Choosing What to Study: Motivations, Epistemic Values, and Reflections on Research Practice

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Research Master Thesis

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Date of Submission

27 October 2025

Acknowledgments

I would like to thank the 18 researchers who generously shared their time and perspectives with me. I am especially grateful for the openness with which they spoke about their experiences and for devoting an entire hour to this project—only through their commitment could this work have been realized.

I also wish to thank the four pilot participants, whose feedback during the piloting stage helped refine the instruments, as well as Dr. Vlasta Sikimić who provided valuable feedback on the newly developed questionnaire.

I am deeply grateful to my supervisors Anna, Daniel, and Sajedeh, for their thoughtful guidance and inspiring ideas, and for inviting me to workshops. Most of all, I am thankful for their contagious passion for research, which made this project such a rewarding experience.

Abstract

This study examines how researchers choose specific projects, linking individual motivations and values to the situational and institutional contexts in which decisions are made. Building on a person–situation perspective and the intention–behavior gap, it investigates when choices align with—or deviate from—stated epistemic and non-epistemic motivations and values. A mixed-methods design combined two survey measures of dispositional motivations and values with semi-structured interviews across different career stages and disciplines within the Social Sciences.

Curiosity emerged as strong motivation for project choice. Recognition was viewed along a spectrum—from career necessity and peer validation to being explicitly dismissed as a driver. Mentorship shaped not only intellectual direction but also ethical orientation and academic identity. Field communities, collaborations, and interdisciplinary contexts influenced topic selection by revealing gaps, offering expertise, and expanding perspectives, though coordination challenges were common. Many participants criticized the current funding system, saying it pressures researchers to focus on projects that are most likely to get funded. Still, most found creative ways to adapt to limited resources. Societal relevance motivated many, while the need to balance fundamental inquiry with practical application was also relevant. Innovation was valued as methodological advances or cumulative problem-solving rather than novelty for its own sake. Researchers described their contributions to science as multifaceted, spanning theoretical, methodological, and contextual advances. They outlined a wide range of qualities that make research valuable, emphasizing clarity, honesty, rigor, replicability, and societal relevance. Reflections revealed varied gaps between what researchers want to do and what they actually do in practice, shaped by time constraints, funding limitations, and institutional incentives, with career stage influencing autonomy and risk-taking.

By integrating epistemic and non-epistemic motivations and values with situational constraints, the study clarifies how and why project choices diverge from ideals, and identifies practical levers (e.g., better coordination, incentive design) to better align individual decisions with collective epistemic goals.

Keywords: problem choice; research agenda; epistemic motivation; epistemic value; intention–behavior gap

Layman's Abstract

This study explores how researchers decide which projects to work on and what influences those choices. It looks at both personal motivations, values, and external factors like funding systems and academic culture. The study combines survey data and in-depth interviews with professors in the Social Sciences.

Curiosity was a driver behind researchers' work. Recognition mattered for some, especially early in their careers, but others viewed it as less important or even undesirable. Mentors played a key role in shaping researchers' thinking, ethics, and working style. Communities, collaborations, and interdisciplinary connections also influenced topic choice by opening new ideas and research opportunities, though they could be difficult to manage. Many participants criticized the current funding system, saying it limits creativity and pushes researchers to focus on what is most likely to get funded. Despite this, researchers found ways to adapt and continue pursuing work they care about. All researchers wanted their research to make a difference to society. Innovation was valued when it improved methods or deepened understanding, rather than simply producing something new. When reflecting on their work, researchers often noticed differences between what they ideally wanted to do and what they could do in practice—mainly because of time, funding, and institutional pressures. Such tensions were most pronounced for early-career researchers, who often felt constrained by limited autonomy and career pressures.

Overall, the study shows that research choices are shaped by both personal motivations and values and external constraints. Understanding this relationship can help universities and funding agencies create better research systems that strengthen science and contribute to society.

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Introduction

The Importance of Problem Choice in Science

The shape and quality of our science depend as never before upon the nature and quality of the problem choices made by our scientists. […] We all know, of course, that much discovery is serendipitous or opportunistic, that much research is admittedly exploratory, and that the published results of an investigation are a poor guide to the original purposes for which it was undertaken.

— Ziman (1987)

Throughout their career, researchers make decisions about which scientific questions and projects to pursue. These choices are far from trivial: selecting a research problem determines not only an individual scientist's trajectory and career success but also the direction, efficiency, and quality of scientific progress. As Ziman (1987) emphasized, a scientist who chooses an unproductive problem "can never hope to produce valuable research results," whereas choosing good problems is a prerequisite to staying "in the game." In other words, the choice of research agenda is a pivotal act of knowledge creation and shapes which projects are pursued.

At the broadest level, scientific research is an investment of resources aimed at improving knowledge and society. Meta-research estimates suggest that a large fraction of research effort is wasted, often by asking questions already answered or not connected to prior evidence (Chalmers & Glasziou, 2009). Inefficiencies originate upstream, at the stage of project selection. Researchers have pointed out that the preference for novelty over iterative improvement of existing evidence hinders the scientific community from establishing a cumulative and reliable knowledge base (Elson et al., 2023).

Thus, the projects scientists choose to pursue do not merely determine individual career trajectories but collectively shape the direction and efficiency of scientific progress. When research projects are driven primarily by the pursuit of novelty rather than the systematic accumulation of knowledge, science risks producing fragmented insights rather than coherent understanding. To understand and ultimately improve this process, it becomes crucial to examine how researchers actually make choices about which projects to pursue.

Criteria for Problem Choice

A distinction is made between problem choice and research agenda. *Problem choice* refers to the selection of a specific question or project to pursue at a given moment. R*esearch agenda*, in contrast, denotes the broader and longer-term pattern of topics, questions, and strategies that

characterize a researcher's program of inquiry. While these concepts are closely related, the agenda provides the overarching orientation within which particular problem choices are made.

Classic sociological work (e.g., Zuckerman & Cole, 1994) has provided valuable insight into how scientists select their research problems. Zuckerman and Cole (1994) described three criteria for problem choice: the significance of the problem, its feasibility (i.e., whether the researcher can readily solve it), and its duration (i.e., the estimated time needed to produce the anticipated results). The significance of a problem consists of two aspects. On the one hand, a problem holds a certain importance for the researchers themselves. On the other hand, significance is also determined by the response of the scientific community. The latter points to evaluation and reward mechanisms, such as peer recognition, which affect researchers and are linked to the influence of collaborators and peers on the decision to choose a certain problem.

Extending the focus on feasibility, Fujimura (1987) introduced the concept of constructing "do-able problems" in cancer research. The author showed how researchers align scientific, technical, and social factors to make a problem feasible, fundable, and meaningful. Thus, problem choice is not just about curiosity theory but about articulating alignment among different constraints such as lab resources, funding, and peer expectations.

Together, these studies show that problem choice is both a strategic and social act, shaped by the perceived importance of the problem, the scientific community, and the resources available for research.

Understanding Problem Choice and Research Agendas

To study how researchers set their research agendas, a particularly influential line of work has been developed by Horta and Santos and colleagues (Horta & Santos, 2016, 2020; Santos et al., 2022; Santos & Horta, 2018). Their research makes multiple contributions to understanding research agenda setting by developing and refining a multidimensional framework that captures how individual researchers establish and adapt their research focus.

As an important first step, Horta and Santos (2016) developed and validated the Multi-Dimensional Research Agendas Inventory (MDRAI) to assess how researchers establish their research agendas. They identified eight key dimensions that define research agenda setting: Scientific Ambition (i.e., the degree to which a researcher aims to be a leading figure in their field), convergence (i.e., the tendency to focus on a specific research topic and consolidate expertise), divergence (i.e., the inclination to explore multiple, often unrelated research topics), discovery (i.e., the pursuit of high-risk, high-reward research), conservative research (i.e., a preference for

established, well-supported research topics), tolerance for low funding (i.e., the ability to pursue research despite limited financial resources), mentor influence (i.e., the extent to which an academic's research agenda is shaped by their PhD advisor or early-career mentors), and collaboration (i.e., the degree of engagement in collaborative research networks). These dimensions were later used to classify researchers into different agenda profiles, providing a more systematic understanding of the strategic orientations underlying research choices.

Santos and Horta (2018), consequently, classified higher education researchers into two distinct research agenda clusters: Cohesive researchers and trailblazing researchers. Cohesive researchers focus on the development of expertise in a field. Their research agenda is mostly disciplinary and requires less collaboration. Trailblazing researchers are motivated to expand their field of research to other fields and are inclined to engage in collaborative projects and multidisciplinary research. This research agenda cluster is considered high-risk in comparison to the low-risk character of cohesive research agendas.

To further broaden the applicability of their framework, Horta and Santos (2020) refined the original MDRAI, which had been designed primarily for social sciences. The result is the MDRAI-R, an inventory that incorporated two new dimensions to widen its scope of application. One of the new dimensions, Academia-Driven Research, captures the extent to which research is shaped by normative traits of the academic environment. The second, Society-Driven Research, measures the focus of a research agenda on solving challenges in society.

Finally, Santos et al. (2022) extend the MDRAI-R framework by examining the relationship between researchers' strategic research agendas and their preferences for basic research, applied research, and experimental development. Only a relatively small number of researchers pursues both basic and applied research, and experimental development. The majority of researchers pursue either basic or applied research. They also find that more researchers preferred applied research over basic research.

These studies are complemented by Horlings and Gurney (2013), who examined how research strategies evolve across academic careers, demonstrating that researchers follow distinct patterns when selecting their research topics. They found that researchers' portfolios consisted of multiple research lines that were not pursued sequentially but often overlapped, first focused narrowly before broadening later on.

Complementing these findings, McBeath and Hopkins (2024) examined the strategies researchers use when selecting their research topics. The study is based on interviews with 18

researchers across different career stages and disciplines. The study identified three primary strategies that researchers employ when choosing and modifying their research agendas. One factor is self-reflection and personal interests: many researchers report choosing their topics based on interest and personal experiences. Secondly, researchers mention that their choice was based on environmental scanning and stakeholder engagement, including funding availability, societal needs, and emerging trends in their disciplines. The third factor is scholarly collaboration: researchers modify their research focus by collaborating with colleagues who offer different theoretical or methodological perspectives ("complementary collaboration").

Extending this focus from individual strategies to institutional contexts, other studies have analyzed how autonomy, collaboration, and disciplinary environments influence researchers' strategic behavior. Zalewska-Kurek (2016), for instance, investigated how researchers navigate their professional environments based on semi-structured interviews with scholars in the social sciences and nanotechnology. The author analyzed researchers' strategic choices and categorized them into four different strategic behavioral modes based on their level of autonomy and collaboration. Mode 1 is called "Ivory Tower" (i.e., High Autonomy, Low Collaboration), Mode 2 "Demand-Oriented" (i.e., Low Autonomy, High Collaboration), Mode 3 "Research Entrepreneur" (i.e., High Autonomy, High Collaboration), and Mode 0 "Side Project" (i.e., Low Autonomy, Low Collaboration). Nanotechnology researchers behave mostly in Mode 3, i.e., sharing resources and being highly autonomous, while social science researchers behave mostly in Mode 1, the "Ivory Tower." According to the author, social scientists' low need for scientific collaboration might be explained by the fact that they rely less on shared specialized facilities for research and are less used to uniting different disciplines in one project.

At a broader institutional level, Cooper (2009) showed that commercialization pressures also shape problem choice. In her study of U.S. biological scientists, she finds that links to industry and patenting experience encouraged researchers to pursue commercially promising problems and to reorient science from public-interest questions toward those with market potential.

More recent studies have shown that institutional factors and feasibility influence the kinds of research projects scientists choose to pursue. For instance, Pittelkow et al. (2023) found that researchers' decisions to conduct a replication study were shaped by factors such as being invited to become part of a larger replication project or by journal policies that incentivized replications. Feasibility plays a role since researchers take into account the available resources and the ease of potentially adapting and running a replication based on the complexity of the design and the clarity and comprehensiveness of the original study, especially the methods section. Researchers consider

the number of available resources (e.g., time, money, equipment, skills and expertise, and potential collaborators) in comparison to the resources required to run the replication study. Time constraints, the burden on participants, and the ease of getting ethical approval are also mentioned as influential factors in the decision to run a replication study.

Together, these studies demonstrate that researchers' choices are shaped not only by personal strategies but also by institutional structures that define what is feasible, valued, and rewarded. Beyond these structural influences, it is equally important to consider the role of researchers' underlying values in shaping what kinds of problems are pursued.

Values in Research

Scholars in the philosophy of science have examined the role of both epistemic and non-epistemic motivations and values in selecting a research agenda (Kitcher, 1995; Longino, 1990). Longino (1990) argues that social values are inevitably embedded in scientific reasoning and that objectivity is achieved through critical dialogue and diverse perspectives. Kitcher (2001) states that scientific agendas reflect both the epistemic goals of truth-seeking and the human motivations that drive individual scientists. He proposes that the organization of science should acknowledge these motivations and design institutional arrangements—such as systems of recognition, reward, and collaboration—that channel personal ambitions toward collective epistemic goals. In this view, the tension between individual incentives and communal scientific aims is not a flaw but a structural feature that can, if well-managed, enhance the overall pursuit of knowledge.

Building on this debate, Douglas (2009) argues against the possibility of complete value-free science. According to Douglas (2009), non-epistemic values are unavoidable in decisions under uncertainty, such as weighing risks and consequences, and therefore must be made explicit and subject to scrutiny.

These philosophical perspectives highlight that values, both epistemic and non-epistemic, are intrinsic to scientific decision-making. This raises the question of whether researchers themselves are aware of the values and motivations that drive their project choices, and how these internal orientations interact with external situational and institutional pressures. Research on how values and motivations manifest in researchers' actual project choices remains limited. Addressing this underexplored question, the present study investigates how researchers' motivations and values interact with situational and institutional factors when choosing specific research projects.

The Person-Situation Framework

Drawing on Kitcher (2001), this study aims to shed light on the concrete social and scientific situations or predicaments researchers find themselves in. Research project choices arise from an interaction between individual dispositions and situational forces. A useful lens for understanding this is the classic person–situation framework in psychology: behavior (i.e., choice of a research project) is a function of the person and the environment together (Lewin, 1951). In the context of scientific research, we can view a researcher's general orientations (i.e., motivations and values) as predisposing them toward certain choices, but whether those choices are actually made will depend on the opportunities and constraints of the situation.

Although it is often acknowledged informally that scientists do not always 'practice what they preach,' systematic inquiry into this phenomenon remains limited. In particular, little is known about how researchers themselves perceive potential misalignments between their stated ideals (e.g., choosing problems solely based on importance or curiosity) and the practical realities that shape their decisions (e.g., selecting a project to secure a publication). It also remains unclear whether, and how, researchers seek to realign their practices with their ideals through processes of reflection. This introspective dimension has been insufficiently examined, despite its central relevance for understanding the integrity of scientific careers.

Research Objectives and Hypotheses

In light of the above, the present study seeks to investigate how researchers choose a research project by using a mixed-methods approach. Specifically, the study aims to identify researchers' general orientations (i.e., motivations and values) and to explore qualitatively how researchers select a specific project. While existing literature has primarily examined the dispositional personal motivations that shape research agendas, less attention has been given to how motivations and values influence the choice of specific research projects.

Although exploratory, the study is guided by theory-informed expectations. Drawing on the intention–behavior gap, I expect to find tensions between researchers' dispositional motivations, values, and the situational drivers of their project choices. At the same time, I also expect to find cases where researchers' decisions align with their epistemic and non-epistemic orientations.

In sum, this study aims to shed light on the underpinnings of research project choice by linking researchers' values and motivations with their actions under an intention–behavior framework. Understanding how researchers choose their projects is a first step toward fostering a more reflective and effective scientific practice—one that can better adapt to the structural and

societal constraints currently shaping research and safeguard science's role in the face of growing institutional and economic pressures.

Ultimately, the study speaks to broader debates on how epistemic and non-epistemic motivations should be balanced within the scientific community. Since there is little research on how motivations and values influence the choice of specific research projects, it cannot be subjected to any form of structured scrutiny, which is important to critically evaluate them (Merton, 1973). As part of making science more open, it becomes essential to examine how researchers' motivations and values interact with collective norms, such as Merton's (1973) principle of disinterestedness—the expectation that scientists pursue knowledge for its own sake, guided by epistemic rather than personal or strategic gain.

This study also serves the goal of making the scientific process as open and transparent as possible and preserving information of use for the entire scientific community. It may raise the question of whether the scientific community should legitimize certain non-epistemic motivations (Kitcher, 2001), especially in competitive environments where individual and collective goals may diverge (Van't Veer & Fried, 2024).

Ultimately, the insights gained from this study aim to contribute to a more coordinated and reflexive research culture—one in which collaboration, institutional support, and shared epistemic values work together to align individual project choices with the collective advancement of science (Rasti et al., 2025b).

Methods

Research Design and Rationale

This study adopted a mixed-methods case study design. The rationale for using a mixed-methods approach lies in the complexity of the phenomenon under investigation: how researchers' motivations and values shape their concrete project choices. The mixed-methods case study approach focused on researchers within the Faculty of Social and Behavioral Sciences at Leiden University and allowed for an in-depth exploration of project-specific decision-making within a relatively bounded institutional context. This institutional focus ensured a certain degree of homogeneity in terms of organizational culture and structures while still capturing disciplinary diversity (Institute of Psychology, Political Science, Education and Child Studies, and Center for Science and Technology Studies).

Given the complexity of factors that influence researchers' project choices and the central role these choices occupy in the "derivation chain" of research, it is important to develop a detailed and nuanced understanding of how such decisions are made. To this end, we combined interviews with two questionnaires: the Multidimensional Research Agendas Inventory-Revised (MDRAI-R) (Horta & Santos, 2020) and a newly developed Epistemic Values Scale. These instruments aim to provide a systematic assessment of dispositional motivations and values, while the interviews were performed to offer a more detailed account of how these orientations are expressed in practice. The Epistemic Values Scale has been developed by researchers who work in the field of metascience.

The interviews followed a semi-structured format with open-ended questions. To ensure complementarity with the quantitative data, predetermined questions were developed around four thematic domains: motivations, cumulative science, epistemic values, and reflection. The questions on motivations were informed by the MDRAI-R, while the questions on epistemic values drew on the newly developed Epistemic Values Scale. The semi-structured design gave participants the flexibility to elaborate on their perspectives and to raise issues beyond the pre-defined questions.

This study used a one-phase, qualitative-dominant mixed-methods case study with an embedded quantitative component. Integration occurred by (a) building the interview guide from the MDRAI-R and Epistemic Values Scale, and (b) connecting during interviews, where participants revisited their questionnaire answers as reflective prompts. Quantitative results serve as background about the sample and are not used in the thematic analysis.

Participants

The sample was drawn from the faculty staff list available on the website of the Faculty of Social and Behavioral Sciences. Using purposive random sampling, only assistant, associate, and full professors were included in the sampling frame. From this frame (N = 251), participants were then randomly selected in two rounds: first 33, followed by an additional 60. The random selection took place in April 2025. In my email invitations, I briefly informed participants about the aims and purpose of my study. The data collection took place from June 2025 to August 2025. Two participants I knew from participating in their course before conducting the interview. Participants did not receive financial compensation for their participation.

The decision to focus on established academics rather than PhD candidates or postdoctoral researchers was based on their relative autonomy in the research agenda setting. Doctoral and postdoctoral researchers typically work within predefined projects and under the supervision of senior academics, which constrains their autonomy in decision-making. In contrast, professors are

more likely to enjoy long-term stability and the freedom to pursue independent lines of research which make them a suitable population for examining how motivations and values shape project choices.

Due to the lack of a theoretical framework, it was not possible to determine the sample size required to reach theoretical saturation (Fugard & Potts, 2015; Palinkas et al., 2015). Within the given time constraints, I opted for a maximum of twenty participants to reach adequate stability and richness of patterns across interviews.

The final sample included 18 researchers at different career stages and across diverse disciplinary orientations within the social sciences. Specifically, the sample included four assistant professors, five associate professors, and nine full professors. In terms of disciplinary distribution, the sample covered five researchers from Political Science, one from the Centre for Science and Technology Studies (CWTS), seven from Psychology, and five from Education and Child Studies. While the study invited participants from all five institutes within the Faculty of Social Sciences at Leiden University, no participants from the Institute of Cultural Anthropology and Development Sociology took part.

Measures

Multidimensional Research Agendas Inventory–Revised

This study used an inventory that characterizes factors influencing the research agendas of researchers in all fields of knowledge: the MDRAI-R. The MDRAI-R (see Appendix A) optimizes an initial inventory designed for the social sciences, the MDRAI, by including new dimensions ("Academia Driven" and "Society Driven") that reflect the greater influence of social and organizational structures on knowledge production and demands for research impact. This inventory enhances our ability to measure research activities and assesses eight distinct motivations and contextual influences on researchers' agenda-setting. *Scientific Ambition* reflects the drive for prestige and recognition within one's field. *Discovery* captures the preference for pursuing novel and potentially groundbreaking research, even when risky. *Collaboration* represents the motivation to engage in joint projects, either self-initiated or by invitation. *Mentor Influence* measures the extent to which a PhD mentor's ideas have shaped subsequent research agendas. *Tolerance for Low Funding* indicates willingness to pursue research despite scarce financial resources. *Divergence* reflects openness to crossing disciplinary boundaries and engaging in interdisciplinary projects. *Academia Driven* refers to alignment with institutional or disciplinary community expectations, while *Society Driven* captures motivations to address societal challenges and incorporate non-academic

perspectives. Items were rated on a 7-point Likert scale ranging from *completely disagree* to *completely agree*. If participants considered an item not applicable to their research context, they could respond with *N/A*. For this study, the items of the *Mentor Influence* subscale were slightly rephrased to fit the study's sample (see Appendix B).

Due to a high level of similarity of items within each subscale which was pointed out by one pilot participant and the very first participant, the original MDRAI-R had been shortened. For the first participant, the original MDRAI-R was administered. All subsequent participants received the shortened version. We selected items judged to best capture each dimension's conceptual core which resulted in a 19-item short questionnaire (see Appendix B). All items from different subscales were randomized once before data collection, and this fixed randomized order was administered to all participants. Descriptive statistics for the MDRAI-R are reported in Appendix D (Tables 1 and 2).

Epistemic Values Scale

In addition to the shortened MDRAI-R, I administered a newly developed Epistemic Values Scale (see Appendix C). This scale was designed to measure the extent to which researchers prioritize epistemic values.

The first step in developing the scale involved conducting an extensive literature review to gather theoretical insights into the construct of epistemic values. As no existing scales specifically measured this concept, item development relied on the theoretical literature. The development of this scale was guided by literature on epistemic values in science (Douglas, 2009; Kitcher, 1995, 2001; Kuhn, 1977; Longino, 1990; Zagzebski, 1996), Mertonian norms of scientific practice (Merton, 1973), and classic accounts of trust and epistemic responsibility (Hardwig, 1991) and falsifiability (Popper, 2002). Based on this theoretical foundation, items were created to represent the scope of the construct.

The deductive approach, grounded in the literature review, was complemented by an inductive method that incorporated qualitative input from a domain expert, Dr. Vlasta Sikimic. This combined approach resulted in a pool of 21 items. The items then underwent several rounds of revision through discussions with my supervisors. Their feedback helped refine the wording and improve clarity, ensuring that each item accurately reflected the intended construct.

The scale aims to measure epistemic values such as methodological rigor, reliability, contribution to cumulative knowledge, and truth-seeking. Items also probed preferences for

originality versus replication, the correction of errors in the scientific record, and attitudes toward balancing speed over thoroughness in research.

The Epistemic Values Scale consists of the three subscales *Reliability and Truth, Cumulative Collective Knowledge,* and *Criticism and Error Correction* and has 21 items (see Appendix C). Items were rated on a 7-point Likert scale ranging from *completely disagree* to *completely agree*. If participants considered an item not applicable to their research context, they could respond with *N/A*. To minimize order effects, all items were randomized once prior to data collection. This randomized version of the questionnaire was then administered consistently to all participants.

The scale aims to address the limitations of existing instruments like the MDRAI-R, which have primarily measured personal motivations rather than epistemic values and commitments. Combining the MDRAI-R and the Epistemic Values Scale allow for a more comprehensive measurement of researchers' motivations and values influencing research-related decisions. Moreover, recognizing which values guide research-related choices could inform the development of behavioral interventions that reinforce commitment to epistemic values, while ensuring that institutional reward and recognition systems support these values. Descriptive statistics for the Epistemic Values Scale are reported in Appendix D (Tables 3 and 4).

Piloting

The Epistemic Values Scale and the interview procedure have been piloted with four participants (three PhD students, and one associate professor) before the start of data collection. Based on the first six interviews, two items of the Epistemic Values Scale were slightly adapted to increase their understandability (see Appendix C).

Procedure

Sessions were scheduled either in person at the Faculty of Social and Behavioral Sciences or via Microsoft Teams, depending on participants' availability and preferences. 17 interviews have been conducted in English, one in German. Interviews lasted between 27 and 58 minutes (M = 43 minutes). Sessions (questionnaire and interview) lasted at least 60 minutes or longer (except for two sessions), and in some cases, informal conversations continued afterwards.

Six interviews were conducted online. For in-person interviews, participants received printed questionnaires, whereas for online interviews, digital questionnaires were provided via Microsoft Teams. While collecting the data, I kept a lab notebook to document details relevant to the interviews.

Each session followed a standardized sequence: (1) informed consent (appendix F), (2) questionnaire completion (see Appendix D), (3) semi-structured interview, and (4) debriefing. The same sequence was followed in both online and in-person settings, with printed questionnaires used in person and digital versions provided via Microsoft Teams for online sessions. Recording began with the start of the interview after questionnaire completion. At the end of the semi-structured interview, after all main questions had been asked, participants were still recorded while being invited to share any additional thoughts or mention anything they felt had not yet been covered. Then, the recording was stopped and the session moved on to the debriefing.

The questionnaires were administered first in order to capture participants' broader motivational orientations and epistemic values at a general level. The subsequent interview then zoomed in on one specific current research project and allowed participants to reflect on how these broader ideals were expressed in their actual practice. In this way, the design intentionally juxtaposed dispositional orientations (what researchers want to do) with the current project they chose (i.e., what they actually do) and thereby created the opportunity to probe the gap between ideals and practice.

The semi-structured interview began with a set of opening questions. The two opening questions, *"Can you tell me a bit about one of your current research projects and how it started?"* and *"Can you briefly elaborate on what influenced your decision to do this study?"*, served as an entry point into the conversation and aimed to encourage participants to reflect on how their current projects were initiated and what shaped their choices.

The opening questions were followed by pre-determined questions around four thematic domains (see Appendix E for complete list of pre-determined interview questions). The questions related to the first theme, researchers' motivations, have not always been asked in the same order, depending on the conversational flow.

1. Motivations

   Participants were asked about the extent to which different motivations influenced their decision to pursue their *current* study. Building on prior work with the MDRAI-R, these questions contextualized general orientations in concrete project choices.

2. Cumulative Science

   Participants were asked to what extent their past research influenced their decision to conduct their current study and how they saw the study contributing to the broader literature on their research topic.

3. Epistemic Values

   Participants were asked to describe what they consider hallmarks of valuable scientific contributions and why they regarded their current study as contributing to science.

4. Reflection

   Finally, participants were asked to reflect on their questionnaire responses and on whether their actual research practice aligned with their dispositional orientations. In this way, the questionnaires also functioned as a reflective tool. The concluding reflection in the interview was designed to probe the intention–behavior gap (Sheeran, 2002).

Through this four-part structure, the study not only measured general dispositions but also explored how they are enacted and negotiated in practice.

Ethics

The study was approved by the Psychology Research Ethics Committee of Leiden University (reference number: 2025-05-14-A.E. van 't Veer-V2-6035). All participants provided informed consent prior to participation and were reminded of their right to withdraw at any time without providing a reason since the interview questions touched on personal or sensitive aspects of researchers' careers. Information letter, informed consent form, and debriefing letter can be found in Appendix F.

Transcripts were anonymized, and participants could choose whether their anonymized transcripts would be shared openly, for research purposes only, or not shared at all. Identifying information (e.g., names, institutional affiliations, project details) was replaced with descriptive placeholders such as *[research project]* or *[institute]*. Only in case participants chose that their anonymized transcripts could be shared openly or for research purposes only, I shared access to the anonymized transcripts with my supervisors.

Data Management

All interviews were transcribed using the medium model of Whisper (OpenAI, 2022), an open-source speech recognition system, run locally to ensure data privacy. The transcription was performed within Python (Version 3.12; Python Software Foundation, 2023). I conducted one of the interviews in German and translated it to English; translated quotations are indicated with [translated].

The data (questionnaire data, recordings, and transcripts) are stored in the institutionally managed OneDrive environment with multiple-factor authentication. Printed questionnaires have been digitized and stored on OneDrive. The file linking ID codes to participants' names and email

addresses was stored separately from the research data on OneDrive and additionally encrypted with an extra password. Access to this file was restricted to me alone. Data will be retained for ten years in accordance with Leiden University regulations. All data will be archived on the University's secure J:-Drive network.

The code used for calculating descriptive statistics and transcribing the interviews and all supporting materials are available at https://github.com/JuliaWeschenfelder/research_master_thesis.

Data Analysis

Descriptive statistics for the questionnaire data were calculated in R (Version 4.3.2; R Core Team, 2023) using the packages *readxl* (Wickham & Bryan, 2023), *dplyr* (Wickham et al., 2023), and *tidyr* (Wickham & Girlich, 2023). Descriptive statistics (means, standard deviations, minima, and maxima) were calculated for each item and dimension to provide an overview of participants' responses. The descriptive statistics of the shortened MDRAI-R and the Epistemic Values Scale are reported for contextual purposes only (see Appendix D; Tables 1-4). For the first participant, the original version of the the MDRAI-R was administered. However, only the items corresponding to the shortened version of the MDRAI-R, which was administered to all other participants, were included in the analyses.

For the thematic analysis, I primarily adopted an inductive approach. At the same time, my coding was partly deductive in that it was guided by the need to generate themes that were meaningful in relation to my research questions. Phases included familiarization, initial coding (data-driven with sensitizing concepts from the research questions), iterative theme development, review against coded extracts and entire dataset, naming and refining, and producing the narrative. As a single coder, I discussed the interviews and the thematic analysis with my supervisors. As data analysis software, I used ATLAS.ti for Windows (*ATLAS.ti*, 2023). Quotations are verbatim with minimal edits; bracketed insertions [] indicate grammatical clarifications without altering meaning; ellipses […] indicate omitted text.

The questionnaires (MDRAI-R and Epistemic Values Scale) were administered prior to the interviews to provide a structured assessment of participants' dispositional orientations. They serve two functions: (a) to provide descriptive background about the sample as a whole, and (b) to function as a reflective stimulus during the interviews, as participants were invited to revisit their responses to the questionnaires when reflecting on their research practice. Rather than identifying gaps through direct comparison of questionnaire items and interview answers, the thematic analysis captures

participants' self-awareness, the narratives they construct, and the meanings they attribute to their choices.

Researcher Position and Reflexivity

I conducted this study as a master's student in psychology at Leiden University. My position was that of a student researcher rather than a professional peer, which meant that I did not occupy a dual insider–outsider role in relation to my participants. At the same time, the asymmetry in academic seniority may have influenced the data in several ways. Participants, who were experienced faculty members, might have adjusted their responses to what they perceived as the knowledge level of a student researcher, for example by simplifying or elaborating their accounts more than they would with a peer interviewer. At the same time, my status as a master's student may have encouraged participants to articulate aspects of their decision-making that they would not typically reflect on with peers.

My own academic background and interests also might have shaped the research process. Coming from psychology and with a strong interest in epistemic values and metascience, I was aware of a potential bias toward attending more closely to participants' reflections on epistemic criteria. This may have influenced how I interpreted or probed their responses. To minimize such effects, I relied on a standardized interview protocol, piloted the questions beforehand, and maintained a reflexive stance throughout the project.

As preparation for the qualitative study, I attended a workshop on open qualitative research at the Paul Meehl Graduate School at Eindhoven University of Technology, taught by Dr. Sarahanne Field.

Results

The thematic analysis revealed a set of themes that capture what motivates and influences participants' project choices. These themes span different levels of influence, from deeply personal motivations and values, through the relational dynamics of communities and collaborations, to broader systemic conditions such as funding structures and academic culture. Presenting the findings in this order allows to "zoom out": beginning with individual-level drivers (e.g., recognition), then moving to meso-level influences (e.g., field community), and finally to macro-level structures (e.g., funding systems) that shape opportunities and constraints. This is followed by participants' reflections on their contribution to science and their epistemic values. Finally, I will present participants' reflections on the alignment between what they want to do and what they do in practice.

Micro-Level Influences

Curiosity

When asked about what drives their research, several participants highlighted a deep sense of personal fascination and curiosity. One researcher simply explained, "[…] it's just fascinating for me to understand" (ID 11). Another described how this pull of interest shaped their trajectory: "I always get sucked into it. So, I just got sucked into it and I want to do more with it. Now I think it's the most fascinating stuff ever" (ID 1). These reflections illustrate that many researchers are guided by a more fundamental motivation: a genuine intellectual attraction to their topic.

Recognition

While curiosity represented an internal motivation, participants' reflections on recognition revealed how personal motivation interacts with structural incentives. When asked to what extent academic recognition influenced their decision to pursue this project, participants positioned recognition along a broad spectrum. While never described as the primary motivation, recognition appeared as a career necessity for some, as a secondary byproduct for others, and was sometimes explicitly rejected.

Recognition as Career Necessity

For early-career researchers, recognition was linked directly to advancement and survival in academia. One participant explained: "Once you have established […] a reputation, you can use that to apply for grants" (ID 7). Another carefully thought about where they would publish the book study they are currently working on. They stated:

[…] if you want to grow, I think professor, that having a book is also very highly valued career wise. So especially with a top publisher, that's why I'm already thinking it has to be Cambridge University Press, or it has to be Oxford University Press (ID 1).

One participant recalled the need to build a reputation early on: "When I was early in my career and young, it was quite important to try to build up a reputation and to become fairly well known in your field" (ID 5). This pressure often meant adhering closely to mainstream research: "Before that time, of course… You're very aware that you need to be productive. So, you can't deviate too much from mainstream" (ID 3).

Recognition by Peers and Community Validation

Several participants highlighted recognition not as career advancement but as validation from peers and communities. One noted: "What I found interesting is that my community […] recognized that this is an interesting research question […] I'm getting good feedback and

encouragement" (ID 11). Similarly, another explained: "It's nice to find people whom these things resonate with" (ID 1), while one agreed: "That is nice to see that other researchers find that interesting, that's encouraging" (ID 2).

Personal admiration also shaped project choice. One participant described how their respect for senior figures in the field guided their own orientation: "I admired people that I could not […] work with […] So I really wanted to do something like them. And this topic really feed into how these people thought" (ID 18). The same participant recalled: "And when I presented it, people tell me this is a very original idea. […] it was my moment to shine" (ID 18).

Recognition as Secondary to Deeper Motivations

Some participants emphasized that recognition was not a driver but rather a natural outcome of doing strong research. One explained: "That's not my main driver. I think it's the secondary thing and it kind of helps. It's kind of a catalyzer in doing more research, but it's not a driver at all for me" (ID 17). Another framed it similarly: "I think if you do the good stuff, then you will see that you will get recognition" (ID 13).

This perspective often tied recognition to validation rather than ambition. As one participant put it: "We care to be taken seriously. […] If they like my work, I become naively happy. And that's fine. It's kind of childishness, I think is also very healthy" (ID 16). Another echoed this sentiment: "It's not that I have set a deadline for myself, like, okay, before I'm [age], I want to be [job title]. It's not about the title; I want to be recognized for what I do" (ID 6).

At the same time, one participant also reflected on the vulnerabilities tied to recognition. As one noted: "We are only human. And people are very afraid of reputational damage. Very afraid." (ID 14 [translated]). This highlights how recognition can function not only as encouragement, but also as a source of anxiety.

Several acknowledged recognition as emotionally rewarding but stressed that their true motivation lay elsewhere. One explained: "I think it's more inner motivation. […] I do want to do something that makes a difference. […] but it's not that I need prizes" (ID 10). Another agreed: "It's nice to have that as well. […] I'm not saying it's not relevant at all. […] But it hasn't been like, oh, yeah, I want to get recognition. So, I will do this" (ID 15).

Rejection of Academic Recognition as a Driver

Finally, some participants explicitly distanced themselves from recognition as a motivating factor. One explained: "I don't care. […] I don't aspire to become a full professor at all. I kind of hate the whole professor system. […] it doesn't have to do anything with me gaining international

recognition from fellow peers" (ID 4). Others tied recognition to problematic academic cultures: "I was very sensitive to this hero culture type of thing and getting a career based on [grant title] grants. I actually think is not a good thing. […] I didn't submit [the proposal] for that reason" (ID 8).

Recognition was also described as less relevant at later career stages: "That's not a motivation anymore because I'm already there […]. That's simply because [field] is not a very big field […] So it's not because I'm such a wonderful person but I just been in that field for so long" (ID 9). For others, societal recognition was more meaningful than academic prestige: "I'm really driven mostly by the questions and the content and definitely not by academic recognition. If anything, I find it more interesting to get societal recognition" (ID 12).

Mentorship

When reflecting on the influence of their former PhD mentors' ideas on their project choice, participants emphasized that their former mentor often shaped their academic paths profoundly. Researchers sometimes referred to their former PhD mentor as supervisor.

Mentorship as Ethical and Intellectual Influence

Participants emphasized that PhD mentors often had enduring influence, not only through intellectual guidance but also by modelling interpersonal and ethical ways of being in academia. One recalled: "He treated people in such a respectful and caring way that it made a huge difference" (ID 5). For this researcher, the relational dimension outweighed technical training:

That interpersonal relationship that we PhDs had with our mentors […], that probably had even more effect than what we learned from him about […] designing research. It was his ethical way of working with people and his respect that he had for people. Even when he disagreed with people in the field, he had respect for them, and he tried to treat people with respect and always tried to work in an ethical way. […] That has a huge influence on science and that we should be focused on trying to hand that down to the people that we work with or the people that we influence and help to train (ID 5).

They summarized: "He influenced the ways that we thought about living an academic life and what it meant to live an academic life" (ID 5). At the same time, this researcher worried that the current emphasis on "excellence" risks overshadowing such values.

Mentors also shaped orientations to research identity and style. One participant described being inspired by their "slightly anarchistic way of doing explorative stuff" (ID 7). One the other hand, they also described becoming independent form their mentor: "I made a shift from the things that

my supervisors were doing. […] I just like to do my own thing. And if I look around, actually, I see that a lot of people, they become some kind of a copy from their supervisors" (ID 7).

One recalled: "One of my mentors, she was really big on, yeah, the bridge between practice and research" (ID 4). This influence left a lasting mark, as the participant emphasized that "if you want to develop something for practice, you cannot do that from your ivory tower, you have to collaborate with the practice that is going to implement it" (ID 4). Here, the mentor's perspective appears to have directly informed the researcher's own orientation toward collaborative, practice-grounded science.

Others noted subtle, implicit influences— "the way you're shaped as a researcher and ideas that you get" (ID 9)—or moments of explicit encouragement that opened new paths: "Had my mentor not said this to me […] I wouldn't have dared" (ID 15). Some remembered mentors as role models of creativity and intellectual style: "One of my PhD supervisors […] his enormous flexibility and creativity […] of course deeply influenced me at the individual level. I really admire him to this day" (ID 16).

Mentorship and the Experience of Independence

Others focused on the balance between mentorship and independence. Some described having exceptional freedom: "My doctoral supervisors […] never steered me. I was completely free to pursue what I wanted" (ID 14 [translated]). Because of this freedom, they emphasized the need to build their own network: "This means I, as a PhD student, […] had to build my own network. I had to find my own path. I could never benefit from my supervisors because I was doing something entirely different. But this enriched me." (ID 14 [translated]). This researcher also gave this freedom to the students they supervise themselves: "I leave students as free as possible because I believe the best emerges only in freedom" (ID 14 [translated]).

Similarly, one participant reflected on being in a department that let them follow their own interests and contrasted this with the "hierarchical" experiences of peers (ID 11). Another emphasized continuity: "There is definitely a red thread with the type of questions that I'm interested in from my PhD project […]. I also think it's very problematic that we think about independence of PhD supervisors as a good thing. I think that is one of the flaws in the system—that we have to get independent of someone else to build good knowledge" (ID 8).

Moreover, one participant emphasized how their mentors opened doors to one of the research projects they are currently working on. They described ongoing collaboration: "[…] he was

kind of the key person to ask me to join this initiative. […] I always discuss my ideas with them and let them read my grants and my papers." (ID 6).

In contrast, some mentorship relationships were more limited. One said simply: "Nothing that my supervisor does at all" (ID 1), while another noted: "Not at all. No, I made a clean start after the PhD" (ID 3). Others described partial or methodological influence, such as feedback and advice without steering project direction (IDs 11, 12).

Meso-Level Influences

Field Community

When reflecting on the influence of their field community (i.e., the group of researchers they identify with), participants described that supportive communities and a sense of belonging encouraged certain directions and that conferences, networks, and informal exchanges both fostered collaborations and revealed gaps. Also, institutional settings appeared to have an influence, as researchers adapted their research to the work that was expected from them in their current position or department.

Traditions and Institutional Contexts

Some participants reflected on broader traditions and institutional settings. One explained: "The tradition that I come out of is looking at challenges that exist in practice and then trying to draw from theory to create well-designed studies" (ID 5). Others described aligning their work with departmental contexts: "I'm trying more to focus my research on something that fits within the department or in the broader [research topic] work. […] I'm thinking […] how can I reposition myself […] within this new place at work" (ID 2). One participant explained that their project direction was largely determined by their employment conditions: "Because it's the most clearly related to the job I was hired to do. […] I really wanted to deliver and to show that I'm willing to contribute to this sector plan" (ID 18).

Institutional settings sometimes limited participants' freedom, particularly early in their careers. One participant reflected:

At first, I didn't get to pick my own research agenda. […] doing research on [research topic] was not exactly voluntary, but it was, yeah, the best of the worst that I could pick from. That's why I ended up doing research on [research topic]. […] So, at first, I was just forced into this topic that I didn't really feel any affinity with. And now I found this combination that I can use what I've learned during all those years of doing something that I really like (ID 4).

This illustrates how institutional contexts can both constrain and, eventually, reshape personal research trajectories: what begins as an imposed topic can later become a foundation for more self-directed work.

Conferences, Networks, and Field gaps

Community connections were often formed at conferences and through informal exchanges. As one participant put it: “The field is a vague concept, of course. […] You do nothing but talking about research when you are at conferences. And so, of course, people influence your decisions. Sometimes you decide to put up collaborative projects” (ID 3). Another described exchange with practitioners as pointing to urgent needs: “If you talk to [professionals], […] this is what we would like to have, and we don’t have it. So, if you can work on this, it would be great. It makes me happy […]. That’s always been, for me, an important drive” (ID 13).

One participant recalled: “From this conversation with a field expert, this idea came about, and I started pursuing it” (ID 16). This new idea also drew strength from what was missing in the wider debate, as the same researcher noted: “Yes, the field influenced me, […] this absence of [research project] in this entire debate was an important motivation” (ID 16). Also, conferences reinforced choices by repeatedly highlighting the gap, as another participant explained: “I think this topic and this knowledge gap is often emphasized at the conferences that I visit. […] It influences my decision to work on this study” (ID 12).

At the same time, one participant also reflected critically on the scientific community. One wondered “whether or not we are as a community of scientists too inward looking, too concerned with our own way of working but sometimes forget to think about the bigger picture” (ID 8).

Collaboration

Participants described collaboration both as a practical necessity and as a meaningful driver of their research choices. While some saw it as a way to access expertise, data, or funding, others emphasized the intellectual and social value of working together or expressed frustration with challenges in large collaborations. Across accounts, collaboration shaped not only what topics they pursued but also how projects were designed and carried out.

Collaboration as Intellectual Enrichment and Access to Expertise and Data

For many, the motivation to collaborate came from the stimulation of new perspectives. One participant reflected: “To me, it’s just more fun to work with colleagues. And it’s also important because they bring things that I don’t know or don’t think of” (ID 5). Another emphasized how partners expanded the scope of their research: “I was collaborating with so many groups and so

many PIs with all their own ideas, [it] challenged me to deepen my understanding… it added an additional layer to my work" (ID 6).

Several participants (IDs 2, 11, 15, 17) highlighted that collaboration gave them access to knowledge, methods, and data that they would not have been able to obtain alone. As one explained: "That's why I also develop good collaborations with local scholars, because first, I think their expertise is excellent and undervalued. And second is because they're on the ground, so they help me get the data sometimes I want" (ID 11).

Moreover, one participant pointed to complementarity as central: "We […] bring people together that really are complementary in what they bring in the team" (ID 17). Another described forming a large consortium: "We actually sought out people who are good in what they do and invited them to be a part of the consortium […] we really need different expertise. And also, different fields. […] And we need that collaboration to really make something that can be groundbreaking" (ID 15).

Collaboration as Social and Relational Practice

Beyond strategic concerns, participants also emphasized the social dimension of collaboration, and described it as rooted in reciprocity, collegiality, enjoyment, and "nice synergy" (ID 16). One explained:

Like I said, the colleague who wanted to revamp their academic career […]. So, it's more like strategic, not necessarily strategic for me, but sort of giving something back. Let's carry you towards publications so that you can have maybe a promotion in your institute (ID 1).

Another highlighted continuity in personal ties: "My former PhD students, it's really nice to still collaborate with" (ID 10). Some pointed to the interpersonal satisfaction of teamwork: "So my supervisor, I really like working with him" (ID 2); "I always liked working [in] small teams. […] The smaller the group, the biggest the impact of what these people bring" (ID 7). For some, collaboration with practice was especially valued. One explained:

I really strongly feel that it is essential to always collaborate. So, if you want to develop something for practice, you cannot do that from your ivory tower, you have to collaborate with the practice that is going to implement it (ID 4).

Others stressed the importance of good personal dynamics when building teams: "So the social component, like we want to get together a nice group of people […] you really want to put a team together that also personally functions well" (ID 15). Along similar lines, one participant

emphasized complementarity: “So we […] bring people together that really are complementary in what they bring in the team” (ID 17).

Challenges and Limits of Collaboration

At the same time, participants recognized that collaboration could be difficult. One participant noted that divergent interests were sometimes hard to reconcile: “[…] sometimes one person has a different interest than the other person. And then sometimes it’s a challenge to bring that together in one project” (ID 11). Large collaborations, in particular, were described as frustrating when commitments broke down: “I’ve also collaborated with larger groups… And then in the end, I was the only one who wrote an article. […] And that I find frustrating” (ID 10). The same participant added: “It’s difficult for all people in the team, I think, to stay committed. And everyone is so busy” (ID 10).

Others voiced discomfort with collaborations that were driven more by production pressure than intellectual interest:

I will accept an invitation to collaborate—if I really feel that I can contribute something, and not just because it means a publication. I’m not interested in that at all. I’m not here to… I don’t work in a factory. I’m not in a production company. That’s not how I see it. I’m really only interested in the subject matter itself (ID 14 [translated]).

Beyond such pressures, participants also reflected on the effort it takes to manage large, diverse teams. One explained that collaborations often require coordinating not just technical tasks but also interpersonal relations: “I was collaborating with so many groups and so many PIs with all their own ideas, [it] challenged me to deepen my understanding… it was not only data management and research, it was a human management” (ID 6).

Interdisciplinarity

Interdisciplinarity was not central to participants’ motivations to choose what to study. For most, interdisciplinarity was not a conscious goal but rather a natural outcome of the kinds of questions they pursued or a structural feature of their field. Some participants also elaborated on the challenges of interdisciplinary work.

Interdisciplinarity as a Byproduct of Research Questions

Several participants emphasized that interdisciplinarity emerged organically rather than being deliberately pursued. As one explained: “That’s not a reason for doing this project. But I think my interests are in multiple fields. […] It’s driven by the theoretical foundations of my work” (ID 2). Similarly, another participant said:

Interdisciplinarity has never been a goal in itself for me. Basically, it was always very interdisciplinary research. […] For me, it was just research. I had no idea. I only found out that that was interdisciplinary later on. So, for me, it feels very natural to do that, but it never was a motivation in itself (ID 6).

Interdisciplinarity of Research by Design of Field

Other participants described interdisciplinarity as an inherent feature of their disciplines. One explained, "[field] […] is by design interdisciplinary […] it's built in so I don't have to come up with a motivation" (ID 8), while another put it more simply: "So by nature, it is a pretty interdisciplinary field" (ID 9).

One participant explained that their move toward interdisciplinarity "grew out of a bit of a disappointment with monodisciplinary research" (ID 3). For them, working across fields offered greater stimulation and collaboration: "I was specifically looking for something with more collaborations and also a bit more intellectually challenging" (ID 3). However, they also acknowledged the difficulty of sustaining such work without resources and noted that once grant support ended and administrative duties increased, their research shifted back toward a monodisciplinary focus (ID 3).

Interdisciplinarity as a Practical Necessity

Some participants linked interdisciplinarity to the practical demands of research questions. As one explained:

I think it's just because a lot of the research questions I don't think can be answered within just one discipline. So, I think it's just important to work with people that are actually from these disciplines and not myself try to reinvent the wheel. […] I'm also not dogmatic. I don't think it's one discipline that gets all the answers right. […] But also increasing […] demand by all these grants to do cross-disciplinary, multi-disciplinary research […] there's also an institutional impetus for it (ID 11).

For one participant, interdisciplinarity was framed in more philosophical terms. They argued that disciplinary boundaries are human constructs designed to manage complexity:

Deeply, when you look at reality as one ambiguous thing […] all these disciplinary boundaries [are] our way of tackling the complexity of reality. […] Reality comes as one thing and it doesn't recognize any of the boundaries we want to put onto it. […] If you want to understand reality, then you need to use everything, every piece of knowledge that you can get into. This inevitably implies [an] interdisciplinary approach (ID 16).

Interdisciplinarity as a Challenge

Participants (ID 7, 9, 14) also highlighted the challenges of working across disciplinary and linguistic boundaries. As one explained: “[…] all these different fields that don’t talk to each other. […] You have to sort of create your own language that other people recognize. That’s the challenge. But it’s also fun” (ID 9).

One participant reflected on the challenges of interdisciplinarity:

Interdisciplinarity is extremely challenging. […] These concepts fall into a different conceptual system, and as a result the terms take on another meaning. So, I am not even sure that true interdisciplinarity exists. […] In the end, it always remains more of an approximation between disciplines. And there is no authority that can unify them. There is no point of view of the universe (ID 14 [translated]).

Macro-Level Influences

Funding and Resource Constraints

In the course of the interview, participants reflected extensively on how the funding landscape and resource constraints shaped their project choices, motivations, and possibilities. Most pointed to persistent pressures that demanded pragmatic adaptation with respect to which research questions they pursued, depending on the funding that was available.

Proposing Research Likely to be Funded

Several participants criticized the way large grants are distributed, as they promoted more of the same research. One argued:

I think it's a very, very impractical way of dividing our money. […] so many good, great researchers […] just don't get the funding. […] And then the people who get the grants are the people who were successful in the past. […] I have the impression that there's less creativity because […] basically doing what you were already doing gets rewarded (ID 7).

Beyond outcomes, participants also noted that the funding process itself shapes research proposals, as researchers are more likely to submit ideas they believe committees will evaluate positively: “You are constrained by what you think […] that a committee wants to read” (ID 7). Others contrasted current practices with earlier, more flexible institutional structures: “When I started working here […] the department still had […] PhD students from university money. […] The chairman […] could do what he want with that. […] It was a very light assessment” (ID 3). By contrast, today’s

reliance on competitive grants was seen to narrow research possibilities and privilege certain types of research, reducing the freedom to study what you want.

Criticism on Funding Focus

Several participants highlighted the growing emphasis on societal relevance in funding calls. While supportive, they worried about distortions this created for fundamental research:

When you do a fundamental study, you also have findings that you cannot readily explain. […] The problem occurs more when researchers feel like they have to adapt their research questions because that's where the money is. […] You need to have all kinds of societal partners […] it makes things a little bit far-fetched sometimes (ID 15).

Different participants saw it as problematic when researchers adapt their research questions in order to increase their chances to receive funding.

Avoiding Large Grants and Tailoring Projects to Feasibility

Participants described the funding process as exhausting and demoralizing. One admitted: “It feels like it's so ridiculously unfeasible to get your money there […] if we can circumvent the whole funding circus, that would be my dream come true. I really detest the way it works” (ID 4). Another highlighted the strain not just in terms of efficiency, but also in personal terms: “all those grant applications and all those hunts for money, it's also personally very intense, I think” (ID 6). Another recalled early career advice:

One of the professors […] told me if you want to make a big career […] you have to write grants in your own time. And it has to be about certain topics. And then I thought, yeah, that's not going to make me happy. […] So, I decided to follow my own path (ID 10).

One participant explained how these frustrations shaped their strategy: “I'm not going to do this prestigious big project […] because I'm losing a couple of months. I can spend that time on a small project with a couple of people and at least do something” (ID 7).

Some participants emphasized that funding was never a limitation, either because they found alternate solutions (ID 11) or because institutional budgets covered their work (ID 12). Some participants said they would proceed regardless; they use grants as accelerants rather than prerequisites: “I would do it anyway […] even without funding, I would do it. […] Funding […] allowed me to have a postdoc […] a very good bonus” (ID 16). Another participant said: “If we wouldn’t have gotten that grant, then I probably would have still done this, but more as a side project. Now it’s becoming the main objective” (ID 9).

Few participants described working with comfortable funding buffers. One noted, “I am in a luxury position […] we have quite a bit of funding. […] We don’t have many constraints. […] We don’t have to adjust our ideas to get the funding” (ID 17). One described a “Matthew effect” situation where choices were driven by momentum, not money: “I was a bit overloaded already. I got this Matthew effect […], an avalanche of things that worked. […] I didn’t need it for financial reasons” (ID 8)

Participants often adapted their work to what was feasible with limited resources. Some designed research that could be done without large grants: “So my […] research line has always been the one that I can do without much external funding. […] Since I didn't get [the grant], I'm staying on this path. I find it equally fascinating” (ID 1). When resources were scarce, researchers scaled down ambitions.

Similarly, two participants (IDs 4, 5) reworked unfunded ideas into smaller, sequential studies: “It may not be as large scale as you would like. But you can also answer questions with smaller scale studies […] rather than have a large-scale trial” (ID 5). Still, they admitted: “Funding makes it easier” (ID 5). One researcher highlighted that they tried to make the best of it despite limitations: “[…] that’s also the creativity in research what I like a lot” (ID 7). One researcher described how a failed grant directly put their research line at risk, particularly when combined with teaching loads: “I’m not sure yet how I can continue that work. […]” (ID 6).

These accounts show that while some participants managed to adapt creatively or rely on smaller-scale studies, many described the funding system as exhausting, restrictive, and sometimes even demoralizing. The sense of frustration with external constraints extended beyond funding to the broader academic culture itself.

Academic Culture and Publication Pressure

Participants also described how the academic system influenced research. Several emphasized the continuing influence of traditional markers of success on career trajectories. As one participant noted:

So, there are a lot of ideas about how things should be done. But you still see that a lot of things require you to have done things in the old-fashioned way, so to say. So, it's very nice that people are saying you don't have to publish as many papers because it's more important about the quality of the papers rather than the quantity […] But for example, in grant applications, in job applications, you notice that the number of papers, the nice journal, the previous funding that you got […] still, if

you have that Nature paper on your CV, people still see the journal Nature and they think you're a good researcher without even reading the paper that you published there (ID 2).

The pressure to publish was seen as distorting research practice. One participant described universities as “production factories”:

I believe that the publication pressure is too strong, that too much is produced. And then there is a lack of time, of course, to write it up in a way that the knowledge which you acquired with whatever method is integrated. […] I think the structures we have built, which fit into this publish-or-perish-cult, are devastating for our institution. Many of us know that but we can’t escape it, because after all we want to make a career. […] I see young people who are products of the distortion of this system, they are not to blame. […] if they want to make a career here […] they must join in. That is the disciplining of the organization (ID 14 [translated]).

Societal Relevance

Participants described different positions on how societal relevance shaped their project choices. For many participants, societal relevance was a strong motivator in shaping their research choices. While some described societal impact as their primary driver, others sought to balance fundamental inquiry with practical application, or framed engagement as an obligation shaped by current reward structures.

Societal Relevance as a Primary Driver

For many participants, societal relevance was a central motivation. One researcher stated plainly, “I'm mostly motivated by societal problems or […] common day issues that people encounter” (ID 4). Participants (IDs 9, 10) emphasized actionable outcomes: “I don’t like doing research for research’s sake. […] I want to do stuff that practitioners may use in real life” (ID 9). One researcher (ID 7) had even shifted their research from fundamental to applied research and explained:

I'm still interested in fundamental research but […] my evaluation of the value of that research has changed. So, I now think, okay, we just should be much more careful in how do we spend our money. […] I want to have projects, I want to be involved in projects that also have much more value, like societal value. […] I love fundamental research, but I also realize that it's kind of like a luxury thing, you know, as scientists to just spend our time on fundamental questions that are absolutely irrelevant (ID 7).

For one participant, societal impact was pursued primarily through outreach: “[…] increasingly I've included more and more policy or social outreach […] writing short op-eds […] meant to a non-academic public” (ID 11).

Balancing Basic Inquiry with Societal Application

Some participants described their work as basic research that was nonetheless shaped by practice: “It is basic research; it is quite fundamental. But it's inspired by things that we hear and see from practice” (ID 12). Others framed their role as laying the groundwork for later application: “I hope that my work provides a foundation for others to do more practical implementation work” (ID 6). In this light, engagement with societal stakeholders such as NGOs or governmental institutions also varied. While some (ID 4, 9, 17) emphasized active contact, others had no contact or saw such interaction as premature until results were available: “Because I don't have the story to tell them yet. I need to do the research first” (ID 1).

Replication offered yet another route to connect fundamental inquiry with downstream application. One participant pointed to replication as a form of fundamental work that may not show immediate practical outcomes but is still crucial for building reliable knowledge. They explained: “What we found was nothing. […] We could not replicate anything […] It does not have direct societal impacts. […] But […] we need […] insight in the […] mechanism” (ID 6). Although the results did not produce a direct application, the participant highlighted that testing whether existing findings hold up is indispensable. Replication ensures that later, practice-oriented interventions are built on solid evidence rather than on fragile or unverified results. In this sense, even work that seems detached from societal use can be seen as an indirect but necessary contribution to downstream application.

Reward Systems and Societal Obligation

Participants also reflected on the systemic context that shapes how relevance is defined and rewarded. One questioned whether the current reward system—with its emphasis on “high impact peer-reviewed journal articles for a small international community”—is “effective enough” and argued that the system is “skewed towards rewarding that kind of activity and less so around […] openness to society, collaboration with society, societal questions” (ID 8). Another framed engagement as a responsibility:

I feel that it's also like an obligation […]. We should put [our] research skills […] with the purpose of making the lives of these [participants] a bit better. […] There's always this gap of what practice or policymakers want […] and what you can say. […] We want to be very careful and say it with as much nuances as it needs (ID 13).

Similarly, a participant who had shifted their research from fundamental to applied research pointed out: “If I now have to write a grant, […] I really want it to be more societally relevant. […] We owe that to society” (ID 7).

Innovation

Participants expressed varied perspectives on whether and how the pursuit of innovative or unconventional research influenced their decision to do their projects. Innovation was rarely explicitly mentioned as a goal. Instead, participants valued innovation as a challenge and in terms of methodological innovations. For some researchers, innovation was less about radical novelty and more about effectiveness and cumulative problem-solving.

Innovation not a Primary Driver

Several participants emphasized that innovation was not central to their motivations. One reflected that their work was firmly rooted in traditional scholarship:

So I think I'm a classic [research topic] scholar in the sense of I'm not using the tools that are now very popular among funders, big data, AI, etc. […] So basically I wouldn't say that I'm innovative, but maybe I'm even ancient in terms of focusing on the core themes of [field], even though what's currently hot is not a part of that (ID 1).

Others explicitly distanced themselves from innovation: “Not at all. This is not something that I'm motivated by at all” (ID 4); “The motivation to the idea of like, I want to do unconventional research. That in itself is not the idea. It's not a driver” (ID 9). Also, one participant emphasized that innovation was less about deliberate pursuit and more about natural progression. One explained: “I don't know whether any of these aspects really were in my head when I came up with it, just felt like the logical next step to do” (ID 7).

Innovation as Relative and Contextual

Some participants acknowledged innovative aspects in their projects, even if innovation was not their main driver. One described their work as “a classic innovation type project” (ID 8), though innovation was not the initial goal. Others placed their work in a more ambivalent position: “You could see it as innovative, but not against mainstream” (ID 3), or “I think it's unconventional […] but for me, it's very conventional” (ID 11). These reflections show that innovation was sometimes understood as relative defined by context and the research focus of the field.

Innovation as Cumulative Problem-Solving

Several participants rejected novelty for novelty’s sake and instead reframed innovation as a matter of problem-solving. One explained: “That is an innovative technique. […] because it's useful,

or I think that it is useful, I want to see if it is useful. So, it's not necessarily trying something new for the sake of it being new, but it is something that can help move this field forward" (ID 2).

Another emphasized effectiveness over labels: "Other people may say, well, that's not innovative. I think I don't care what you call it. […] I'm only interested in effectiveness" (ID 5). For this participant, real innovation was cumulative:

I really dislike the whole emphasis on innovative research, because it often seems to me to imply that it has to be something that no one has ever done before. […] But I think that innovation can also be viewed as something that's building upon the blocks that have been laid before you by other people's research. […] That seems to me what science is. Science is building upon those blocks. […] That's where innovation is, it's the best kind of innovation (ID 5).

Methodological Innovation

Others highlighted that the methodological approach was the innovative aspect of their research project. One described that as follows:

The innovation is in the sense that you bring together a lot of data and that you try to get more insight in what's really going on, because […] it was annoying to me […] if you read different papers from different groups, then there is no consensus. […] It's more innovative from the methodological aspect […] you want to help the field forward by seeing what's really related to [research project] (ID 6).

Another focused on improving research design in difficult contexts:

So, a lot of research is correlational […] but in a general population. […] So, it's hard to do very good quality research in this population. That drives me. And it's not always easy […] We need to be very innovative […]. But that's my main driver because the research out there, the hypotheses that are being tested are often being tested based on not the most thorough research designs (ID 17).

Innovation as Challenge

For some participants, innovation was closely tied to ambition and the pursuit of difficult, high-risk projects. One researcher described their work as unusually demanding but ultimately rewarding:

That is innovative, and it is very challenging. I mean, there are many things where you could say, oh, that would be great, but […] how are you going to pull that off? It's just like the costs are too high, or it's just not really doable. But the fact that we received this, that's just like, yeah, it's in a way a bit of a dream come true, because it is something that is really challenging (ID 15).

Here, innovation was equated with the courage to take on projects that others might consider infeasible. Another participant approached the idea of innovative or unconventional research differently. They argued that while conventions serve important functions, they can also limit imagination. They presented innovation more as a mindset that questions established practices and dares to see connections that conventions may obscure:

Conventions are not for nothing, but conventions are somehow methods of dealing with complexity. […] Quite often things are just in front of us, but we cannot see them because we are conditioned by conventions. […] Being aware of conventions sometimes may be useful, but I think more often they are kind of obscuring things that prevents us from seeing some connections, connecting some dots. […] You should not be constrained by them. […] Science is some kind of a hot mess (ID 16).

Epistemic Goals and Values

Epistemic Goals

When participants were asked how their current research project contributes to science, some gave broad answers, for example describing their aim to "help us to look at things in a different way" (ID 5). The majority of answers illustrate that they saw contributions to science as multifaceted. Projects could make theoretical or methodological contributions, extend knowledge into new contexts, or provide insights for future work through exploratory research.

Theoretical Contributions

For some participants, contribution was primarily about advancing theory. One researcher emphasized their project contributed to science "Because I'll provide a theory of [research project] that doesn't exist yet." (ID 9). Another emphasized novelty in applying existing models to a new case: "A lot of these models, theories were not really applied to the [research project] context. So, it's a new case and it's not a small case to apply a lot of these arguments. I think it also helps the broader theoretical conceptual debate" (ID 11).

Methodological Contributions

Others highlighted methodological advances as their key contribution. For example, one researcher described how their project provided "an alternative to these complex methods that we had to rely on previously" (ID 2). Another emphasized the innovative use of datasets from different institutions: "I really feel that how we analyze all those [research project] from different institutes, from different sources, in one harmonized way, that that would give the most reliable insights" (ID 6).

Methodological contributions were also described in terms of combining smaller studies. As one participant explained: "often these have […] small sample sizes that are on itself valuable and

you can publish on them. But if they can then be combined in some way, and this shows, I think, that this question really needs to be answered in a bigger, more thorough way" (ID 17). Another emphasized their ambition to push qualitative methods further: "I think what I want to do is to show how qualitative methods can really help to make rigorously […] inferences without having this like ideal data sets. […] We can use these pieces […] connect the data into a qualitative framework. So, I think I can make […] this methodological contribution" (ID 18).

Contextual Broadening and Foundational Groundwork

Some participants saw their role as providing groundwork for future research and in the extension of research contexts. One explained: "We are doing the very first exploratory step. So, then I would say it contributes to science by providing a descriptive kind of baseline for future research to get this empirical evidence" (ID 12). Moreover, one researcher argued that their project mattered because "most research has been performed in the Western world. And I think it's really about time that we also focus more on the other parts of the world" (ID 10).

Epistemic Values

Beyond how their work contributes to science, participants also reflected on what makes research valuable. Participants offered a wide range of perspectives. The answers revealed key points: research should be clear, honest, and rigorous; it should ideally address practical or societal needs while also providing fundamental insights. Moreover, participants valued methodological diversity, replicability, pre-registration, and conceptual innovation.

Clarity and Honesty

For some, value lies primarily in the clarity and honesty of research. One participant criticized overly abstract theorizing that lacks empirical grounding:

Depth, empirical depth. […] There are so many scholars that take this grand theory […], then apply it with some quantitative data […] and then make a lot of conclusions about that. […] You can't do that. […] You have to be able to say it to a student in the first year with concrete examples. If you can't do it and you're just babbling […] (ID 1).

Similarly, another highlighted transparency as a foundation of value:

To me, it makes more sense to look at those studies who are more honest or pre-registered […] We tried as scientists, we tried to find reality, what is true. […] The aim is shifting towards trying to report something that is the truth or that is at least done in such a way so that it can uncover the truth (ID 2).

Practical Importance

Participants emphasized that research is only valuable when it combines methodological rigor with practical importance. As one participant explained: “A relevant question, I would say, but also, of course, rigorous methodology. […] A problem in someone’s everyday life, either a patient or a clinician struggles with a question” (ID 3).

Another was more skeptical of research that lacked societal application:

The link to indeed societal issues or […] societal relevance […] must be very clear. […] If there are no clear clinical applications and a study seems to be performed just because we’re curious how something works, I’m really skeptical about that type of thing. […] New ways to measure certain things more reliably or more quickly, that’s really valuable (ID 4).

Originality and Conceptual Innovation

A number of participants highlighted originality as a central source of value. One argued that valuable research is that which “look[s] at a problem differently than it’s been looked at before and think[s] about the solutions in different ways” (ID 5). Another emphasized the importance of challenging accepted ideas, which they saw as part of verification and correction in science: “I like the studies that, for example, test a very solid idea and then show that it’s just crap” (ID 7). For some, conceptual innovation was seen as rare but highly prized. As one researcher reflected: “Well what has always drawn me to [field] are studies that are highly conceptually innovative. And not a lot of people are capable of doing that” (ID 8). Another put it simply: “When you provide better theories. I think that’s the essence” (ID 9).

For one participant, value lay less in critique and more in refining research questions and advancing conceptual clarity. One participant explained that they deliberately avoid engaging with weak studies:

When I found some […] study that I think is methodologically weak, I ignore that […] because it’s a personal choice, I just don’t feel comfortable […] exposing people, maybe because I am, I’m kind of scared of being exposed myself. […] I am a conflict avoider (ID 18).

Methodological Thoroughness and Accuracy

One emphasized methodological thoroughness: “It needs to have a very thorough research methodology. And conclusions […] that you actually can draw based on them” (ID 17). Also, one researcher said: “New ways to measure certain things more reliably or more quickly, that’s really valuable” (ID 4).

Participants repeatedly stressed the importance of a good methodology and precision. One criticized incomplete reporting and poor engagement with existing evidence:

What I am always very critical about is whether there is a good methodology and that research questions and hypothesis and the outcomes […] have a good story, that it really learns you something new. […] What I don't like is when important details are missing, when people are ignoring existing evidence. […] I also think that researchers should be honest about what their results mean and what they don't mean. […] I think it's really important that as a researcher, we learn to write accurately (ID 6).

They added that inaccurate writing can have negative downstream effects: "People build on that. And then, then it becomes much more valuable. It becomes much more than it initially was" (ID 6).

Some participants emphasized the importance of replication and knowledge synthesis. One researcher emphasized: "I think it's relevant to replicate findings. […] Meta-analysis […] is really, really valuable that they summarize what we have so far in order to get more knowledge from that" (ID 10). Another added that also "the replication of existing models with new null results or negative results" are very important: "it shows just the limitations of existing theory" (ID 11).

Methodological and Collaborative Diversity

Others pointed to diversity of approaches and collaborative research as central sources of value. As one explained:

I think, first of all, value lies in variation, so different types of studies, from qualitative to quantitative, from exploratory to confirmatory. […] Value really comes from […] use-inspired research. […] I think value also comes from team science, because working in teams, disciplinary teams, and even more so interdisciplinary teams […] really increases the value of research, because it buffers against blind spots (ID 12).

Balancing Societal Relevance and Fundamental Inquiry

While many participants emphasized societal relevance, others stressed the importance of fundamental work. One cautioned: "I do think […] we shouldn't neglect the influence of every little bit that kind of helps us understand how things work. And I think […] our main aim is to understand […] why do we do what we do? How do people function?" (ID 15). Another highlighted the often-unpredictable value of fundamental contributions: "Science is not a domain which is really standing by itself alone, independent from any other concerns and interests. […] Maybe 50 years from now,

certain things will become clearer. […] Certain things were very valuable, but maybe not that obvious at that time" (ID 16).

Reflection on Motivations and Research Practice

At the end of the interviews, participants were invited to revisit their questionnaire responses (MDRAI-R and Epistemic Values Scale) and reflect on whether they perceived tensions between what they aim to do and what they actually do in their research practice. The questionnaires served as a reflective stimulus. While several participants described little or no gap, many articulated specific constraints related to time, funding, and systemic pressures that limited their ability to fully align their work with what they want to do.

Alignment between Ideals and Practice

Several participants reported being broadly satisfied with the alignment between what they want to do and their actual practice. One expressed gratitude for having followed their own path:

[…] when I decided to be in the academy, I will do what I like to do. That was my decision. And this really informed my choices. And therefore, I am happy. But I also know that wherever you stand is not only your own making. […] But actually, at this point, I'm very grateful that I'm doing exactly what I want to do. So, I really am fine (ID 16).

Similarly, another highlighted the privilege of freedom and flexibility:

It's true that I think I might have been a bit privileged in a position. I was in a department that just let me work on whatever I wanted. So, I didn't feel a pressure that I needed to publish this on this particular issue. […] I think I had quite a lot of flexibility […] (ID 11).

Other participants also expressed this sense of satisfaction. One participant (ID 4) explained that they pursued practical questions they enjoyed and saw little tension while also noting that their main driver was teaching: "I've been identifying more and more as a teacher instead of as a researcher even though I still like to do it and I love to teach about research" (ID 4). Another said:

I think it fits well with what I had hoped to do with my life when I first started as a PhD. That's not true. When I started as a PhD, I wanted to change the whole world. And I think over the years, my expectations for what success is have become much more realistic. But […] I'm doing the things that I like to do. I'm doing research in a way that I like to do research (ID 5).

For one participant, research was framed as more than a job, but also as a vocation. They stated, "My job as a researcher was not just a position, [I] see it as a way to live and an inner calling"

(ID 14 [translated]). Another said simply: “I think I'm quite satisfied with the things that do influence my decisions. And I think I'm also quite lucky because of some collaborations […]. (ID 12)

Tensions and Constraints

By contrast, others described clear tensions between their ideals and their circumstances. One researcher felt bound by commitments made in grant proposals. They explained: “I wanted to turn it into something a little different but felt very constrained. […] I need to keep it there because that's what I promised” (ID 18). Even so, they concluded: “I feel blessed with what I do” (ID 18). Time constraints were another recurring challenge. One participant reflected:

It's remarkable how little you can do in a whole career, actually. […] If you then look back and think, well, how did the field progress the past [number] years? Yeah, then I have to admit, we did progress. But there's a lot of slowness also. But you can pinpoint […] your own or your team's contribution to the progress is tiny, actually. […] You have to be modest” (ID 3).

One participant emphasized the judgment calls researchers must make about when they know enough to contribute meaningfully: “So the tension is where you think, I think I saturated. […]. So, at some point, you have to say authoritatively I think I know what I want to know to say what I think I can say. That's a judgment call.” (ID 9). They linked this also to a broader sense of normative responsibility in academic life:

How do you want to be seen? […] You want to change the field? Better be good. […] I don't care if I publish something this year. Of course, I can afford it. Young researchers can't. But that also comes with responsibility, because if you can afford it, you should take the time to do it properly and not just write quickly (ID 9).

Funding was also described as a major gap between ideals and practice. One participant reflected: “[…] I think that the biggest tension for me is now the funding and the possibilities to do the work. […] I would love to say that this was not true. This statement about limiting funding does not constrain my choice of topic […], but it does” (ID 6). They noted that funding was tied not only to resources but also to recognition: “Of course, having such grants is also recognition” (ID 6). The same participant highlighted the undervaluation of collaborative labor:

This [research project] structure involves 60, 70, 80, 100 collaborators. […] Reviewers say you don't have any last author publications. […] Well, it takes me almost a day to get everything organized. […] But does it give me a publication? No. […] It's a silent contribution to science (ID 6).

Similarly, one participant reflected on the conflict between ideals of independence and the realities of the funding system:

That's for me is always struggling as a researcher is this idea that there's these two things at the same time: you have to plan your research, come up with great ideas and do something that is relevant and good. And at the same time, you have the whole system where you have to apply for the grants where you have to hit a different note, a different tone […]. It is impossible to be completely free in writing up what you really think is relevant and sending that to a review committee. […] I would love to do research without having review committees deciding about the type of research that I do (ID 7).

Others mentioned tensions shaped by the type of research they could pursue. One participant wished for more patient contact: "[…] Looking only at theoretical studies like reviews, it's about studies, not about people. […] I would like to go back a little bit to real people" (ID 2). Another described balancing curiosity with feasibility: "Assume I want to go in that direction. You can't jump a big leap. How would it fit in my line? […] What you can show is based on your CV, what you have done. […] That brings restrictions in a way. […] As much as I would want to investigate some research questions, there's also the reality of how would you do it? […] With certain things, maybe it's going to be too big of a leap" (ID 15).

Career Stages and Lasting Imprints

Career stage was consistently described as shaping the nature of tensions. One participant explained:

[…] the question of what influences your decisions and research depends very much on the phase of your career. […] In the beginning phases of your career, you're more dependent on others than in later phases. So, your decision-making process is also different (ID 3).

Another participant emphasized how pressures during their PhD had left a lasting imprint when reflecting on the answers they had given in the questionnaire: "I mean, I need to publish, I need to publish, I kind of said no, disagree. But I look back at it like a year ago or something, that was a major point of the career. Right? So, you had to. […] We were all publication driven" (ID 1). This had shaped a strategic orientation: "Everything I've started, I want to flip it. […] It's going to go to a journal. It's going to be published somehow, which is a very strategic line of thinking, which I didn't like" (ID 1).

Reflecting on the questionnaire item about conducting revolutionary versus safe research, the same participant admitted: "That's maybe not fully true. […] That's the person who I want to be. I want to be that daring person, but I think I'm also a little bit cautious" (ID 1).

Reflection as a Useful Exercise

Finally, participants expressed the usefulness of the reflective exercise itself. One explained:

I think it's just, like I said, it's an interesting questionnaire and set of questions because it does force a little bit to think about, to trace back the process because we just do this day-to-day and don't have to go back and think about the choices we made at one point. So, I think that's quite useful (ID 11).

Another noted that questionnaire responses sometimes revealed ambivalence: they strongly agreed with the statement "I aim to be recognized by my peers" but found recognition had not motivated their grant applications, concluding: "So both is true, right?" (ID 8). In a similar way, one participant highlighted how the reflective exercise made visible broader patterns of their academic journey:

And I think if I look back at the road, it's the road, but then it also broadened quite a bit. This interdisciplinarity also in terms of educational programs that we set up. And I think that's if I reflect on it now, I never take the time to reflect. So that is […] a nice thing as well. So, you become an expert in something, but then also learn again" (ID 17).

This reflection nicely captured the dynamic of narrowing to become an expert, but later broadening again—for example, through collaborations.

Summary

Taken together, these reflections show that while several researchers felt largely satisfied with the alignment between what they want to do and their practice, many described tensions linked to systemic pressures, funding constraints, or lack of time. Career stage emerged as a crucial factor, with early pressures leaving lasting imprints. Importantly, the act of reflection itself was valued as an opportunity to articulate these dynamics.

Discussion

This study explores how participants' personal and epistemic motivations and values shape their project choices. The thematic analysis is organized into themes at the micro-level (motivations such as curiosity and, recognition), meso-level (mentorship, field community, interdisciplinarity, and collaboration), and macro-level (funding, societal relevance, and innovation), followed by participants' epistemic research goals and values, and their reflections on their research practice. While the analysis focused on themes directly relevant to the research question, one additional theme—negative research environments shaping field choice—emerged that offered valuable contextual insight into participants' career trajectories (see Appendix G).

In the discussion, I connect the results to literature from philosophy and sociology of science, science and technology studies, and metascience. In doing so, I aim to show how participants' problem choice is nested within broader social, institutional, and societal contexts. Moreover, I will discuss what we can learn from insights into participants' epistemic research goals, epistemic values, and their reflection on a potential gap between what they want to do and what they do in practice.

Micro-Level Influences

Curiosity

Participants' emphasis on curiosity as an intrinsic driver resonates with classic accounts of science, where curiosity is framed as an epistemic virtue (Zagzebski, 1996). Their reflections also align with Kitcher's analysis of scientific goals. He distinguishes between personal epistemic goals—the desire to find out for oneself—and impersonal epistemic goals, which concern contributing to a shared body of knowledge and enabling others to understand. In light of this distinction, participants' accounts suggest a strong orientation toward the personal dimension of inquiry: the joy of exploring questions that matter to them and the satisfaction of finding out. At the same time, several participants linked this personal curiosity to a sense of contribution—an impersonal epistemic orientation—when they described the importance of adding a small piece to the puzzle.

Kitcher also notes that such curiosity-driven goals often coexist with desires for recognition or resources, yet the pure joy of discovery remains a central epistemic motivation (Kitcher, 1995). This interplay was evident in participants' accounts, where fascination with their research questions coexisted with awareness of institutional pressures and performance metrics. In motivational terms, this fascination reflects what self-determination theory describes as intrinsic motivation—engagement driven by inherent interest and enjoyment rather than external rewards—while the attention to recognition or advancement illustrates extrinsic motives.

Recognition

Participants' accounts of recognition as career necessity, validation, secondary byproduct, or as explicitly rejected illustrate the ambivalence Merton (1973) described: "Scientific inquiry, like human action generally, stems from a variety and amalgam of motives in which the passion for creating new knowledge is supported by the passion for recognition by peers and the derivative competition for place" (p. 218). While recognition can motivate and reassure, it also risks displacing the intrinsic pursuit of knowledge.Recognition is thus never merely personal but structurally built into how careers and credibility advance in science.

For early-career researchers, recognition was described as indispensable for career progress in academia—whether through publishing with prestigious journals, securing grants, or staying within mainstream topics. Participants' accounts also echo Glaser's (1963) observation that early career researchers tie recognition directly to job security. Participants' cautious adherence to mainstream lines also aligns with Horlings & Gurney's (2013) finding that early careers often converge before broadening later. From a structural perspective, this reflects Merton's (1973) claim that recognition is the mechanism by which careers advance, and Latour & Woolgar's (1979) notion of the credibility cycle, where recognition translates into resources that sustain future research. Participants' accounts of targeting prestigious outlets or avoiding deviation from mainstream topics illustrate how strongly their project choices are embedded in such cycles. In this sense, recognition for early careers is less an optional motivation than a structural necessity.

Several participants described recognition as a byproduct of good research, not a driver. This ambivalence echoes Merton's (1973) description of scientists' discomfort with their own desire for recognition: they may insist it is secondary, yet still admit to being gratified when it arrives. As Merton (1973) put it:

The ambivalence of scientists toward their own interest in having their priority recognized […] shows them to assume that such an ancillary motive somehow tarnishes the purity of their interest in scientific inquiry. Yet it need not be that scientists seek only to win the applause of their peers but, rather, that they are comforted, reassured and gratified by it when it does ring out (p. 218).

Glaser (1963) likewise found that mid-career scientists tied recognition to reputation-building but not always as their primary driver—mirroring participants who saw recognition as a catalyzer but not their core motivation. At the same time, participants' ambivalence toward recognition illustrates what Kitcher (2001) described as the need for institutional designs that channel competitive motives—such as the pursuit of prestige—toward collective epistemic goals, without letting them overshadow the fundamental aim of truth-seeking. One participant, for instance, reflected that it would be beneficial if the extensive organizational work she contributed to a large-scale collaboration—such as coordinating meetings among numerous partners—were not treated as invisible labor but formally acknowledged. Such reflections point to the importance of institutional mechanisms that reward this type of contribution alongside traditional research outputs—for instance, by recognizing it in grant evaluations or contributor statements. These structures could help align individual recognition with the collective epistemic goals of science.

When recognition was described as encouragement rather than career survival, participants highlighted its role in sustaining motivation. This resonates with Longino's (1990) argument that

knowledge is validated through social interaction and feedback within communities. Peer recognition here functions less as currency and more as affirmation of significance, which echoes Ziman's (1987) observation that problem significance is partly defined by community response. Also, participants described recognition as being taken seriously by certain researchers. This resonates with Storer's (1966) claim that each scientist defines their own set of colleagues whose work they see as related to their own. Scientists define certain groups of researchers, custom-made reference groups, which provide the immediate context in which recognition is granted or withheld.

Finally, some participants explicitly distanced themselves from recognition as a motivating factor. Glaser's (1963) finding that recognition maintains freedom rather than drives ambition in late-career scientists resonates with participants who said recognition wass not a motivation anymore because they already achieved it. Similarly, Ziman (1981) noted that while recognition and reward systems channel attention toward certain problems, many scientists resist overtly calculating their project choices in terms of prestige, since doing so risks undermining their credibility. This also echoes Hagstrom's (1965) observation that openly admitting recognition as a driver violates the ethos of good science: when project choice is too visibly guided by reputational calculation, it is judged as deviant. Participants' rejection of prestige-driven systems, or their preference for societal over academic recognition, highlights alternative orientations. This aligns with Santos et al.'s (2022) distinction between academia-driven and society-driven research agendas, where value is defined by societal stakeholders rather than peers.

Taken together, these accounts confirm recognition's dual nature: as the structural currency through which careers and credibility advance, and as the personal validation that sustains motivation and belonging. Participants' narratives also show its ambivalence: recognition is indispensable for survival, yet often disavowed as a true motive. This pattern underscores a central tension in science: recognition is both necessary and normatively problematic.

Mentorship

Several participants described mentors who modeled respect, care, and ethical conduct, and whose manner of doing science became a template for their own research style. Classic accounts argue that scientific knowledge is inherently social and norm-governed: objectivity emerges from community practices and critical interaction (Longino, 1990). Mentors are a primary conduit for transmitting those norms in day-to-day work (e.g., how to debate and treat colleagues). From a Mertonian perspective, the ethos of science (e.g., communalism, universalism, disinterestedness, organized skepticism) is not simply codified in formal rules but is learned through enculturation into research groups and academic communities (Merton, 1973). Participants' emphasis on their

mentors' ethical stance toward academic integrity, research ethics, and the respectful treatment of colleagues illustrates how such values are transmitted as lived practices rather than abstract principles.

Several participants described how mentors shaped their research identity, whether by encouraging exploratory approaches, modeling bridges between research and practice, or offering nudges that built confidence. Those accounts highlight the pivotal role of mentors in shaping participants' identities as researchers. Participants also highlighted how mentors influenced the degree of independence they could exercise. Some described projects closely aligned with their mentor's research agenda, while others emphasized divergence and the pursuit of their own path. As prior research notes, the continuation of the topic of the PhD depends on several factors, such as the PhD topic itself and its potential to 'grow', the work environment of the early career researcher, or their interest in the topic (Laudel & Gläser, 2008). Interestingly, one participant critically reflected on the academic ideal of independence from one's supervisor, questioning whether detachment should be considered an epistemic virtue. Their perspective points to the value of intellectual continuity, suggesting that cumulative progress emerges from sustained research lines, where ideas evolve across generations of researchers.

Overall, participants emphasized how mentors shaped their research identity and belonging in science. This resonates with Montgomery et al.'s (2022) argument that mentorship is a "critical yet undervalued dimension of academic life." While participants did not explicitly call for institutional change, their accounts nonetheless highlight the centrality of mentorship to researchers' development. This underscores the importance of fostering supportive mentoring relationships and ensuring that mentoring responsibilities are accompanied by appropriate recognition and accountability.

Meso-Level Influences

Field Community

The findings indicate that participants' project choices are shaped by both formal institutional structures and the informal networks in which they are embedded. Beyond official collaborations or departmental expectations, participants' narratives highlight the significance of their field communities as networks of colleagues and peers that inspire and orient individual research trajectories.

The participants' descriptions of conferences, informal exchanges, and collaborative ties align with what Contandriopoulos et al. (2018) describe as organic collaboration networks that

underlie institutional research networks. These organic networks have been differently conceptualized in the literature, e.g., as invisible colleges (Crane, 1972) or scientific communities (Hagstrom, 1965). As Contandriopoulos et al. (2018) point out, the formation of these informal networks is shaped by multiple factors, including disciplinary traditions, methodological approaches (e.g., researchers using quantitative methods tend to collaborate more; Moody, 2004), funding conditions, and individual preferences. One researcher explicitly mentioned being formed by a specific research tradition. This finding resonates with research in the sociology of science (Abbott, 2001; Knorr-Cetina, 1999; Shapin, 1995) that describes disciplinary culture as influence on researchers' choice of research problem. However, research traditions have also been criticised as impeding scientific progress and productive engagement (Lake, 2011) which did not surface in the interviews.

More broadly, researchers' networks can be seen as self-organizing structures which change constantly. Interestingly, this is a mechanism of implicit research formation that is not tied to any form of coordination and management. Community interaction seems to operate as an informal steering mechanism of scientific inquiry and also complements formal mechanisms such as peer review and funding calls.

Participants' accounts demonstrate that conversations at conferences or within professional circles often helped identify knowledge gaps and motivated researchers to pursue new lines of inquiry. These findings align with research shown that academic research is frequently driven by inspiration derived from other scholars—whether through publications, conference discussions, or participation in research networks (Defazio et al., 2009), and what researchers consider novel and worth pursuing. Being embedded in a network also opens up opportunities for visibility and participation, such as contribute to journal special issues or co-edit publications, as researchers also described in the interviews.

At the same time, participants described aligning their work with departmental structures and sector plans. This aligns with previous research describing institutional context as an influence on researcher's problem choice. Yet, narratives also revealed a dynamic process of adaptation: topics initially imposed by institutional circumstances could later be redirected to a research area that was interpreted as personally satisfying and meaningful. This process demonstrates how researchers actively negotiate and reconfigure their research identities within structural constraints throughout their career.

Interestingly, not all reflections on the community were positive. One participant expressed concerns about academic insularity and questioned whether scientific communities risk becoming

“too inward-looking” and detached from societal issues in general. Similar concerns have been raised in discussions of the “ivory tower” perception of academia and ongoing efforts to foster more socially responsive, mission-oriented research (Nowotny et al., 2006).

Taken together, participants’ accounts reveal researchers as actively engaging with the communal environments in which they work. Their networks and disciplinary communities both constrain and enable research, offering resources, recognition, and inspiration while also defining the boundaries of legitimate research.

Collaboration

Collaboration emerged as both an intellectual, social, and pragmatic resource for researchers. Participants described collaboration as providing access to expertise, methods, and data that would otherwise be unavailable. This aligns with prior research (Hagstrom, 1965; Melin, 2000) which suggests that collaboration is driven by the need for access to the research infrastructure and expertise required to answer questions of interest. Researchers also emphasized complementarity. The participants’ emphasis on complementarity illustrates this diversity in practice: in team science, complementarity can be enacted by taking on different roles and sharing tasks, as well as by combining diverse methodological and theoretical repertoires.

Moreover, participants highlighted positive effects of collaboration. For instance, they emphasized that collaborative settings offer opportunities for critical discussion and mutual quality control. This resonates with Vazire’s (2020) view that team science fosters greater mutual oversight, making errors less likely to go unnoticed.

Collaboration was also described as a deeply social and relational practice. Participants repeatedly emphasized reciprocity, collegiality, enjoyment, and “nice synergy” as core aspects of productive teamwork. Positive interpersonal dynamics were viewed as essential for both the continuity of collaborations and the satisfaction derived from them. These accounts highlight that collaboration is sustained not only by shared intellectual goals but also by affective ties. This resonates with research showing an association between interpersonal relationships and scientific productivity (DeHart, 2017; Love et al., 2023). It further emphasizes that effective teamwork depends not only on scientific expertise but also on the ability to cultivate constructive interpersonal relationships, similar to the role of community managers.

Participants’ accounts also pointed to different preferences in how they chose collaboration partners. Some valued long-term partnerships—often with mentors or colleagues they had worked with for a long time, even since their PhD—while one participant deliberately sought new

collaborators out of curiosity or a desire to explore unfamiliar topics. One participant, for example, noted intentionally having chosen a collaborator who was not one of the “usual suspects.” This diversity in collaboration strategies reflects the broader tension between comfort and exploration. Although people tend to be around similar others (Montoya & Horton, 2013), diversity can lead to higher effectiveness of teams (Liu et al., 2020). Taken together, these insights underscore the need for institutional and managerial strategies that actively promote diversity in collaborations, which in turn may foster more effective research outcomes.

While collaboration was generally valued and pursued in their current research projects, participants also described its limits and frustrations. Divergent interests were sometimes hard to reconcile throughout the collaboration. One participant explicitly mentioned disappointment when commitments broke down, or when colleagues disengaged with the collaboration, which created uneven workloads. This also supports the importance of evaluating shared goal setting at the beginning of collaborations to avoid misalignments later. Others expressed discomfort with collaborations with colleagues who were driven by publication pressure rather than genuine intellectual curiosity. These experiences show that collaboration can be beneficial but also emotionally demanding.

Participants also reflected on the effort required to manage large, diverse teams. One researcher working on a mega-study noted that the extensive coordination work involved in keeping a large team aligned is an invisible contribution. They described this as demotivating, since such efforts rarely translate into publications and are unrewarded in formal evaluation systems. These findings illustrate that for the academic system to become truly collaborative, it needs to build a reward structure that recognizes and incentivizes participation in collaborative research and big-team science. Participants’ accounts of the demands of managing large teams also underline how important leadership and communication skills are for successful collaboration. Prior research similarly emphasizes that leadership and management training are crucial for effective team science (Bennett & Gadlin, 2012; Conn et al., 2019; Forscher et al., 2023), and consequently, dedicated management teams should be created in larger collaborations.

Taken together, the findings show that collaboration serves multiple roles—intellectual, social, and organizational—that enhances research quality, creativity, and enjoyment, while also introducing new demands for coordination, communication, and recognition. Overall, collaboration emerges as both a cornerstone of effective science and a site where the values, skills, and structures of academic work must evolve to support truly collective and cumulative knowledge production.

Interdisciplinarity

Participants' accounts revealed a variety of perspectives on interdisciplinarity. For most, interdisciplinarity was not a conscious motivation when choosing projects but tends to emerge naturally from their research interests or from the structure of their fields. A smaller group described turning to interdisciplinary work out of dissatisfaction with the limits of monodisciplinary approaches, while others framed it as a practical necessity—either because their questions required expertise beyond a single discipline or because funding schemes increasingly demanded it. This diversity of perspectives mirrors broader debates in the literature on whether interdisciplinarity is an exceptional undertaking or an ordinary feature of contemporary knowledge production (Jacobs & Frickel, 2009).

Jacobs and Frickel (2009) argue that the common image of disciplines as isolated silos is misleading, since scholarship is already characterized by a dense web of connections across fields. In this sense, interdisciplinarity may be less an exceptional undertaking than an ordinary feature of knowledge production. Some participants turned to interdisciplinarity out of dissatisfaction with monodisciplinary work. Jacobs and Frickel (2009) note that such shifts often take the form of what they call scientific and intellectual movements, where new cross-disciplinary fields emerge when researchers collectively push beyond established boundaries.

For some participants, interdisciplinarity was framed as a practical necessity—either because their questions demanded expertise beyond a single discipline or because external funding increasingly required it. Jacobs and Frickel (2009) observe that the promise of interdisciplinarity is often cast in exactly these terms: it is expected to deliver problem-solving capacities that individual disciplines cannot achieve on their own. At the same time, they caution that this assumption is more rhetorical than empirical, since there is limited comparative evidence that interdisciplinary work consistently produces superior results.

One participant suggested that disciplinary boundaries are artificial and that reality itself does not divide along disciplinary lines. This aligns with Fuller (2003) who sees disciplines as "artificial holding patterns." Interdisciplinarity can rather be understood as a key internal driver of ongoing epistemic change. As Fuller (2000) observes, "disciplines were born interdisciplinary," as social movements engaging with diverse phenomena and aspects of life. Yet, while interdisciplinarity may be a natural feature of scientific evolution, participants also described difficulties in cross-disciplinary collaboration.

Challenges with interdisciplinary work were also central in our participants' accounts, particularly around developing a shared language and negotiating conceptual differences. Jacobs and Frickel (2009) identify these as epistemic and organizational barriers: differences in thought styles, methods, and institutional arrangements make cross-disciplinary collaboration more challenging than disciplinary work.

In general, interdisciplinarity might remain fragile without structures that manage interdependencies, align goals, and sustain collaboration. Rasti et al. (2025a) argue that coordination depends on three dimensions—shared goal setting, interdependencies, and management. Unbalanced or lack of coordination might often lead to inefficiencies or premature collapse of collaborations. This perspective helps explain why participants in our study experienced interdisciplinarity as challenging and difficult to maintain: without coordination mechanisms, interdisciplinary work remains ad hoc, contingent on individual initiative, and vulnerable once funding or institutional support ends.

Thinking about interdisciplinarity through the lens of coordination thus reframes it not simply as an intellectual orientation but as a structural condition. Where goal-setting, interdependency management, and collective oversight are in place, interdisciplinarity can move from being incidental, as several participants described, toward being sustainable, cumulative, and institutionally organized and recognized. Taken together, supporting interdisciplinarity thus requires more than encouraging cross-disciplinary projects—it calls for durable infrastructures and dedicated coordination roles.

Macro-Level Influences

Funding and Resource Constraints

Across most interviews, participants described working under relative resource scarcity. A common critique of funding systems was that applied research received more funding, consistent with prior work (Laudel & Gläser, 2014). Concerns were especially pronounced among those pursuing basic or theoretical research, who felt disadvantaged by criteria that privilege short-term applicability. Participants repeatedly reflected on how funder and policymaker priorities shape proposals and project selection. They worried that peer review is biased against risky or unconventional research which aligns with prior findings (Langfeldt, 2006). Moreover, some participants described tailoring projects to what "a committee wants to read," indicating a strategic alignment of their planned research with the goal of increasing chances of getting funded.

Another specific frustration concerned the short duration of project funding. As one participant noted, complex and wide-scope questions are difficult to pursue—and to plan for—within limited funding periods. Prior research supports this: stability and control over resources enable riskier designs and more ambitious agendas (Hellström et al., 2018; Laudel & Gläser, 2014). Overall, many accounts pointed to a loss of epistemic autonomy, suggesting a need for changes in funding architectures. Coping responses varied—from detesting the funding system, frustration and worry, to feelings of helplessness. Targeted institutional buffering such as bridge funding is therefore critical: baseline support during periods of unsuccessful applications can soften major disappointments and reduce perceived threats to a researcher's ongoing line of work, thereby preserving continuity, agency and control over own research lines. This matters for well-being, given the role of perceived control and self-efficacy (Bandura, 1982).

At the same time, participants demonstrated creative adaptation to scarcity—scaling questions to feasible scopes, staging projects, and selecting low-cost designs. While adaptive, such bricolage can bias designs toward short time-frames and low-cost approaches, potentially narrowing the kinds of questions that can be asked. Some also reported a withdrawal from competition, particularly for large grants: statements like "I detest the way it works" and descriptions of the process as "personally very intense" with work spilling into evenings and holidays make non-participation understandable. Yet this is also unfortunate for the research system: when capable researchers opt out, promising ideas may never enter competition and thus remain unfunded.

Concerns about the impact agenda further illustrate these tensions. While societal relevance is valued, several researchers felt that assembling partner consortia or crafting immediate impact narratives can become "far-fetched" for fundamental work. This suggests that basic versus applied projects should be assessed with tailored criteria, recognizing that impact in basic research is often delayed; prior research also notes downsides of impact-driven logics (McCowan, 2018).

Equity concerns also surfaced. It is plausible that alienation arises within the community, separating those who receive large grants from those who do not or no longer compete. Importantly, a legitimate funding system should be supported by the majority of researchers; without broad acceptance, the community risks being characterized by negative emotions toward large-grant recipients, which can corrode collegiality and collaboration. At the same time, participants' accounts show how not having those large grants negatively impacts career progress: receiving major awards was perceived as a decisive factor for reaching the full-professor stage. A fairer distribution of resources could mitigate this divide; notably, the worry that egalitarian sharing would unacceptably

dilute resources appears unfounded, with large room for more egalitarian allocation (Vaesen & Katzav, 2017).

Taken together, the findings reveal that funding availability and resource constraints exert a pervasive influence on how researchers select, design, and sustain their projects. Researchers strive to maintain autonomy and curiosity through adaptive strategies.

Academic Culture and Publication Pressure

The findings highlight how academic norms and evaluation systems continue to shape researchers' project choices. Participants' experiences suggest that traditional metrics of success—notably publication counts, journal prestige, and funding history—remain the dominant criteria in career advancement.

Researchers face constant pressure to produce and disseminate work in high-prestige journals in order to optimize their visibility and competitiveness. However, this quantitative emphasis often overlooks the quality and originality of research and may discourage scholars from pursuing deeper or high-risk research questions. Instead, the current incentive system may encourage researchers to pursue short-term goals (Trueblood et al., 2025).

The account of one participant depicting universities as "production factories" reflect the systemic nature of this publication pressure. This encourages productivity metrics that frequently conflict with responsible and integrative scholarship. One participant pointed to the "disciplining" of the organisation, where power operates not through overt coercion but through subtle forms of surveillance, normalization, and self-regulation. Within this framework, researchers internalize expectations of constant productivity and regulate their own behavior in accordance with institutional standards of productivity.

Overall, these findings contribute to ongoing debates about academic reform and research assessment to ultimately realign academic incentives and foster a healthier research culture. Initiatives advocate for alternative systems of evaluation that recognize a broader range of scholarly contributions. For example, universities in the Netherlands have introduced the Recognition & Rewards framework which bases assessment on a diverse range of quantitative and qualitative criteria (van Ravenzwaaij et al., 2023). By challenging the incentive structure that privileges numerous publications in prestigious journals, such reforms can mitigate the pressures that have long influenced researchers' behavior.

Societal Relevance

The findings indicate that participants were motivated by societal relevance to a considerable extent, though the strength and expression of this motivation varied across researchers. This aligns with results from prior research showing that societal relevance influences the choice of research (Rosenlund et al., 2017).

For many participants, societal relevance represents an intrinsic commitment—an ethical orientation toward producing knowledge that addresses tangible social problems. Participants who positioned societal relevance as their primary driver described a desire to contribute to real-world solutions. This orientation resonates with the broader shift towards more societally relevant research (Hessels & Van Lente, 2008). At the same time, it reflects an ethical awareness among researchers that research should fulfill the social contract of science. This emphasis on accountability and responsibility aligns with frameworks of Responsible Research and Innovation, which emphasize responsiveness to societal needs (Owen et al., 2012). In these participants, societal relevance appears as a core identity component and intrinsic ethical commitment.

A participant working on a mega-study mentioned that their contribution—analyzing a large dataset in one harmonized way—can be seen as an indirect yet crucial contribution to societal benefit. This highlights an alternative framing of impact: that robust and reliable knowledge is itself a societal good, even when immediate applications are absent. This perspective challenges narrow, output-based understandings of societal usefulness and underscores the importance of epistemic reliability and, hence, also the trustworthiness of research as a form of societal contribution.

Other participants articulated a more balanced stance, seeking to preserve the value of fundamental inquiry while maintaining sensitivity to practical relevance. They viewed societal impact as a long-term or indirect outcome, positioning basic research as the groundwork upon which applied research can later build. As Small et al. (2008) already showed, researchers with a focus on basic research do consider the wider public. However, as funders increasingly prioritize applied research and collaboration with non-academic partners, researchers focused on basic research may be at a disadvantage.

This stance should be supported by management mechanisms that ensure the groundwork being laid will be taken up by other researchers so that the research can also achieve the desired societal impact. Without coordination mechanisms, research might prematurely stop at a basic level without reaching societal impact. Overall, several participants criticized that funding agencies

forcefully encourage or even require researchers to have societal partners. This criticism was also voiced by researchers who do not work primarily in basic research.

Based on these findings, the question arises whether research with societal impact—what funders hope and researchers work for—is currently funded and directed in the right way. We should avoid situations where researchers engage in collaborations with non-academic partners merely to fulfill funding requirements, without genuine commitment or shared purpose. Collaboration with societal partners should not be only strategic. It requires coordination to ensure that both parties share goals and a clear vision of what they want to and can achieve within the resources available.

Moreover, the pressure to deliver research with societal impact can be challenging, even when researchers are motivated by pursuing societally relevant research. This pressure, introduced by funders, may be perceived as yet another research assessment factor on which researchers must perform well. It also requires improved assessment of how societally relevant research goals are pursued. Our sample showed a great variety of societally relevant research activities, such as collaborative research, contract research, consulting, public dialogue and outreach, and stakeholder engagement. This signals a very complex landscape of knowledge creation, and in their complexity, these activities are not yet well understood in terms of how they influence research and how researchers can best use these activities to benefit society.

Overall, societal relevance emerged as both an intrinsic driver and an external requirement. The answers reflected researchers' efforts to balance scientific autonomy, responsibility, and the growing institutional emphasis on demonstrating social impact.

Innovation

The accounts reveal that producing innovative or unconventional research was rarely an explicit motivation for researchers when choosing their projects. Rather, innovation appeared as an emergent property of research practice. Across interviews, there was little consensus on what innovativeness truly means. Although funders such as the NWO assign high weight to innovation in their evaluation criteria, these criteria are often not clearly defined in call documents. Given this institutional emphasis, one might have expected stronger motivation among researchers to align explicitly with this expectation. While some participants mentioned aligning their proposals with funder expectations in abstract terms, they did not articulate how they operationalize this alignment, also regarding innovation. However, what we know from prior research is that even when funding bodies explicitly encourage high-risk and innovative research, applicants often frame their proposals

to accommodate the conservative expectations of peer review (Barlösius & Blem, 2021; Philipps & Weißenborn, 2019).

The interviews shed light on how innovation operates as a discursive category in academic communities. In a few interviews, innovation was equated with novelty, reflecting the most immediate and intuitive understanding of the term; however, novelty constitutes only one aspect of what innovativeness can entail. However, understandings of innovation varied. If researchers align their work with their own, often idiosyncratic, interpretations of innovativeness, collective orientation toward a shared vision of innovation becomes unlikely. This lack of convergence may complicate collaboration and the coherence of innovation-driven funding programmes.

Notably, some participants explicitly distanced themselves from innovation as a guiding motivation. Their focus remained on traditional scholarship, established topics, and cumulative research rather than novelty for its own sake. They valued continuity and incremental progress — a stance characteristic of what Kuhn (1962) described as normal science. In this regime, conservative strategies are common, while riskier, more innovative strategies are rare.

Moreover, innovation was also described as relative — what appears innovative to one researcher may seem conventional to another, depending on disciplinary norms and individual research histories. This relativism underscores that the term innovation is dependent on the context it is used in. A recurring theme was the definition of innovation as effective problem-solving rather than radical novelty. Many participants emphasized usefulness, accumulation, and the incremental extension of knowledge. This reframing presents innovation as a process-oriented and cumulative phenomenon rather than as disruption. It frames innovation as a cumulative process of advancement rather than one that privileges radical novelty.

One participant explicitly associated innovation with critical awareness and intellectual freedom: questioning conventions, resisting cognitive constraints, and fostering epistemic creativity. In this sense, innovation refers to an attitude of inquiry rather than a method of inquiry or research outcome. Others described their projects as innovative in the following ways. For some, innovation meant taking on ambitious, high-risk projects, framed as intellectual challenges demanding courage and vision. Others located innovation in methodological advances — the development of methods and tools, or the novel application of existing ones. New methods can indeed reroute scientific practice and serve as foundations for new discoveries; they serve as critical and often independent foundation for future scientific discovery (Leahey, 2008; Shi et al., 2015).

Interestingly, participants rarely mentioned theoretical advances as innovation. Methodological advances typically referred to improved measurement techniques or the analysis of multiple datasets in one harmonized way. Following Kuhn (1962), theoretical advances arise only when existing frameworks can no longer accommodate accumulating anomalies, and as Cole (1983) argued, new theories at the research frontier must connect with other work to enter the paradigmatic core of a discipline. The limited attention to theory development observed here aligns with broader critiques about the lack of coordinated theory construction efforts (Borsboom et al., 2021; Gigerenzer, 1991, 2010; Meehl, 1978). This suggests that innovation in theory construction remains an underdeveloped yet crucial domain for progress in psychology and the social sciences in general.

Overall, these findings suggest that there is no universal or discipline-independent understanding of innovativeness. The description of research projects as traditional or innovative coexisted within the sample. Overall, for many participants, innovation functions less as a predetermined goal and more as a by-product of their research.

Epistemic Goals and Values

Epistemic Goals

Participants' accounts of their epistemic research goals reveal a diverse set of contributions to science. This diversity mirrors the distinction between basic, applied, and use-inspired basic research (Stokes, 1997). Most participants situated their work within applied or use-inspired basic research, while only a minority identified with purely theoretical or basic research orientations. In general, participants often framed their contribution as laying foundations for future inquiry, reflecting an understanding of science as cumulative. However, given the limited depth of information available about each individual project and my lack of knowledge about the specific bodies of literature to which they contribute, it is beyond my expertise to assess how these projects advance knowledge cumulatively within their respective fields. A more detailed evaluation of how each project addresses particular research questions lies beyond the scope of these conversations.

Several participants described methodological innovation as their primary form of contribution. For others, contribution was understood primarily in terms of theoretical development, with some aiming to create new conceptual frameworks. Yet, even among these theoretically oriented participants, there were differences in how they related their work to practice. Some emphasized the connection between theory and the world of practitioners and seek to make theoretical insights directly applicable. Others, in contrast, viewed their role as acquiring and

consolidating knowledge before it can be translated into practice which suggested a more sequential model of knowledge transfer. These divergent perspectives point to fundamentally different understandings of knowledge production—whether science should generate knowledge for practice, to be delivered and applied, or with practice, through co-creation and iterative engagement.

A further group of participants viewed contribution as broadening the empirical and geographic scope of science. By extending research beyond Western contexts, participants sought to address long-standing critiques concerning the lack of diversity and representativeness in scientific knowledge (Henrich et al., 2010). This orientation reflects a commitment to more inclusive and globally relevant science.

Overall, the findings show that participants pursue different epistemic goals, ranging from theoretical work and methodological innovation to contextual broadening and exploratory research. For research management, this diversity highlights the importance of ensuring that team composition and structure are well aligned with the project's epistemic aims. Teams should be designed to integrate the specific expertise required to achieve their intended research goals. In other words, effective research organization depends on aligning team composition with project-specific epistemic demands.

Epistemic Values

The findings reveal that participants hold nuanced views on what makes research valuable. Participants' accounts echo metascienctific work emphasizing transparency, replication, and cumulative synthesis as scientific values (Chalmers et al., 2014; Nosek et al., 2015; Open Science Collaboration, 2015). Specifically, participants' support of replication aligns with calls for more replication research (Koole & Lakens, 2012).

For many, valuable research is defined by its relevance to real-world problems—clinical or societal. Others, however, defended the importance of fundamental research, noting that knowledge often gains value retrospectively. Concerns about the future of basic research stem from growing pressure on scientists to demonstrate social and economic impact. The idea of use-inspired basic research (Stokes, 1997) was seen as a way to bridge the divide between applied and basic science by addressing fundamental questions rooted in social problems. Participants' caution can thus be read as a defence of academic autonomy and a call to strengthen implementation research so that such inquiry can reach its full potential.

Participants highlighted the importance of communicating research in a clear, accessible, and transparent way. This emphasis aligns with calls for open and reproducible science, such as the

UNESCO Recommendation on Open Science and the Transparency and Openness Promotion (TOP) Guidelines (Nosek et al., 2015; UNESCO, 2021). Similarly, Registered Reports have been proposed as a structural solution to enhance credibility and transparency (Nosek & Lakens, 2014). Honesty was framed not only as ethical integrity but also as precision in reporting findings to avoid misleading downstream research. This finding underscores concerns about inconsistent or incomplete reporting, which can obscure the robustness of results and limit cumulative progress. In addition, the lack of precision in reporting findings also points to an insufficient use of reporting guidelines such as CONSORT (Hopewell et al., 2025) or PRISMA (Page et al., 2021), which were designed to improve the completeness and clarity of research reports.

Participants also emphasized the importance of correcting the scientific record, reflecting science's self-corrective ideal. However, several participants described feeling less motivated or less involved in efforts to address scientific errors. While participants expressed strong support for correction in principle, their accounts raise questions about how the scientific community envisions effective self-correction—whether as the responsibility of a few dedicated individuals or as a collective responsibility and social process in which all or most researchers participate. Moreover, motivating a diverse community of scientists to engage in error correction could help detect and address "a more diverse, and therefore more comprehensive, range of scientific errors" (Vazire & Holcombe, 2022).

Taken together, participants' reflections point to differences in how different fields define and enact the value of correction. What counts as appropriate self-correction—replication, re-analysis, debate, or theoretical refinement—may vary across disciplines, suggesting that the norm of correction is interpreted through field-specific epistemic cultures rather than as a single universal practice. Recognizing this plurality highlights the need for cross-disciplinary reflection on how scientific communities sustain accountability and trust while respecting different modes of inquiry.

Interestingly, one participant noted deliberately avoiding exposing weak research which highlights the potential conflict-avoidance aspects of scientific error correction and peer feedback. This notion is further supported by a participant's notion that researchers have a lot of fear of reputational damage. Beyond transparency, science depends on transformative interrogation (Longino, 1990) as a key mechanism of self-correction. Systematically encouraging the identification of errors—and protecting those who point them out from negative repercussions—emerges as a crucial task for improving science.

One participant explicitly reflected on experienced hierarchies of value that privilege quantitative over qualitative research methods. Such hierarchies risk narrowing the epistemic scope

of science by equating rigor solely with the method of inquiry. Instead, it might be more useful to discuss advantages and disadvantages of specific research methods in the context of specific research problems (Allwood, 2012). Recognizing the distinctive contributions of qualitative research and countering misconceptions might help broaden what counts as valuable knowledge.

Also, participants explicitly pointed to the value of team science. This aligns with notions of the increasing recognition of the importance of coordination in scientific research (Rasti et al., 2025b) as it is evident in big team science projects which one of the participants was currently working on. Coordinated team efforts can improve science for different reasons, such as creating a more cumulative science (Rasti et al., 2025b).

Moreover, participants' accounts indicate that different individuals and disciplines prioritize different epistemic values, even when areas of overlap exist. Field-specific conceptions of quality have long been observed, for instance in the varying importance attributed to quality criteria during the manuscript review process (Cicchetti, 1991). Similarly, studies of grant peer review have demonstrated that the relative emphasis placed on theoretical versus methodological considerations varies between disciplines (Hartmann & Neidhardt, 1990). Participants employing qualitative methods frequently highlighted theoretical contribution as a central marker of value. Although Chase's (1970) study contrasted social with natural scientists rather than methodological orientations within a single field, the pattern he identified—social scientists giving higher rankings to theoretical significance—echoes the distinction found here between qualitative and quantitative researchers.

More broadly, participants' accounts align with Knorr-Cetina's (1999) conception of diverse epistemic cultures and with the notion of the disunity of the sciences proposed by Galison et al. (1996). Participants' diverse accounts of epistemic values also raise the question whether there is a single, unified scientific value system. Future research might explore these variations further such as investigating how discipline-specific notions of value influence judgments of quality of other disciplines.

Reflection on Motivations and Research Practice

Across accounts, the findings highlight a recurring tension: participants are motivated by intellectual curiosity and a desire to contribute meaningfully, yet their practices are shaped by systemic constraints and strategic considerations.

When participants reflected on the alignment—or misalignment—between their ideals and actual practice, there was a strong sense of alignment and self-determination for some researchers. They described being "lucky" or "privileged" to work in environments that allowed them to follow

their intellectual interests. Their accounts resonate with Deci and Ryan's (2000) self-determination theory, which identifies autonomy as a fundamental psychological need underlying intrinsic motivation and well-being.

However, participants described tensions linked to funding pressures (e.g., grant demands and recognition systems), time scarcity and productivity expectations, and evaluation practices that prioritize measurable outputs over collaborative contributions. The degrees of choice and control experienced by the researchers differed, based on career stage, but also based on amount of funding required for the type of research. Many participants experienced a form of constrained independence where they can still choose research questions but within tightly defined institutional and funding frameworks. Institutions and individuals must operate within externally defined rules and evaluative criteria that emphasize utility, value for money, and scientific excellence (Henkel, 2005). Within such frameworks, academic rights of self-determination and self-regulation are curtailed. Henkel (2005) draws on Elzinga's (1985) notion of epistemic drift to describe this process—where the orientation of research gradually shifts from internal scientific norms toward external demands. Moreover, participants' awareness that "having such grants is also recognition" points to an academic system where success is tied to resource accumulation and symbolic capital.

Participants' reflections can also be situated within broader coordination and reward challenges in contemporary science. Embedding research within more supportive frameworks that (a) broaden recognition for the often-invisible labor underlying team or large-scale collaborations—such as organizing meetings and events—and (b) better align basic inquiry with broader societal missions could help reduce these tensions. Such systemic reforms could gradually shift science from an individualistic, competitive model toward a collaborative, mission-oriented one in which researchers' intrinsic and normative motivations and values are sustained rather than constrained. In addition, policies that foster a normative motivation to publish—driven by advancing knowledge rather than personal career advancement—are particularly important, since the pressure to publish affects publication strategies (Johann et al., 2024).

Finally, participants' reflections and their positive feedback on the reflection exercise itself underscore the value of self-awareness and reflexivity in academic life. Creating institutional spaces for reflexivity—in research training, evaluation, and policy design—may help researchers to navigate structural pressures more consciously and to better align practice with epistemic motivations and values.

Limitations and Future Directions

Several limitations of this study should be acknowledged. First, I used a shortened version of the MDRAI-R. This may have affected the validity and reliability of the subscales, as some dimensions were represented by fewer items.

Second, this study did not aim to provide a full psychometric evaluation of the Epistemic Values Scale, which was newly developed for this project and primarily used as a reflective instrument in the qualitative component. Because the scale has not yet been validated, some items may have been influenced by my own disciplinary background in psychology and interest in metascience, raising concerns about content validity. This orientation may have made certain items less applicable to disciplines with qualitative research traditions. Indeed, participants with qualitative orientations noted that some statements—such as those referring to the reanalysis of existing findings for correctness—were difficult to answer because such practices were not part of their methodological repertoire. Overall, different items and dimensions might apply differently do different groups of researchers. After thorough validation of the scale, repeating this study with a larger, more diverse group of participants allows to learn more about the reasons underlying those differences.

Since not all items of the Epistemic Values Scale were equally relevant or meaningful across disciplines within the social sciences, there is a need for further refinement of both the construct and the scale's content. Rather than offering a definitive measurement tool, the Epistemic Values Scale should be interpreted as a first step toward a more comprehensive and empirically grounded approach to studying epistemic values and their influence on researchers' choices.

This study employed a mixed-methods approach to capture both dispositional orientations and reflections on actual project choices. However, the interpretation of the quantitative data were limited by the small sample size. As a result, the quantitative data were primarily used descriptively rather than for inferential purposes. In addition, the coding process was conducted solely by myself. Although I practiced reflexivity, repeatedly reviewed the transcripts and codes, and discussed the interviews with my supervisors, there was no second coder to provide intercoder reliability.

The findings are also context-specific and exploratory and reflect the experiences of researchers within a single faculty at a Dutch university. However, the sample was diverse consisting of researchers across diverse career stages and disciplines within the Social Sciences. Consequently, the study offers insight into motivations and values in a particular academic context but cannot claim generalizability. Researchers sometimes also talked about more than one specific research project. In

addition, since I aimed to limit sessions to approximately 60 minutes, opportunities for deeper follow-up questioning were sometimes restricted.

Another challenge was interpreting how researchers themselves situated their projects within cumulative science. While all participants provided understandable rationales for how their work built on prior research, it was not possible to assess to what extent these projects indeed contributed to cumulative knowledge, given my limited knowledge about their research projects and fields.

Building on the study's findings, future studies could explore coordination, societal engagement, correction practices, and resource distribution in greater depth.

First, future research could assess the extent to which research projects are effectively organized around shared goals, interdependencies, and management structures (Rasti et al., 2025a). Due to the limited time available in the interviews, this study was unable to explore in depth the coordination strategies and arrangements researchers employ. Future work could therefore examine how projects align along these dimensions, how researchers currently evaluate their coordination practices, and what lessons can be drawn from instances of failed or suboptimal coordination.

Second, future research could investigate how to strengthen the translation of basic research into societal impact. This might involve evaluating coordination mechanisms such as translation or brokerage teams that facilitate the movement of fundamental research toward practical application. This would help researchers with a focus on basic research to demonstrate the relevance and value of their research, which is particularly important since funders favour applied research.

Third, there is a need to develop a typology of impact pathways that systematically maps the diverse forms of societally engaged research—such as consultancy, public dialogue, and outreach—and examines their respective epistemic and societal consequences.

Fourth, future studies should examine how error-correction practices operate across fields. Such research could also identify both formal and informal protective mechanisms that safeguard researchers who expose errors, thereby fostering a more open, accountable, and safe research culture.

Fifth, future research could develop and test shorter, tailored reflection tools for use within research organizations, building on the approach employed in this study. Integrating such tools into existing institutional routines—whether through regular or large-scale implementation—could foster a more self-reflective scientific culture, enabling researchers to more consciously align their practices with their epistemic values and motivations.

Finally, more research is needed on alternative models of research funding and resource distribution. For instance, investigating approaches such as departmental or unit-level bridge funding—which some research units already practice—could show how they promote equity by reducing dependence on competitive grants and allowing more stable—and potentially even more risk-tolerant—research agendas.

Conclusion

The present study examines how researchers choose specific projects by linking their motivations and values with the institutional and structural conditions in which academic work takes place. It represents an initial effort to empirically investigate epistemic values through the development of a self-report scale. Despite their centrality to scientific reasoning and behavior, epistemic values have not been examined using empirical methods. The present study contributes to the understanding of epistemic values by designing and testing a preliminary instrument intended to capture how researchers understand and enact epistemic values in their research project choice.

The results of the interviews reveal that while curiosity and societal relevance are important drivers of research, structural pressures—such as funding systems and evaluation criteria—often steer choices toward what is fundable or strategically safe. These tensions are particularly prevalent for early-career researchers, who face limited autonomy and high-performance demands.

A central implication is that a more rewarding and satisfying research system requires closer alignment between researchers' motivations, values, and collective scientific goals. The study also highlights the usefulness of reflection: providing spaces for researchers to examine how their ideals connect to their practice can help foster more deliberate, transparent, and value-conscious decision-making. Strengthening this alignment—through more flexible funding, supportive mentorship, greater research coordination, and evaluation systems that recognize diverse contributions—can help build and sustain a research culture that balances individual goals with collective scientific goals and societal responsibility.

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Appendices

Appendix A

Multidimensional Research Agendas Inventory-Revised (original version)

Scientific Ambition

I aim to one day be one of the most respected experts in my field.

Being a highly regarded expert is one of my career goals.

I aim to be recognized by my peers.

I feel the need to constantly publish new and interesting papers.

I am constantly striving to publish new papers.

I am driven to publish papers.

Divergence

I look forward to diversifying into other fields.

I would be interested in pursuing research in other fields.

I would like to publish in different fields.

I enjoy multidisciplinary research more than single-disciplinary research.

Multidisciplinary research is more interesting than single-disciplinary research.

I prefer to work with multidisciplinary rather than single-disciplinary teams.

Collaboration

My publications are enhanced by collaboration with other authors.

I often seek peers with whom I can collaborate on publications.

I enjoy conducting collaborative research with my peers.

My peers often seek to collaborate with me in their publications.

I am often invited to collaborate with my peers.

I am frequently invited to participate in research collaborations due to my reputation.

Mentor Influence

Part of my work is largely due to my PhD mentor.

My research choices are highly influenced by my PhD mentor's opinion.

My PhD mentor is responsible for a large part of my work.

My PhD mentor largely determines my research topics.

Tolerance of Low Funding

Limited funding does not constrain my choice of topic.

Highly limited funding does not constrain my choice of topic.

The availability of research funding for a certain topic does not influence my decision to conduct research on that topic.

I am not discouraged by the lack of funding on a certain topic.

Discovery

I would rather conduct revolutionary research with little chance of success than replicate research with a high probability of success.

I prefer “innovative” research to “safe” research, even when the odds of success are much lower.

I would rather engage in new research endeavors, even when success is unlikely, than safe research that contributes little to the field.

I am driven by innovative research.

Academia Driven

My choice of topics is determined by my field community.

I often decide my research agenda in collaboration with my field community.

I adjust my research agenda based on my institution’s demands.

My research agenda is aligned with my institution’s research strategies.

Society Driven

I decide my research topic based on societal challenges.

Societal challenges drive my research choices.

I often strive to engage in issues that address societal challenges.

I choose my research topics based on my interactions with my non-academic peers.

I consider my research topics myself, but this consideration often occurs after I hear what my non-academic peers have to say about these topics.

I consider the opinions of my non-academic peers when I choose my research topics.

Appendix B

Multidimensional Research Agendas Inventory-Revised (short version)

Scientific Ambition

Being a highly regarded expert is one of my career goals.

I aim to be recognized by my peers.

I am constantly striving to publish new papers.

Divergence

I would be interested in pursuing research in other fields.

I enjoy multidisciplinary research more than single-disciplinary research.

Collaboration

My publications are enhanced by collaboration with other authors.

I enjoy conducting collaborative research with my peers.

I am often invited to collaborate with my peers.

Mentor Influence

Part of my work is largely due to my PhD mentor's ideas. (after session 6 updated to: Part of my work is largely due to my former PhD mentor's ideas.)

My research choices are highly influenced by my PhD mentor's ideas. (after session 6 updated to: My research choices are highly influenced by my former PhD mentor's ideas.)

Tolerance of Low Funding

Limited funding does not constrain my choice of topic.

I am not discouraged by the lack of funding on a certain topic.

Discovery

I would rather conduct revolutionary research with little chance of success than replicate research with a high probability of success.

I am driven by innovative research.

Academia Driven

I often decide my research agenda in collaboration with my field community.

I adjust my research agenda based on my institution's demands.

My research agenda is aligned with my institution's research strategies.

Society Driven

Societal challenges drive my research choices.

I consider the opinions of my nonacademic peers when I choose my research topics.

Appendix C

Epistemic Values Scale

Reliability and Truth

I want my research to represent the truth as accurately as possible.

I want my research to serve as reliable evidence.

I want others to rely on my research because of its methodological rigor.

I want to communicate my research truthfully to others.

I believe it's acceptable to sometimes prioritize speed over thoroughness in research.*

Cumulative Collective Knowledge

I want my research to contribute reliable evidence to the literature that other researchers can build upon.

I want my research to contribute to resolving long-standing research questions.

I want my research to be relevant and useful for other researchers.

I want my research to remain valuable to the field in the future.

I want my research to be updated by the scientific community. (After participant 6 updated to: I want my research to be updated *or corrected* by the scientific community.)

I prefer developing new ideas over engaging with existing research.*

I believe that building on existing literature can constrain originality in research.*

I want my research to be distinct from the existing body of research.*

Criticism and Error Correction

I aim to identify errors in published research through careful reanalysis.

I want my research to challenge and potentially falsify existing findings.

I aim to critically examine and expose the limitations of existing research.

I prioritize original contributions over correcting the work of other researchers. (After participant 6 updated to: I prioritize original contributions over correcting the *published* work of other researchers.)

I see it as my responsibility to help correct inaccuracies in the scientific record.

Once research has passed peer review, I generally assume it's reliable.*

I believe it's often unproductive to reevaluate accepted findings.*

No study is perfect.

Note. * Indicates a reverse-worded item.

Appendix D

Descriptive Statistics of MDRAI-R and Epistemic Values Scale

Table 1

Descriptive Statistics of Items of the MDRAI-R (short)

No.	Item	Mean	SD	Min	Max
Scientific Ambition					
1	Being a highly regarded expert is one of my career goals.	4.44	1.42	1.00	7.00
2	I aim to be recognized by my peers.	5.11	0.83	4.00	7.00
3	I am constantly striving to publish new papers.	3.72	1.45	1.00	7.00
Divergence					
4	I would be interested in pursuing research in other fields.	4.39	1.46	2.00	7.00
5	I enjoy multidisciplinary research more than single-disciplinary research.	4.94	1.55	2.00	7.00
Collaboration					
6	My publications are enhanced by collaboration with other authors.	6.11	1.13	4.00	7.00
7	I enjoy conducting collaborative research with my peers.	6.33	0.91	4.00	7.00
8	I am often invited to collaborate with my peers.	4.83	1.10	2.00	6.00
Mentor Influence					
9	Part of my work is largely due to my former PhD mentor's ideas.	3.47	1.55	1.00	7.00
10	My research choices are highly influenced by my former PhD mentor's ideas.	4	1.54	1.00	7.00
Tolerance of Low Funding					
11	Limited funding does not constrain my choice of topic.	4.41	1.66	1.00	7.00
12	I am not discouraged by the lack of funding on a certain topic.	4.11	1.71	1.00	7.00
Discovery					
13	I would rather conduct revolutionary research with little chance of success than replicate research with a high probability of success.	5.11	1.37	2.00	7.00
14	I am driven by innovative research.	5.72	1.13	3.00	7.00
Academia Driven					
15	I often decide my research agenda in collaboration with my field community.	4.17	1.38	1.00	6.00
16	I adjust my research agenda based on my institution's demands.	3.5	1.25	1.00	6.00
17	My research agenda is aligned with my institution's research strategies.	4.78	1.06	3.00	7.00
Society Driven					
18	Societal challenges drive my research choices.	5.47	1.28	3.00	7.00
19	I consider the opinions of my nonacademic peers when I choose my research topics.	4.67	1.5	2.00	7.00

Table 2

Descriptive Statistics of Subscales of MDRAI-R (short)

Subscale	# Items	Mean	SD	Min	Max
Scientific Ambition	3	4.43	0.93	2.67	6.00
Divergence	2	4.67	1.00	2.50	6.50
Collaboration	3	5.76	1.00	2.50	6.50
Mentor Influence	2	3.74	0.86	3.33	6.67
Tolerance of Low Funding	2	4.28	1.53	1.50	7.00
Discovery	2	5.42	1.18	3.00	7.00
Academia Driven	3	4.15	0.86	2.33	5.33
Society Driven	2	4.97	1.30	2.00	6.50

Table 3

Descriptive Statistics of Items of the Epistemic Values Scale

No.	Item	Mean	SD	Min	Max
Reliability and Truth					
1	I want my research to represent the truth as accurately as possible.	6.22	0.94	5	7
2	I want my research to serve as reliable evidence.	6.39	0.85	4	7
3	I want others to rely on my research because of its methodological rigor.	5.17	1.25	3	7
4	I want to communicate my research truthfully to others.	6.61	0.61	5	7
5	I believe it's acceptable to sometimes prioritize speed over thoroughness in research.*	4.67	1.68	1	7
Cumulative Collective Knowledge					
6	I want my research to contribute reliable evidence to the literature that other researchers can build upon.	6.00	0.84	5	7
7	I want my research to contribute to resolving long-standing research questions.	5.33	1.14	3	7
8	I want my research to be relevant and useful for other researchers.	6.17	0.62	5	7
9	I want my research to remain valuable to the field in the future.	6.00	0.91	4	7
10	I want my research to be updated or corrected by the scientific community.	5.67	1.08	3	7
11	I prefer developing new ideas over engaging with existing research.*	3.33	1.50	1	6
12	I believe that building on existing literature can constrain originality in research.*	4.67	1.28	3	7
13	I want my research to be distinct from the existing body of research.*	3.71	1.21	1	6
Criticism and Error Correction					
14	I aim to identify errors in published research through careful reanalysis.	3.00	1.27	1	5
15	I want my research to challenge and potentially falsify existing findings.	4.39	0.92	3	6
16	I aim to critically examine and expose the limitations of existing research.	4.94	1.55	2	7
17	I prioritize original contributions over correcting the published work of other researchers.*	2.22	1.17	1	5
18	I see it as my responsibility to help correct inaccuracies in the scientific record.	4.67	1.03	3	6
19	Once research has passed peer review, I generally assume it's reliable.*	4.72	1.27	3	7
20	I believe it's often unproductive to reevaluate accepted findings.*	5.83	0.99	4	7
21	No study is perfect.	6.5	1.04	3	7

Note. * Indicates a reverse-worded item.

Table 4

Descriptive Statistics of Subscales of the Epistemic Values Scale

Subscale	# Items	Mean	SD	Min	Max
Reliability and Truth	5	5.81	0.82	4.20	7.00
Cumulative Collective Knowledge	8	5.12	0.59	4.25	6.25
Criticism and Error Correction	8	4.55	0.66	3.12	5.86

Appendix E

Interview Questions

Opening Questions

- Can you tell me a bit about one of your current research projects and how it started?
- Can you briefly elaborate on what influenced your decision to do this study?

Motivations

- To what extent did your motivation to pursue innovative or unconventional research influence your decision to do this study?
- To what extent did your motivation to gain academic recognition influence your decision to do this study?
- To what extent did your PhD mentor's ideas influence your decision to do this study? (after participant 6 updated to: To what extent did your former PhD mentor's ideas influence your decision to do this study?)
- To what extent did your collaborators or the motivation to collaborate influence your decision to do this study?
- To what extent did your motivation to cross disciplinary boundaries influence your decision to do this research?
- To what extent did your field community influence your decision to do this study?
- To what extent did societal relevance influence your decision to do this study?
- To what extent did resource constraints influence your decision to do this study?

Cumulative Science

- To what extent did your past research influence your decision to do this study?
- In what way does this study contribute to the larger scientific literature on this topic?

Epistemic Values

- In general, what do you think are aspects of studies that make a valuable contribution to science?
- For your current study, why do you think it contributes to science?

Reflection

To conclude, I'd like to invite you to reflect on your research practice. Now that you've answered the questionnaires and had a chance to reflect on your current research practice, do you notice any areas where your actual behavior differs from what you want to do? If so, in which area do you feel a divergence or tension occurs? Feel free to take a moment to revisit your responses to the questionnaire as a reference.

Appendix F

Information Letter, Informed Consent Form, and Debriefing Letter

Universiteit Leiden

Faculty of Social Sciences
Institute of Psychology

Information letter for participants

"The Role of Motivations and Values in Researchers' Decision-Making"

Dear Participant,

You are invited to take part in a scientific study. This study is being conducted by Julia Weschenfelder, supervised by Dr. Anna van't Veer of the Methodology and Statistics Unit at Leiden University and Dr. Daniël Lakens and Sajedeh Rasti of the Human-Technology Interaction group at Eindhoven University of Technology (TU/e), and has been approved by the Psychology Research Ethics Committee of Leiden University (reference number: 2025-05-14-A.E. van 't Veer-V2-6035).

Purpose of the study

This study investigates the factors influencing researchers' decision-making processes in academic settings. Specifically, the study examines how motivations and values impact decisions within the context of a specific research project.

Participation

Your participation in this research project does not involve any physical, legal or economic risks. You do not have to answer questions which you do not wish to answer. Your participation is voluntary. This means that you may end your participation at any moment you choose by letting the researcher know this. You do not have to explain why you decided to end your participation in the research project. None of this will have any negative consequences for you whatsoever.

Procedure

Your participation will involve completing a questionnaire followed by an interview to help us understand how you make research-related decisions. If possible, the data collection (survey and interview) will take place in a room at the Faculty of Social and Behavioral Sciences. You will be asked to reserve 60 minutes of your time.

Benefits and risks

Your participation in this research project does not involve any physical, legal or economic risks. You do not have to answer questions which you do not wish to answer. Your participation is voluntary. This means that you may end your participation at any moment you choose by letting the researcher know this. You do not have to explain why you decided to end your participation in the research project. None of this will have any negative consequences for you whatsoever.

Confidentiality, use, and privacy

How do we protect your privacy?

We will do everything we can to protect your privacy as best as possible. The research results that will be published will not in any way contain confidential information or personal data from or about you through which anyone can recognize you, unless in our consent form you have explicitly given your consent for mentioning your name, for example in a quote.

Within the framework of the research project we process the following personal data: name, e-mail, audio recording. The personal data that were gathered via audio recording and questionnaire within the framework of this research project, will be stored on a secure server, as required by Leiden University regulations on data management. The raw and processed research data will be retained for a period of 10 years. The research data will, if necessary (e.g. for a check on scientific integrity) and only in anonymous form be made available to persons outside the research group.

We wish to share the transcript of the recordings as open data for future academic purposes and scientific reviews. However, due to the nature of the questions, we cannot guarantee complete anonymity. Later in this document, we ask you to choose between these options regarding sharing the transcript of your interview: a) openly share the transcript with their identity anonymized as much as possible, b) share the transcript with limited access for research purposes only, or c) decline to share the data entirely.

Can you withdraw your consent for the use of your data?
Participation in this research project is entirely voluntary. You may end your participation in the research project at any moment or withdraw your consent to using your data for the research without specifying any reason. Ending your participation will have no disadvantageous consequences for you.
For more information on data privacy and your rights, please check the European Union's data privacy law, known as the General Data Protection Regulation ("GDPR").

Contact information

If you have any questions before or after participating in this study, you can contact the principal investigator, Dr. Anna Van't Veer, a.e.van.t.veer@fsw.leidenuniv.nl, or Julia Weschenfelder, J.Weschenfelder@umail.leidenuniv.nl.

You can also contact the (principal) investigator if you have a complaint. If you prefer not to do so, you can contact the Contact point for research participants at the Faculty of Social Sciences of Leiden University: Contactpuntparticipanten@fsw.leidenuniv.nl

If you have any questions or complaints about your privacy or the processing of your personal data, you can contact the privacy officer of Leiden University: privacy@bb.leidenuniv.nl

Universiteit Leiden

Faculty of Social Sciences
Institute of Psychology

Informed consent form for participants

I have been asked to give permission to participate in the study **"The Role of Motivations and Values in Researchers' Decision-Making".**

By signing this consent form I acknowledge the following:

1. I am sufficiently informed about the research project through a separate information letter. I have read the information letter and have had the opportunity to ask questions. These questions have been answered satisfactorily.

2. I take part in this research project voluntarily. There is no explicit or implicit pressure for me to take part in this research project. It is clear to me that I can end participation in this research project at any moment, without giving any reason. I do not have to answer a question if I do not wish to do so.

Furthermore, I consent to the following parts of the research project:

3. I consent to processing my personal data gathered during the research in the way described in the information letter.

YES ☐ NO☐

4. I consent to using my answers for example quotes in the research publications – without my name being published in these.

YES ☐ NO☐

5. I consent the transcript of my interview to:

☐ be openly shared with anonomising my identity and masking contextual information as much as possible.

☐ only shared to people with research purpose only.

☐ I wish the transcript of my interview not to be shared with anyone.

I, (NAME) ……
consent to participate in this study.

Signed by ……..

Date ……..

Signature ……..

Universiteit Leiden

Faculty of Social Sciences
Institute of Psychology

Debriefing letter for participants

"The Role of Motivations and Values in Researchers' Decision-Making"

Thank you for participating in this study. We would now like to provide further explanation about this study.

This study aims to explore how researchers make decisions in a research project. Specifically, we focus on understanding the influence of epistemic and non-epistemic motivations and values on the research process. Our goal is to understand how these motivations and values shape concrete decisions within the research process and whether there is a gap between what researchers would like to do and what they do in practice.

By sharing your experiences and insights, you've helped us better understand the complex factors that influence decision-making in a specific research project. Your input, along with that of other participants, will be instrumental in developing a more nuanced understanding of the motivations and values that drive researchers' choices.
As the scientific community moves toward greater openness and transparency, it is crucial to better understand how motivations and values shape the research process. This study aims to offer insights into how research practices might be improved for greater alignment between individual and scientific goals. Moreover, this study aims to lay the groundwork for the development of a scale that assesses the influence of epistemic and non-epistemic motivations and values in a research project.

If you would like to withdraw your consent to use your research data based on the information above, please contact the investigator listed below within two weeks. If you choose to withdraw, this will not negatively affect you in any way.

We have asked you some personal questions. It might be that answering certain questions made you feel upset or experience negative emotions. It is okay and natural to feel this way, and these negative feelings should quickly diminish after the study's completion. If you still experience difficulties after the study or would like to talk to someone about it, there is a possibility to get help from different sources.

Please do not share the information in this debriefing letter with other potential participants, as the information could influence their behavior during the study.

Contact information

If you have any questions after participating in this study, you can contact the (principal) investigator, Anna van't Veer at a.e.van.t.veer@fsw.leidenuniv.nl, or Julia Weschenfelder at J.Weschenfelder@leidenuniv.nl .

You can contact the investigator if you have a complaint. If you prefer not to do so, you can contact the Contact point for research participants at the Faculty of Social Sciences of Leiden University:

Contactpuntparticipanten@fsw.leidenuniv.nl

If you have any questions or complaints about your privacy or the processing of your personal data, you can contact the privacy officer of Leiden University: privacy@bb.leidenuniv.nl

Appendix G

Thematic Analysis: Additional Theme not Relevant to Research Question

During the thematic analysis, one theme emerged that, while important, was not directly relevant to answering the guiding research question of this study—namely, what motivates researchers' *current project choices*. This theme is reported here to acknowledge its presence in the data and to provide context for future research.

Negative research environments shaping field choice

Three participants described how experiences of unsupportive or unsafe academic environments earlier in their careers influenced their decision to change fields.

- "[…] as a PhD […], I didn't really have much of a field community. I also noticed reviewers of articles were just quite mean" (ID 1).
- "And I have to say the research was quite interesting. But the research environment was very bad, actually. So it was a highly, I would say, quarrelsome environment. Yeah, not fun to work there. So I specifically looked for a job in another field" (ID 3).
- "But the Institute where I ended up working was pretty horrible when I got there […] it was a really unsafe academic environment where the professors that I worked under […] were really controlling and restrictive. […] And luckily, that changed over the years. So now it's much better at our Institute" (ID 4).

While these experiences clearly shaped participants' broader career trajectories, they did not emerge as direct influences on their present project choices. For this reason, they were not included in the main thematic structure of the results, but are nonetheless important in understanding the role of academic environments in shaping long-term research directions.