emerges when researchers can incorporate consonant and reciprocal knowledge in parallel (but perhaps under-connected) disciplines in new and novel ways. Returning to the practicality of accomplishing this, scholars should look to the literature review as a forum for this considered and engaged intellectual exploration.

There are important differences between using and writing literature reviews, and the best of their kind illustrate how critical this initial survey can be. In practice, we find there are two literature reviews: the first belongs to the researcher privately, and offers a working schematic connecting assumptions, inconsistencies, areas of insight and forethought. The second review, intended and written for the public, works to explain and situate the ensuing project within the discipline. The former can—and often should—be partially sacrificed for the later, where only the necessary items are cited to situate the research project contribution and highlight the extant lacunae or lingering contradictions. Too often early career scholars—and late-stage careerists, too—want to signal their commitment to prior readings by adding unnecessary elements to these literature reviews. Scholars should recognise that each literature review has a logic of exploration and a logic of explanation.[4] The final literature review, based on a logic of explanation, should be like a shopping window displaying only the vital and compelling items. Customers should not have to search through the storage room to find them.

[4] We owe this helpful distinction from a conversation with Monica Duffy Toft.

The methodology of idea generation, however, demands careful review. While the initial stage often focuses on identifying potential puzzles, these early queries must be analyzed, challenged, and sharpened before they can be polished for use. This process of discovery, through systematic reading and questioning, does not preclude creativity or emotional investment, though. In fact, these features can be assets, given "the role of curiosity, indignation, and passion [plays] in the selection and framing of research topics."(Geddes 2003, 27). Is it possible to be systematic and analytic if driven and trigged by indignation and passion? Yes, but it takes practice. Ideas benefit from intellectual clarity. Or, to be more precise, *good* ideas are the product of intellectual clarity. Those good ideas will only be stronger if their discovery is framed by strong research questions.

We stress that this clarity often comes from a commitment to simplicity[5]: drafting a research question and its related project should begin with the most basic and essential assumptions of how causes and effects are related. Consider this the Occam's razor approach to question formulation. Start from simple premises, proceeding with the most basic explanation to build your initial theory. Not unlike the culinary arts, simplicity, combined with fresh and flavourful raw materials, is what distinguishes superior inquiries and research designs.[6] Is creativity something you either possess or not? Can we strengthen our capacity for creativity?

First, knowing how to connect previously underappreciated insights comes from wider engagement with the academic world, with deep reading across the broad expanse of IR. Further, scholars should explore beyond the discipline, too, as intuitions come from political science at large (Putnam 1988) as well as the fields of sociology (A. Wendt 1999), psychology (Levy 1997a), history (Gunitsky 2014; Levy 1997b) and economics (Hafner-Burton et al. 2017). As researchers, it can also help to develop tastes beyond traditional scholarship, to read fiction, consume television series and films, and leverage these created worlds and how they, too, can capture vital truths. With this depth of background and a notebook filled with potential topics of interest, researchers will be quicker to identify unexplored perspectives that may only be laid bare through participation in conferences and targeted (subject-matter) workshops. Further, researchers often overlook the lowest-handing fruit in the discipline, failing to consume or

[5] As Kristian Skrede Gleditsch would put it, simplicity is the simpler version of the term parsimony.
[6] Bruno Munari (1981) compares the passages of research design building to the cooking steps.

digest presidential addresses of an academic associations, which aim to systematically explore the frontiers of academic inquiry. For IR scholars, the International Studies Association (ISA) addresses have been and continue to be enlightening (Brecher 1999; Zinnes 1980; Lake 2010; Acharya 2016).

2. Specific and General Research Questions

Unsurprisingly, academics usually face the hedgehog or fox dilemma (Berlin 2013): Do you want to know a lot about one thing (Hedgehog), or do you want to know a little about many things? (Fox). More tangibly, are you interested in why the Mau Mau rebellion emerged in Kenya between 1952-1960? Or are you interested, more broadly, in how civil wars start? While these questions are related, their relationship is important. While a scholar eager to explain why civil wars emerge might use the Mau Mau as a case of interest, a scholar focused on the Mau Mau will be limited to the lessons learned in this case alone. Solving the hedgehog and fox dilemma, in part, demands a scholar decides what questions she can answer, and how comprehensive these answers can be. At base, it requires the researcher to decide whether they privilege (or desire to show) causal identification or whether questions (i.e. the abiding curiosity itself) ought to be unbound from the methodological strictures.

In Figure 1, entitled "*From specific to abstract (and vice versa*)", we schematize a process of abstraction for a research question that, while used intuitively and implicitly, can be pedagogically useful when visualized systematically. This process mirrors the form of Sartori's ladder of abstraction (Sartori 1970), and provides a step-wise process for a researcher who might only glimpse the wider research agenda for which their question is vital. For purposes of explication, we explore a fictitious individual, Mr Green, a dictator, who, in 1975, faced a military threat from a democratic country ("Blueland"), but decided not to wage war. The event is quite specific, as it focuses on only one person in a specific year and within a precise country. The second step calls for substituting the specific details or attributes with their more general categories. In this way, our proposed strategy is consonant with others who advocate taking inspiration from a specific event or actors before formulating the wider research question (Kellstedt and Whitten 2018). Hence, we replace Mr Green with a general term "dictator", we change a specific date with a broader historical period and we remove the name of a country, with an another broader category, "democratic countries". Of course, this passage is assuming possible different conceptualizations and, then, their relative operationalisations. Though, we suggest these issues (conceptualization, operationalization and measurement) should not limit or constrain the research question generating process, and should be tackled directly in later stages of the research project. In the third step, we explore how the researcher might shed the temporal and geographical coordinates. Importantly, not all research questions should be a-historical or without geographical context. In fact, as researchers we believe quite the opposite (see Figure 3): historical context and geographical location have complex interaction effects with abstract categories in IR (Eckstein 1998; Tilly and Goodin 2006). However, developing a research question demands an early attempt to simplify, eliminating layers of specificity[7], recognizing that these layers, if necessary, can be added later. In our fourth step, we focus on the main agent, the dictator, and lose the opposing regime type. Hence, we end up with a more general research question in which the project shifts from a study of interaction (dyadic type) that might explain war, to a focus on a single regime type (monadic type) and its influence. Step four still contains some scope conditions, qualifying the specific circumstances that we

Figure 1. From specific to abstract (and vice versa)

i. Why Mr Green, in 1975, when facing a military threat from Blueland did not wage war?	i. Specific individuals and historical context. Explicit event or data point.
ii. Why a dictator, during the Cold War, when facing a military from a democracy did not wage war?	ii. Substituting specific attributes with general categories.
iii. Why dictators when facing a military threat from a democracy do not wage war?	iii. Removing historical or geographical context
iv. Why dictators when facing a military threat do not wage war?	iv. Abstracting with some scope
v. Why do dictators decide to go to war?	v. Shortening and general

are most interested in, when a dictator is under military threat. While some puzzles emerge around the absence of an expected event, we suggest that posing a question in positive terms—why that something happened, instead of why that something did not—provides more clarity for the project, particularly as there can be a complex set of causal paths that explain the absence of a phenomenon.[8] This also echoes the preferred analytical posture of defining

[7] Here the parallel is with sculpturing, getting to the gist of it. Hence, the research question generating process should be similar to the craft of sculpture.
[8] This could be labeled as the "Anna Karenina problem": where all happy families are alike and unhappy in their own way. Hence, studying what makes happy could be easier that its lack of.

concepts in positive instead of negative and residual terms (Sartori 1970). Thus, in our final step, we flip the question, shifting our study from an exploration of the phenomenon in the negative (failing to waging war) to the positive (going to war), settling on our final version: Why do dictators decide to go to war? Jessica Week has studied this very question (Weeks 2014), and, while we do not know how she specifically arrived at her core research question, she assuredly leveraged her knowledge of specific cases to find her overarching questions within this new research agenda. Reading through specific case studies and historical accounts can be a useful starting point for exploring the puzzles that remain. Figure 1, viewed in reverse, illustrates a similarly valuable process of evolution, essentially "stepping down" the ladder adding specificity and context to the research question. This concentration of focus—shifting from the general to their specific elements—is a reflex of intellectual development and can be seen in many research agendas. (see Figure 4 and 5)

While ingenuity and originality are vital for scholarly approach, any new work necessarily lives within the ecosystem of extant scholarship. More than justifying the need for their specific research question, scholars must situate their potential contribution within this constellation of past and parallel insights. This means explaining how the resulting findings clarify or elaborate on previous research; how their research remains relevant to either or both scholarly and policy communities; and—often overlooked—whether the aim of the project is feasible. Increasingly, academics will explain the additive benefits of working across and between disciplines, illustrating how the strengths of interdisciplinary co-authors were vital for performing the work. More generally, though, researchers usually try to direct their energy towards projects that cohere to Merton's (1959) invocation that research agendas ought to: 1) focus on interesting and important phenomena in society; 2) lead to new studies of these social phenomena; 3) point to fruitful approaches for such studies; and 4) contribute to further development of the relevant research fields for these studies. In Figure 2 we report a summary of the pillars that are often discussed as frames for motivating a research question (King, Keohane, and Verba 1994; Gustafsson and Hagström 2018; Bachner 2012): 1) a puzzle; 2) a gap; 3) a real world problem and 4) methodological rigour. While research may not satisfy each of these pillars (and that's ok), we have added a fifth, and perhaps most important one: 5) a passion for the topic.

3. Research Questions Pillars

3.1 What's the puzzle?

Most inquiries begin with a compelling puzzle. As Zinnes (1980: 318) puts it, "puzzles are questions, but not every question is necessarily a puzzle." Gustafsson and Hagström (2018) propose a formula that succinctly captures what research puzzles look like: "'Why x despite y?', or 'How did x become possible despite y?', where x and y are two variables that can be isolated and studied. A puzzle formulated in this fashion is, admittedly, a research question, but one requiring much closer familiarity with the state of the art than the basic "why x-question'". You only know you have discovered a puzzle by exploring the previous research. Have you discovered something unexpected? Found conflict where you expected cooperation? Most puzzles, in fact, usually flow from reorganizing prior arguments and findings to highlight their logical tensions and empirical contradictions. This, alone, ought to encourage systematic reading of the literature as the capacity to "connect the dots." A final point about the first pillar refers to the attractiveness of the question. While this measure is subjective, it is related to the second pillar, filling a gap.

Many scholars have been able to start new research agendas on so called 'hot topics', or those relevant and timely for contemporary politics. So timed, these scholars might gain the "first mover" advantage. However, these potential benefits must be weighed against the risks of investing in a research agenda with thinner or weaker systematic prior work. In the early 2000s, the "new kids" on the academic block were the pioneers of the latest generation of civil war study (P. Collier and Hoeffler 2004; J. D. Fearon and Laitin 2003). Today, the first mover advantage is held by scholars of cyber security and artificial intelligence.

Figure 2. Where good research questions come from?

What's the puzzle?	• Are there contradictory findings? • Is an outcome unexplained? • Is the question puzzling per se?	o clashing findings o unexpected outcome o question attractiveness
Filling a gap?	• What do you know? • How main contributions about this differ? • Strong/wrong assumptions previous work? • Where is agency? • Is this gap due to non-interesting topic?	o previous knowledge o contradictions o challenge assumptions o level of analysis o interesting
Real world problem?	• How relevant is this? • Would an non-academic care? • Could it be translated into polices? • Are there ethical implications?	o societal relevance o audience reach o policy implications o academic ethics
Methodological rigour?	• Can you answer this question ? • How can you answer this question? • Can you use new methods? • Are new data necessary?	o scope o method awareness o method sophistication o data awareness
Will you enjoy it?	• Is this topic "yours" or imposed? • Can you add a creative twist?	o autonomy o creativity

3.2 Filing a gap?

As the second pillar in Figure 2 suggests, one natural starting point for a new research project is formulating a research question that finds and fills an extant gap. One way to attempt this is to explore a recent or old debate, conscious of the incongruities, and look for potential weak points in previous theoretical framework or empirical designs. Researchers will benefit from challenging the assumptions that define either the piece of research or the related research agenda. For instance, this might include questioning how actors form preferences (Moravcsik 1997) or whether actors adopt outcome-oriented rationality or process-oriented rationality. (Hirschman 1982) Perhaps extant research has focused too much on material incentives (capabilities, monetary, etc.) and missed important dynamics associated with ideational aspects such as norms, ideas or emotions (Sanín and Wood 2014; Checkel 1998; Petersen 2002). Perhaps there are stubborn, and under-assessed, assumptions about the utility of specific strategies for a given outcome—i.e. that violence or force is an effective mode to drive change. Challenging this assumption, scholars have developed pathbreaking work on the importance and influence of non-violent protest and its influence on regime change (Chenoweth and Stephan 2011).

Challenging assumptions has a long history of sparking new research agendas in IR: where does action take place (Singer 1961)? Where is the agency (A. E. Wendt 1987)? Waltz (1959) wrote one of the core IR texts focusing mostly on this issue: At what level should we analyse international politics? At the level of the individual, the state/regime, or international structure? Therefore, when we think about research questions and aim to formulate new research agendas, it can help to identify whether the phenomenon can be scrutinised either at the micro level (for instance individuals), the macro level (structural features) or some mix of both. Increasingly, scholars have identified the value of an intermediate layer, called the meso-level, where different organizational configurations (an identity group, the state and international organization) can connect both the micro and macro levels of analysis.

3.3 Real world problems?

Enterprising research often aims to respond to "real world problems." In part, this speaks to the utility of the intellectual practice: "Our task is to probe the deeper sources of action in world politics, and to speak truth to power — insofar as we can discern what the truth is" (Keohane 2009). Karl Deutsch (1970) when discussing a new edition of Quincey Wright's *A Study of War* wrote "war must be abolished, but to be abolished must be studied." Pathbreaking research on security and strategy by Schelling (1960), clearly engaged with the issues in his contemporary period. The researcher ought to ask whether their research question has consequence to wider society and who might be influenced by its findings. Another way to orient our thinking is to explore whether, and what kind, of non-academic[9] may find value in the work. For non-academics, the incentive structure of publications and tenure-track preparation are replaced by a more tangible perspective of the work's value for policy implications, implementation strategy, effectiveness, and efficiency. These non-academics may be government analysts, politicians, practitioners of NGOs, or civil servants at major inter-governmental bodies. This should push scholars to avoid the crutch of academic jargon and avoid focusing on niche issues with unclear implications. Beyond a question of theme or topic, researchers should work tirelessly for clarity in thought and communication. Empirical sophistication is no excuse for sloppy, senselessly convoluted style. Researchers would be well-served to invest energy in crafting clear and "economical" prose (McCloskey 1985).

[9] To be noted the difference between the "grandma test" and the "policy-maker test". The former is about clarity and communication of a research project, the latter about its implications and should answer the non-trivial question "so what?".

Despite perennial and contentious debates over the different responsibilities of the professional academic and the policy maker, IR scholars have learned few lessons. First, if authors are clear about possible policy implications, policy makers will be more apt to read and assess their research. Second, clarity of argument and assertions should be considered a duty, particularly if your research resonates with contemporary policy challenges. If the policy implications are not made explicit, policy makers are apt to infer their own conclusions, potentially using and misusing vital research. One debate around the 2003 invasion of Iraq invasion focused on the Bush administration's misuse of the democratic peace literature in legitimizing the military intervention (Ish-Shalom 2008). This point speaks to our question in the Figure 2: What are the ethical implications of study? Whether in terms of methodological approach and practice of gathering evidence, or the effect of publishing publicly on findings with clear social costs, it is incumbent on the researcher to fully understand the consequences of their work—for themselves and everyone they have involved. Academic institutions have strict procedures and process to assure that who will participate to a research project will not be facing any risk. Supervisors and senior colleagues should take care to assist and advise, where relevant, early career scholars, given the intrinsic dangers of studying specific phenomena within international relations.[10]

3.4 Methodological Rigour?

The fourth pillar, about methodological rigour, is an important concern within Political Science and IR. Our first question is trenchant though problematic: If facing a new research question, will you have the necessary data and methods to find an answer? This question is a necessary first inquiry, as it may also be the last: supervisors and more experienced scholar will be first to question the feasibility of a research project and, with concerns, try to refocus the core question. At times, these good intentions are misplaced, and these individuals are wrong because they are not creative or visionary enough to see the potential. This can be additionally challenging because it is difficult to know what methods and data will be most effective, and if they are available, early in the research process. Effective review of methods and existing data is a critical step, and the following chapters in this handbook will explore the wide, and widening, toolkit for IR scholars. Additionally, scholars should discern whether the requisite data exists, and not to despair immediately if the answer is unclear. A range of successful

[10] We want to remember here the awful death of Giulio Regeni in Cairo. He was a graduate student who was doing field research. This chapter has been written also in his memory.

research projects are based on data gathering (Singer and Small 1994; Sarkees and Schafer 2000; Vogt et al. 2015; Sundberg and Melander 2013; Tierney et al. 2011). However, one should be cautious as large data gathering is quite demanding—most of the existing datasets are the product of years of research and obtained by armies of coders or advanced automated data gathering. A single PhD student or early career scholar could face serious challenges if the data gathering is not thought through carefully. Pragmatism is an asset in assessing the data realities.

3.5 Will you enjoy it?

Think carefully whether the research project is one of your choosing or, to greater or lesser extent, imposed by someone else. This imposition might take the form of a supervisor or academic peers. Your research question and project may also be the path dependent by-product of previous studies or successful essays. Starting a fresh research agenda is quite rare in an academic life, akin to a luxury. Moreover, as research is 10% inspiration and 90% perspiration, this means that while the creative mind is an asset, the net result will depend on commitment and hard work and how you privilege research time. Enjoyment is an important quality that may assist in motivating scholars to finish their project. Remember, however, the project is your own. It can be enervating and ultimately more satisfying to add your own creative twist.

As you might imagine, it is hard to provide strict guidelines here, but reading extensively through sister disciplines (sociology, economics, psychology) and the humanities (history, literature, archaeology etc.) can help. Further, while articles are useful and provide an efficient source of information, subject-matter books provide a larger perspective and may be the loci of many additional creative questions and new research projects. Further insight can be gleaned by asking non-political science colleagues about new methods in their own fields, how their disciplines orient their studies (at the individual level or systems level), and what assumptions they make about agents and actions. Recall again that researchers will also benefit from reading fiction and watching movies, as these works can stimulate strong intuitive ideas about the social world. Too often graduate students and early careers scholars privilege the niche literature from which their initial query emerged.[11]

[11] We do not imply at all that more senior scholars do not commit this sin, reading just what belong to their "niche church". Though, those senior scholars who belong to that church are lost souls and nothing can be done.

4. **Developing Research Questions**

Having discussed where research questions come from and what features should be part of their relative research projects, we shift our focus to a range possible research questions (Figure 3). We do not suggest that these questions are exhaustive, but they do provide a starting point and set of pathways to compare how to define and craft research questions. Broadly speaking there are two types of research questions: the first focuses on the main phenomenon to be explained (Y or *explanandum*). The second questions concern with factors that can explain variation of that Y. Hence, this second form focuses on the X (or *explanans*), or independent variables. Generally, we can explore either the Y-focus or X-focus research questions.[12]

Figure 3. On research question types

1. What is Y?	→	• What is power?
2. How has Y changed?	→	• How has trade increased?
3. Why Y?	→	• Why war?
4. Under what conditions Y?	→	• Under what conditions peace?
5. Do Y and X covary?	→	• Do democracy and peace correlate?
6. Does X cause Y?	→	• Do IOs cause cooperation?
7. What is the effect of X on Y?	→	• What is the effect of aid on civil war?
8. Is the effect of X on Y mitigated by Z?	→	• Is the effect of democracy on war conditional on trade?
9. Why Y varies across G or T?	→	• Why level of cooperation varies across regions?
10. Why X affect Y in T but not in T-1?	→	• Why alliances influence risk of war differently over time?

The first ideal type, "what is Y?" is the most focused embrace of the explenandum-oriented research question. It his highly theoretical, investing in heavy conceptualization and likely the creation of creation of typologies will be part of sub-research questions (D. Collier, LaPorte, and Seawright 2012; Gerring 1999). These questions will demand empirics to describe different trends and types (Gerring 2012), though several empirical methods may be used. As an example, we pose a question on power (Baldwin 2012), but can identify a range of similar forms of inquiry in IR on the concept of security (Baldwin 1997), human security (Paris 2001) and securitization (Buzan 2008). Sambanis (2004) and the Kalyvas (2010) produced works which sought "just" to define civil war, for instance. The second ideal type remains focused on Y, but instead of mere definition and conceptualization of Y, these projects might define how

[12] Stathis Kalyvas, personal communication, suggests that there are Y-focus and X-focus research projects, and often scholars stick to this divide in their careers. It is the case that certain methods work more for Y-focus questions, qualitative methods and quantitative methods for X-focus question. Though, again, this only serves as a rule of thumb and not a law.

the Y has changed over time and space. Research agendas such as those charting countries' Polity IV scores (Marshall and Jaggers 2002) or calculating their position on the V-Dem spectrum (Coppedge et al. 2017) belong to this family of research questions.

The third type, while still Y focused, works to study an explanation and data generating process, and, in part, might be useful for elaborating and studying mechanisms (Tilly 2001; Hedström and Ylikoski 2010). One of the most cited article on processes leading to war, "Rationalist Explanations of War" (Fearon 1995), belongs to this type. Why do we see war? This paper, even without a strict commitment to empirical details, has been one of the most important papers on the possible explanations of war outbreak.

The natural extension, or slightly different elaboration, involves evoking the classic phrasal tool "*under what conditions...".* This framing is useful because it pushes the researcher to think about variation of Y, their central variable of study, as well as the influence owing to variation of in those possible explanatory factors—the Xs. Hence, "*under what conditions*" suggests we should be conscious of co-variation, which is vital for the social sciences.

Thus, it follows that for questions where both Y and X are explicit, the choice of adjective or verb can suggest a correlational nature. Researchers should be cautioned against slipping towards making unfounded causal claims, however. Are democracies richer? Do democracies fight each other less than they fight other regime types? Do countries who are members in many IOs trade more than countries with fewer memberships?

The explicit causal relationship between X and Y is present in our sixth ideal type: Does X cause Y? At some point in our scholarly past, quantitative scholars were less concerned about the differences between correlational and causal claims. Many scholars wrongly assumed that endogenous effects (those that emerge within the phenomena of study) could be suitably controlled by lagging (modulating the expected influence) the relevant variables and, if specific scope controls were used on these variables, academics could discern the omitted causal effects. However, the debate on causal identification among scholars in economics subsequently influenced research projects in Political Science (Angrist and Pischke 2008). This growing sensitivity has generated manifestos on how to conduct research using strict identification strategies, specifically for those who study conflict (Samii 2016). Increasingly, debates have emerged around projects with apparently solid quantitative causal identification strategies that can be challenged when qualitative and archival information casts doubt on this process of exogenous identification (Kocher and Monteiro 2016). Also qualitative scholars are increasingly aware of the high bar of causality, leading to the development of more sophisticated approaches to methods like process tracing (Bennett and Checkel 2014).

For the last three ideal types (see Figure 3 above), we add one layer of complexity. First, these questions ask whether the effect of X on an outcome is conditional or mitigated by another factor Z. In our ideal type, we might ask about the effect of domestic regime type on the odds of intentional war, and assess whether this is conditional on the level of trade interdependence between states. These questions tend to dynamise a research agenda by pushing researchers beyond inquiries on Xs and Ys alone. These studies also allow researchers to introduce theoretical sophistication. At present, we find evidence that quantitative literature scholars have been tackling these questions by studying interactions between involved variables (Brambor, Clark, and Golder 2006; Franzese and Kam 2009), and qualitative scholars have thought about conditionality and contextuality more systematically (Tilly and Goodin 2006).The last two questions offer variations of the conditional questions, but stress how we could think about variation of Y in different historical or temporal moments, and across geographic space. Stated simply, these research questions allow scholars to ask whether the effects of those Xs on Y are conditional, or fluctuate, over time and space.

5. Examples from IR Literature: Democratic Peace and Civil Wars

Hereafter, we sketch some lessons learned looking (briefly) into two research agendas in IR: democratic peace and civil war. Readers who are not so interested on security in IR could try to draw parallel with other IR literatures, however as Keohane points out: "[t]he study of world politics begins with the study of war" before turning to the vital question: "Why is war a perennial institution of international society and what variable factors affect its incidence?" (Keohane 2009).

Figure 4: Democratic Peace Agenda

- Causes of war?
  - Specification : Are democracies more peaceful?
    - Further specification: democracies do not fight each other?
- Mechanisms : Why democracies do not fight? Norms, structure, endogeneity
- Criticisms: Correlation no causation, conceptualization problem , omitted variable bias, rare event, unrepresented sample
  - Reactions: larger samples, further controls, different methods
  - Extensions: international organizations, trade and democracies
- Alternative within: is it capitalism or democracy?
  - Technical discussions : specifications, operationalization…
  - New methods: survey experiments…

In Figure 4, we briefly (and non-comprehensively) sketch a trajectory of research questions and topics on the Democratic Peace. This has been one of the latest debates where paradigms in the study of IR remain contentious.

The research agenda began with a broader research question, with subsequent study refining more stringent scope conditions, and improved accuracy of empirical findings. Subsequent debates have centred on how to make sense of the correlational findings: if democracies do not fight each other, we need mechanisms to explain why this might be the case (Maoz and Russett 1993). In response, several papers, mostly form a Realist perspective, formulated research questions to challenge the causation pattern (Rosato 2003) the conceptualization, and hint towards possible omitted variable bias. Given the rarity of the event (both war and specifically wars that might include democracies) critics believed it difficult, if not foolhardy, to infer from the small size of the sample. These charges sparked reactions and respondent research offering further explanations, finding cause for the seemingly pacific relations between democracies in variables such as trade or international organization (Oneal and Russett 1999; Russett and Oneal 2001)

Subsequent developments came from the non-Realist perspective, where researchers focused on capitalism (Gartzke 2007), then followed again by (mostly technical) attacks against Gartzke's paper (Dafoe 2011) and the use of new methods to assess micro foundation that purported to explain the phenomenon (Tomz and Weeks 2013)

Figure 5. Research Projects on Civil War

- i.) What does explain variation in civil war (cw) onset?
  - plethora of Xs
  - ii.) Refocus on cw facets: durations, intensity, civilians' victimization, outcomes, legacy…
    - iii.) Analytical and empirical refocus: dyad, transnational, groups, local data, leaders…
      - iv.)Refocus on actors' actions: governance, alliances, splintering, displacement, crime…

Figure 5 offers a concise schematic of how research on civil wars has evolved. While the study of civil wars did not commence in the 2000s (Kalyvas 2010), the decade marked a renaissance, given their growing frequency and prior developments that naturally led to systemic studies of how civil wars begin (J. D. Fearon and Laitin 2003; P. Collier and Hoeffler 2004).

This period of academic exploration offered a range of arguments about the related variables (the Xs) that were related to the onset of civil wars, expanding the study to more than simply GDP or ethnicity. Many of these studies sought to explain why we saw variation in the Y (civil wars), based on capabilities (of host states and non-state actors), a country's exports, their domestic share of natural resources, and demographic patterns, just to name a few.

Subsequent scholars looked to unpack and explore different facets of that Y (civil wars). This scholarship looked carefully at potential explanations for civil war duration, intensity, and violence against civilians (Kalyvas 2006). And while some unpacked the specifics of the Y, other researchers focused different levels of analysis working to match the theoretical and empirical ones, called the disaggregation approach (Cederman and Gleditsch 2009). This work was supplemented by related data gathering projects on the actors, or dyads involved (Cunningham, Gleditsch, and Salehyan 2009), transnational linkages between these actors (Gleditsch 2007), relevant local features (Sundberg and Melander 2013) and the role of leaders (Prorok 2016).

A more recent wave of research has explored further ramifications of civil war, driving a turn towards vital qualitative work under a mixed methods framework. This research has explored rebel actions and practices including governance (Arjona 2016), their system of alliances (Christia 2012), how they splintering , and their influence on displacement (Steele 2009).

And now, we are starting to explore how myriad processes leading to political violence may be related. These connections include the substitution effects that may present between civil wars and terrorism (Polo and Gleditsch 2016) or civil war and regime change (Roessler 2016)). Further, as violence in intrastate or civil wars fluctuates drastically, scholars are eager to learn lessons from the study of organized and transnational crime (Kalyvas 2015) to sharpen our explanatory mechanisms and gain additional clarity on how conflict, today, emerges, endures and ought to be responded to.

Similar to our previous schematic on democratic peace, the evolution of civil war literature mirrors a kind of professional call and response, with each subsequent scholar re-assessing the assumptions and models deployed in service of knowledge. Finding room for improvement or cause to doubt extant claims, researchers are challenged to sharpen their opening intuitions about both the theoretical and empirical worlds. Progress is built on failure, but we fail forwards, eager to find solutions to those puzzles that linger.

Hence, research agendas progress through systematic refinement. On the one hand, the initial iterations can involve conceptual thinking about the nature of a specific condition – "what is security" – which opens a space for inquiry. Often the "big think" projects provide specific gravity, attracting scholars who look to splice the projects core assumptions, its level of analysis, or consequent conclusions.

Innovation during this second phase usually comes from leveraging new methods or refinements of the mechanisms that connect those Xs with Ys. These interventions offer competing explanations for empirical or observable realities, which, in turn, spark interest among a new wave of scholars to delve into some of the under-explored particularities or facets of these phenomena.

Finally, as that initial or top-level inquiry settles into position as a well-worn research agenda, scholars are likely to review and refine how each of these waves of research have contributed to, or detracted from, our understanding of the initial puzzle. These reviews of the "state of the art" are often vital to reinvigorate scholars to assess where the frontiers of research lie, and whether methodological innovations and improvements might offer contemporary scholarship a solve old debates.

6. Conclusions

An effective test for any scholar is to apply the "elevator test". If pressed, can you succinctly explain your research questions and project aims, in under 45 seconds?[13] While initially arbitrary, the question is necessary to ensure the clarity of your work. Honing this short and compelling pitch is only stage one, however. Like the acquisition of all knowledge, the true insights are reliant on commitment, iteration, and a significant amount or writing, thinking, and writing again: Research ideas, research questions and research projects must be written and rewritten to be understood. And only when understood, can they be written to communicate that knowledge to others: "You do not learn the details of an argument until writing it up in detail, and in writing up the details you will often uncover a flaw in the fundamentals" (McCloskey 1985, 189)

More practically, journals and some book publishers will often ask a reviewer to assess how this research manuscript fits in one of four categories. Is the work: I) a relevant contribution to a relevant topic; II) an irrelevant contribution to a relevant topic; III) a relevant contribution to an irrelevant topic; IV) an irrelevant contribution to an irrelevant topic. While researchers should clearly avoid questions and projects that might strand them in the fourth category, trade-offs during the research process may mistakenly lead some individuals into the second and third categories. Anyone undertaking early research into their topic should be conscious of how to position yourself in, and remain within, that first category.

Moreover, as Brian Burgoon[14] suggests, research projects are not simply good or bad, but they can be either good or bad across a range of characteristics: from analytical and theoretical quality to methodological rigour. In short, research questions and related agendas should be weighed across a range of criteria, many of which are captured in depth in this handbook. However, adopting Burgoon phrasing again, it is important to ask whether research can be "creative and inspiring in a swashbuckling way". This new axis where creativity or inspiration matters captures those features that signal a research agenda can make a difference. To be

[13] This is what Todd Landman call the "elevator test". However, this is not just a hypothetical test, intellectual exchange when on a lift are frequent interactions at academic conferences and, therefore, is good to be able to pass this test if you aim to increase the audience to your panel of perhaps stimulate the appetite for potential reader of your work in progress.

[14] His point is about IR and Political Science books. Personal communication and his elaboration is from Charles Sabel categorization.

rigorous and extremely innovative may well be an asset, but every researcher must commit, at base, to be considerate and cognizant of the related costs and benefits before they proceed.
To close our chapter, this section includes common questions and brief replies that might be instructive to readers.

i. How specific should a research question be?

From "why do civil wars start?" to "why the Mau Mau rebelled against the UK government?", there are varying degree of research breadth. However, as we highlighted in our discussion (related to Figure 1), scholars can ask general questions and explore through specific cases, or vice versa. The issue remains what scope conditions you select and how generalizable you seek to be. There are tradeoffs on either end of this spectrum.

ii. Shall I only select questions I can fully answer?

This is a critical question and perhaps there could be some contention how to answer it. When discussing Figure 2, we stressed the tradeoff between originality and feasibility. However, we also suggest that there are myriad ways of assessing the potential and value of research agendas. Our advice is to be more curious than concerned in the exploratory phase, while rationally assessing the feasibility before you commit to the project over the longer period. Creativity and innovation are important features of any research projects.

iii. How useful is a literature review when posing a research question?

We touched upon this issue in several passages of this chapter: researchers with a thorough knowledge of previous research (in content and method) will have demonstrable advantage over those without. Remember the distinction between a logic of exploration and a logic of explanation when writing literature reviews. The former is crucial for posing a proper research question, the latter is vital when writing in a piece of research and situating its contribution and relevance.

iv. Do I need to justify my research question?

Referring back Figure 2, researchers must to more than justifying their topic—you need to situate your research question. What previous research are you engaging with/challenging? What research streams does your project bring together? What is your intended contribution (what will we learn within IR?) and why is this research relevance (why we should care?)

v. What do I need to define in my research question?

Definitions are central for research, but their importance vary with the type of research question you are engaging with (See Figure 3). If your question is a Y-focus query, most of the research will be about conceptualization, defining ideal-types, and discussing typologies. However, X-focused research question will also require clear definitions, but this will likely occur at a later stage in the research design.

vi. How many research questions?

A good rule for any piece or writing or research is one paragraph, one idea. Applied to research, a reasonable rule of thumb might be a paper should have one dominant research question, a book or thesis might have more, usually closely related. This predominantly takes the form of a large research question with different, but reinforcing, sub-questions that can improve the comprehensiveness of the project. Remember, the more the research questions, the harder to answer them. Less is more, particularly if the less is more comprehensively studied and argued.

vii. Do I need to keep the same dependent variable?

Usually, yes. It can be disorienting for a reader, unless your central argument is about a selection process (Z→ X, then X → Y) or, though then facing a challenging research design, recursive processes (X ↔Y). Moreover, when you are formulating hypotheses, it is good practice to keep the Y consistent. Clarity rhymes with consistency.

viii. Do I need only one main explanatory variable?

Not necessarily. International relations are complex, often several factors are necessary to account for variation of IR phenomena. However, in early stage projects scholars often confound main explanatory factors, core contributions of the research project, with controls, other factors that could be affecting the Y. Not all Xs should be placed under the spotlight. Focus on those Xs that make a contribution and remain essential to your research question.

ix. Can I change my research question?

There are several way to interpret this question: 1) as change of topic; 2) as change of research question. The former is beyond our scope, as it comes down to a researcher or their student/supervisor relationship. The latter, however, may simply emerge as the researcher reads more of the literature, strengthens their theoretical argument, and advances with empirical analysis. Not all change is bad. But be purposeful when you commit to it.

x. Do I need to specific actors, preferences, strategies and other main components of my theory in the research question?

Not necessarily, but they can help to formulate the research question and project directives (See Figure 3). Understanding the level of analysis, where agency is located, what preferences present (and how they are formed) can be crucial. These details help us explain the strategies actors embrace as well as the structural constraints and ideational factors that might influence the phenomena under scrutiny. After all, the devil is in the details. This information can be essential element for theorizing and, thus, refining your research question.

Finally, the above chapter was always going to be hampered by our own subjectivity. In any domain where creativity is central and where routes to knowledge are variable, variant, and – in part – deeply personal, we run the risk of dissuading researchers from following what might be essential, and individual, insights. To the best of our ability, we have tried to be systematic and egalitarian in our treatment of differences. By capturing our experience, distilling our contextual wisdom within IR research, and highlighting some perennial assets for any researcher, we hope this chapter can be useful without being rigid. If idea generation is more craft than science, than in lieu of being scientists, we ought to commit to becoming better craftspeople.

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