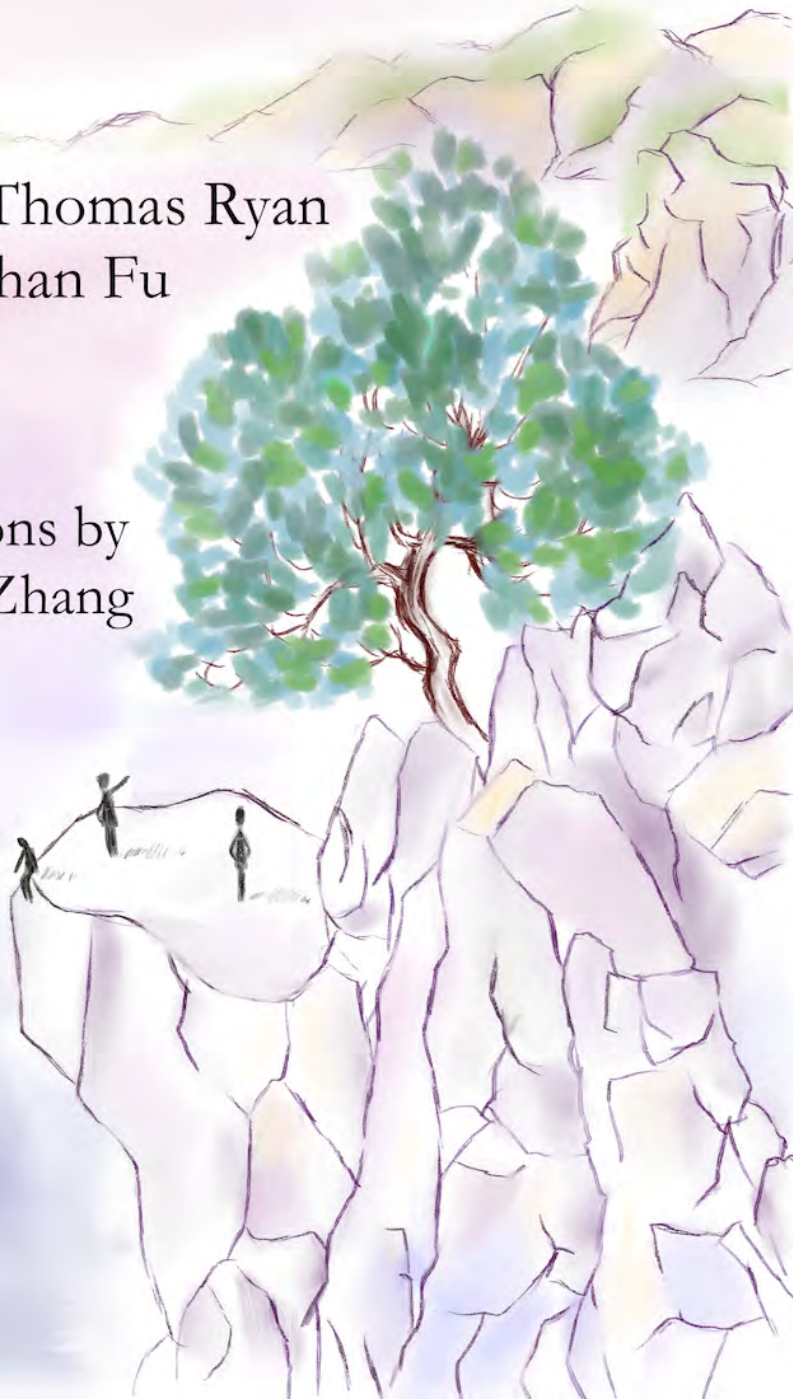


Paths to Research

A companion to research as a career

Christopher Thomas Ryan
and Runshan Fu

with illustrations by
Mengzhenyu Zhang



Paths to research:
A companion to research as a career

Christopher Thomas Ryan and Runshan Fu
featuring illustrations by Mengzhenyu Zhang

PRELIMINARY VERSION

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Dedication

To my wife Cecilia, none of this would have been possible without her love and determination. – C.T.R.

To my mom Cuie, who gives me ultimate love and respect since the first day of my life. – R.F.

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谋事在人，
成事在天。

*Process belongs to humans,
outcomes belong to the heavens.*

— 罗贯中 (Guanzhong Luo)

Foreword

Embarking on a career in research is not an undertaking for those seeking a calm, predictable life. From the birth of an idea to a published paper, and from getting into a doctoral program to making it to tenure, the journey is fraught with enough obstacles to catch even the most experienced off guard, while constantly shaping our minds and souls.

The pull of research is incredibly potent. To paraphrase Jerry Seinfeld, research is like “the lure of the siren’s song. Never what it seems, yet who among us can resist?” In the United States, the number of doctoral degree holders is a staggering 4.5 million. That’s doubled since the turn of the century, while the population has grown only 17% over the same period.

Douglas Prasher is one of those millions. Once a promising chemist, his quest for funding and tenure was fraught with setbacks that eventually led him to quit academia in the 1990s. In an ironic turn of events, in 2008, while Prasher was working as a courtesy van driver for a Toyota dealership in Huntsville, Alabama, it was announced that his groundbreaking research on green fluorescent protein had led to the Nobel Prize in Chemistry—awarded to *others* who had advanced Prasher’s work using the very samples he had produced.

To the casual observer, a career in research can feel like a perpetual cruise ship. Those who are privileged continue their journey while many fellow passengers disembark, voluntarily or otherwise. Would continued perseverance have brought greater happiness? What does happiness even mean to a researcher?

In the wise words of Laozi (老子):

孰能濁以靜之徐清?
孰能安以動之徐生?
保此道不欲盈,

夫唯不盈，
故能蔽而新成。

Who can calm the troubled waters till it settles into clarity?
Who can stir the stillness till it teems with new life?
Those who walk this path don't seek to be filled.
Only those content in their emptiness can renew,
remaining whole yet ever-changing.

* * *

A journey of research unfolds along a serpentine course, like the meandering paths in a Chinese garden, such as the Sun Yat-Sen Garden in Vancouver or the Ming Hall (明軒) at the Metropolitan Museum of Art in New York. But as Chen Congzhou (陳從周), the revered artist and designer of the Ming Hall, noted, every bend, every twist in the path has a deliberate purpose. It invites introspection, fosters a deep connection with our surroundings, and ignites the flames of exploration. Each step on the path is a new revelation, a unique perspective, a fresh insight.

Convoluted and mercurial it is, research has its unique beauty and charm. This is poetically articulated in *Paths to Research* by my friend Chris Ryan and his collaborator Runshan Fu. Their book is a love letter to research and researchers, capturing the wonders, struggles, and often humbling but ultimately rewarding experiences with refreshing honesty and insight.

Chris and Runshan guide the reader through the hurdles, contradictions, and occasional absurdities that researchers face. They spotlight the ironies, while remaining determinedly gracious. This authenticity permeates every page of the book.

The book invites an interactive joyride for all researchers, new and seasoned. Consider the six types of researchers in Chapter 4. You cannot help but see yourself in one or more of these personas: Are you a Promoter, a Discoverer, or perhaps an Artisan? What is your attraction to research—its novelty, usefulness, surprise, or sheer difficulty? Is your research topic-driven or relationship-driven? This exploration is as engaging as a magnetic horoscope column, but with a dose of empiricism and insight.

Reading the book gives the reader a fresh lens through which to re-examine ordinary events: One's favorite paper is never widely read or cited; a management researcher promotes "managerial insights"—whatever it means—among colleagues who are in no position to influence business decisions; the sense of "exhilaration and despair" people feel at large academic gatherings.

Paths to Research brings order to the chaotic beauty of research. Like Chen Congzhou's treatise on Chinese gardens, it finds tranquility within winding paths, lighting the way through twists and turns. In an age where anyone with an Internet connection can don the hat of a researcher, this essential companion offers a pragmatic, compassionate perspective without formulaic answers.

At its core, *Paths to Research* is an essential companion for anyone embarking on the arduous journey of research. It offers a pragmatic, compassionate, and perhaps somewhat flattering view of the career, reminding us that we can find fulfillment and joy in research without losing ourselves. It embraces the seemingly stochastic paths we take, much like a winding route in a Chinese garden, where moments of serendipitous beauty often come from getting lost and looking at commonplace things from a different *leaking window* (漏窗)—the ancient Chinese version of today's Instagram filters.

* * *

On a personal note, I feel fortunate to have developed a friendship with Chris through research. We discovered each other—no kidding—through Google Scholar in 2017, when he was on the University of Chicago faculty. We have had an endless conversation about research (among other things) ever since.

I have come to regard Chris as one of the most prolific, insightful, and versatile researchers in the field of operations research and management science. What sets Chris apart is his ability to seamlessly traverse a spectrum of research areas ranging from optimization to economics, operations management, and even qualitative research. His approach to research keeps him at the forefront of the field, constantly pushing boundaries and challenging what's taken for granted—in the gentlest and most humble way possible.

A star researcher in his own right, Chris' commitment to nurturing the next generation of leaders is evident in his tireless efforts as a mentor and educator. He has traveled the globe sharing his wisdom with students and col-

leagues. As a friend and collaborator of mine, Chris has made me a better researcher. I look forward to seeing more people benefit from Chris as the wisdom he and Runshan spread in *Paths to Research* enlightens all who do (or aspire to do) research for a living.

* * *

Paths to Research is one of those books that is worth reading and re-reading. Each time I read it, I was deeply moved by its respect for the reader. It does not impose a grand scheme or a set of immutable laws on the reader. It inspires the reader to explore and decide what's best for themselves. Even when it offers practical tips, it places the reader where the story is and sets the stage for self-discovery. This is a book written for autonomous beings, and it seeks to enhance that sense of autonomy.

What *Paths to Research* offers is a joyous dialogue, walking alongside Chris and Runshan as they illuminate the twists and turns of research. The book prepares us for the challenges of research while revealing hidden pockets of joy.

Wiśława Szymborska, my favorite poet, wrote:

Nothing can ever happen twice.
In consequence, the sorry fact is
that we arrive here improvised
and leave without the chance to practice.

I invite you to join me on this adventure with Chris and Runshan, and to stop and practice the wisdom you glean as often as you need to. It is a journey reminiscent of the allure of a Chinese garden—inviting contemplation, enlightenment, and a treasure trove of revelations that will forever change what research means to all of us.

Tinglong Dai, Johns Hopkins University, Baltimore, MD, U.S.A.
July 2023

Preface

In our years involved with research, we've had discussions with many colleagues, students, and wisened researchers alike. One sentiment is constant: Each researcher has their own understanding of what research is and how to conduct it. A reasonable opinion is that one's research philosophy, motivation, and approach are so personal that the topic does not lend itself to generalization.

But we have also witnessed serious struggles among young researchers whose work is hindered by ill-conceived approaches to the research process. We have witnessed Ph.D. students become depressed and disillusioned and sometimes even give up trying to understand how to navigate the process. This book is aimed to help stave off such disillusionment. We believe there is indeed a "collective" wisdom that bubbles up from various research experiences. The approach of this book is to collect some of that wisdom in the hope that it may benefit our readers.

The disillusionment experienced by young researchers has numerous causes. One that immediately comes to mind is misconceptions about the true nature of research. A researcher's work is poorly understood culturally, being inadequately captured in popular media and the collective consciousness. We have great difficulty as academic researchers explaining what it is that we do to those outside of academia. Try to explain what a professor does to a person on the street, and they may walk away bewildered. We believe that part of this misunderstanding comes from the academic community inadvertently misrepresenting—even to themselves—the reality of conducting research in the context of a career. This book explores a variety of perspectives on what research is and how different perspectives motivate and establish the identity of researchers.

A second cause of disillusionment is an overly narrow view of the path to

success. In our interactions with students, there is a near-universal feeling of inadequacy: everyone feels they are not smart enough, not creative enough, not enough of a visionary, or not sufficiently connected to a powerful mentor or group of influential scholars to succeed. This book aims to elucidate many alternative “paths to research” to broaden the reader’s perspectives on how one can chart a course of success. This broad view welcomes a wide variety of skills, interests, and dispositions as being viable for a research career.

A third cause of disillusionment is inadequate training in the research process itself. We do not want to suggest that there is inadequate training in research techniques. Most Ph.D. programs do an excellent job equipping students with the *skills* and *knowledge* necessary for success in a research career. However, we believe that skills and knowledge alone are insufficient. Equally, or maybe even more important, are attitudes and qualities that allow students to *use* their skills and knowledge even in the face of failure, rejection, and ridicule. The belief that your ideas are worthy of experimentation is as important as knowing how to conduct an experiment. As important as knowing how to construct a proof of a theorem are the qualities of patience, persistence, and empathy that are needed to produce proofs that can inspire and instruct those who read them.

This book hopes to honor the great variety of successful approaches to research while attempting to describe them with sufficient coherence to allow the reader to chart their own path. We (Chris and Runshan) are continuously adjusting our view of research as our lives and circumstances change. Our purpose is not to elevate one perspective as the most “pure” or most “legitimate” nor to provide a recipe for the “best” way to succeed in research (we doubt such a thing exists). This book aims to describe a myriad of “paths to research” each inviting exploration.

One concrete example of this is that we (Chris and Runshan) have different ideas, experiences, and ideals when doing research. There is much we agree on, but there is also much we have diverging opinions about. This is represented in the book itself. There are two “I”s in this text, which we will preface either as “I (Chris)” or “I (Runshan)”. These two “I”s bring up different points and occasionally disagree with each other. We consider this a feature, not a drawback of the book. We are hopeful “I (reader)” gets regularly inserted in the margins to make even more points as readers go through the book. We would love to get these margin notes in emails to possibly include in later versions of the book!

We hope this book helps young researchers avoid the very real and debilitating lack of confidence that shakes many of us. There are times when it can be a struggle to do *any* work, let alone “great” work. We hope the variety of perspectives we present here may help young researchers to become “people of action under all conditions” even coming off the sting of a rejected paper, the frustration of a failed experiment, or waiting for that job interview email that never arrives.

Of course, one outcome is that someone realizes that research is not for them. If this realization is based on an accurate view of what it takes to succeed in research as a career, we consider this a positive outcome. We hope to avoid someone entering the profession under false pretenses and only later realizing their incompatibility with research as a career.

We are not interested in presenting an academic treatise on the nature of the research process in the context of an academic career (we would be fans of anyone that earnestly attempts this daunting task). We are even less interested in discussing the notion of research, let alone science, at a societal or deep philosophical level. The subtitle of this book—“A companion to research as a career”—belies its pragmatic nature. We do not speak much about the importance of research and science in advancing our society. We do not enter the deep waters of the philosophy of science. When we do tip our toes, we tend towards the shallow end of the Latourian side of the debate, emphasizing a practical and social bent on the process of science.

This work is also not the result of a lengthy dive into the education literature on academic success nor the result of extensive empirical investigation. This work is the result of reflecting on our personal experiences and conversations with other researchers. It reflects our stories and our earnest search for truth. We hope every reader will find a few things they relate to and a few things that challenge their current thinking. We are more than happy if you read and disagree with what we wrote. We would love to get an email about it. This book only attempts to take up the humble task of “guiding” the traveler to deeper investigation, not to provide enduring answers. Often in research, as in life, thought-provoking questions can be as meaningful as answers.

We readily admit that the base of our experiences in doing research is in the context of academic business and public policy schools. Some of the specifics of our thoughts best fit this setting, but we believe that our deliberations here can benefit students and young researchers outside of these contexts. We have some degree of confidence that these ideas apply reasonably well to students

of economics and mathematics, and to a lesser extent, computer science and engineering. We are less confident that they apply to students of the physical sciences, the humanities, and other social sciences, simply because our experience does not extend very far into these domains. Of course, we are eager for feedback from readers in these fields to help us improve on these shortcomings.

Novel features

At the end of each chapter are two sections called “Putting it all together” and “Reflection questions”. Each “Putting it all together” contains a fictional story of a young researcher navigating some of the issues described in the chapter. The purpose of these narratives is to help the reader relate to the material in the chapter in some grounded way. The inclusion of narratives also adds an element of humanity that might otherwise be lacking.

The “Reflection questions” sections include a brief set of prompts meant for group discussion on the topic of the chapter. Versions of some of these questions have appeared in workshops we have offered at various institutions over the past several years (these are acknowledged later in this preface). These reflection questions are best used by groups of peers who are reading the book together. As authors, we would be delighted to hear the fruits of your reflections. Please do not hesitate to contact us with any stories or insights.

Organization of the book

The paper is written in a somewhat modular fashion. We hope the reader finds the book sufficiently interesting to read from cover to cover, but we are also aware that it may serve as a useful reference for those navigating their research career. We believe **Parts I** and **II** are necessary to understand the chapters in **Part III**, particularly **Chapter 5**, which defines the concept of capability that structures the chapters in **Part III**. However, the chapters of **Part II** can be read in any order; they are essentially self-contained. **Part III** is explicitly designed to be modular, and the reader can read any or all of the chapters they find interesting in any order they wish.

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This work is only possible with the support of our friends, colleagues, and family. The following captures a (woefully) incomplete list.

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Of course, the help and support of my collaborators on this project—Runshan and Zhenyu—were pivotal in getting this work to the stage you find now. Runshan and Zhenyu have worked with me on understanding the nature of research since my first workshop on the topic of research at the Shanghai University of Finance and Economics, all through their graduate studies and now as young assistant professor years. I am delighted I have had the opportunity to see the world through their eyes.

But my thanks also extend back to much earlier times in my life. I am es-

pecially indebted to my father, Bob Ryan, and brother, Paul Ryan. My father started me on a path of inquiry at a very young age that continues to this day. He has always encouraged me to think “at the edges” of the ideas I am presented with, to question and expand, but also never to be afraid to believe. I hope this book is an apt testament to his tutelage. My brother fiercely embraced the truth at all costs. He taught me that a truly meaningful life is one that faces struggles with courage and wonder. I channeled some of the boldness and defiance of his search for truth in writing this book.

Last but not least, none of this would be possible without the loving support of my wife (Cecilia Wang) and the inspiration of my children (Leo and Max Ryan). My wife is my strongest advocate—I truly hope I am worthy of her unwavering belief in me. This book may be seen as a small token of thanks to her. It is the love of my family that is the surest source of energy that moves my fingers across this keyboard.

I (Runshan) would like to first thank Chris for his continuing support and guidance during my pursuit of an academic career, as well as the opportunity of participating in this project. I also want to thank Tom Lam for his unwavering trust in me and unreserved support at all times. I met Chris and Tom when I was completely clueless about what research entailed. They have shown ultimate patience in guiding me through the journey and pushing me to think and reflect from time to time. I am particularly thankful for their encouragement to find truth in the world and in myself. Discussions with them have shaped my views and beliefs toward research, which eventually led to many of the ideas in this book.

My deep appreciation goes to my Ph.D. advisors: Yan Huang, Nitin Mehta, Param Vir Singh, and Kannan Srinivasan. They are the ones who have taught me how to approach research and responded to my endless questions through countless discussions on a day-to-day basis. They all have great enthusiasm for research, relentless pursuit of excellence, and genuine care for students. I am deeply indebted to them for my growth. A special thanks goes to Ramayya Krishnan, who encouraged me to bravely extend the boundary of knowledge and offered me many valuable perspectives.

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Last but not least, I am deeply grateful to my parents (Aiguo Fu and Cuie Wang) and my husband (Yangfan Liang). They are my biggest supporters. My parents dedicate themselves to providing me the freedom to pursue whatever I love. They respect my ideas and opinions even when I disagree with them, and they help preserve and foster my curiosity about the world. My husband shares all my joys and sorrows and provides unmatched comprehension and support. He also patiently discussed (and sometimes debated) with me on most of the topics covered in this book.

Finally, we would collectively like to thank our colleagues who read early versions of this manuscript and provided early comments. This list includes Julia Yan, Yannis Stamatopoulos, Nanxi Zhang, Nicole DeHoratius, and Samantha Kepplar.

Chris Ryan, Vancouver, BC, Canada
Runshan Fu, New York, NY, U.S.A.

Part I

Introduction

Chapter 1

Paths to research

The very notion of a path ...beckons to new horizons, it demands effort and movement, it accommodates different paces and strides, it is structured and defined. A path can be experienced and known, not only by one or two but by scores upon scores, it belongs to the community. To walk a path is equally expressive. It requires of the individual volition and choice; it calls for a set of skills and abilities but also elicits certain qualities and attitudes; it necessitates a logical progression but admits, when needed, related lines of exploration; it may seem easy at the outset but becomes more challenging further along. And crucially, one walks the path in the company of others.

– The Universal House of Justice¹

Academic research is a long, often confusing, journey. Throughout this book, we will introduce many analogies to research, but we will start with our favorite. Research can be likened to journeying along a path to the top of a densely forested and rocky mountain. It is easy to get lost and lose touch with the destination. It is easy to meet a dead-end ravine or cliff. For those who first journey on the mountain, the way forward is treacherous, even life-threatening.

Luckily, few of you will ever scale completely new mountains in your lives as researchers. There are a few worn paths, a few flags tied to trees left as a warning, and a few hand-drawn maps that give you some sense of direction. You may even have a compass to help you from getting completely turned around. The journey does not become entirely safe or predictable, but it stops

¹From a message dated 12 December 2011, available on <https://www.bahai.org/library/>.



Figure 1.1: Paths to research

you from making too many of the same mistakes others have made. You can get farther because the path has been partially blazed for you to follow.

This is not an unfamiliar analogy for many researchers. The oft-repeated saying, “We are standing on the shoulders of giants,” says that your journey never starts from scratch; it builds on those who came before you. But, too often, we think of these “shoulders” mainly in terms of previous discoveries, foundational methods, or established tools. We can forget that these shoulders belong to people with stories of emotion, belief, fear, and aspiration. Whereas many of the important discoveries, methods, and tools are a part of public record—captured in papers and textbooks—the stories of emotion, belief, fear, and aspiration are often lost to history. If you are lucky, you have a mentor who can assist you with this “less public” part of the research journey. However, we have witnessed far too many young researchers become lost and disillusioned with nowhere to turn. An incomplete view of the history of research represents it as an unbroken chain of genius and success that seems to have little place for uncertainty, fear, and shaken confidence. In other words, little place for you.

What we believe is called for is a more nuanced description of the paths you can follow in your research journey. Of course, nothing can replace an

excellent mentor who accompanies you until you are fully established on your own. This book attempts to be a complementary source of inspiration and guidance. It attempts to gather some of the “collective wisdom” of researchers the authors have had the opportunity to converse with over the years and, of course, our own personal journeys.

So let’s dwell a bit more here on how the analogy of the mountain path can inform your understanding of research. In the process, we point ahead to many of the chapters in the rest of the book. This serves as a kind of conceptual “outline” for what follows.

Your path to the top of a mountain challenges you to venture into uncharted territory. Newness is central to academic research, as we will see in **Chapter 3**. There is always the promise that a student will eclipse their teacher and discover something about the mountain no one thought to even ask about before. Research is not an endeavor of being content with the known; it is an undertaking that sees the sun made new at every sunrise.

The search for the “new” demands effort and movement. Even strolling down a familiar forested trail while accepting the established signposts requires *some* energy. You have to get out of your chair and put on your boots. How much more is the effort needed to walk a lesser-known path, or worse yet, blaze a brand new path through a forest shrouded in mystery? And where you “blaze”, you might get burnt. Sometimes the forest is so dark that you must fumble on your hands and knees to maintain your bearings. Research is humbling. The task of research continually requires you to expend your energy in questioning and doubting, yet you must remain positive and hopeful.

You can’t reach the peak of a mountain by walking downhill. There is a logic to the journey and milestones that need to be achieved. One foot must go in front of the other. **Chapter 2** presents our view of the fundamental requirement for success in an academic research career: acceptance by the academic research community. This perspective serves as a general trajectory that the remaining chapters build on.

Luckily, not everyone needs to ascend the path at the same pace and in the same manner. One of our major themes is that there are many ways to approach research and many paths to research that you can follow. This is detailed at length in **Chapter 4**. A hike up a mountain is not a race. There is no “standard” research experience, just like—by definition—there is no “standard” adventure. One of the joys of hiking with a group of friends is understanding how different people see the same thing differently. You can view the

hike as exercise, a spiritual encounter, or a lesson in nature's intricate order. Some hikers are so engaged in conversation with each other that the mountain is only a backdrop. Of course, you may have a complex and unresolved mix of perspectives.

This book does not advocate the idea that research is the domain of an elite few. The "best" advice for a career as a research elite is to be the best student in your high school, excel at a famous undergraduate institution, get accepted to a top Ph.D. program under the tutelage of a renowned professor with many connections who can anticipate or initiate research trends, and follow these trends on your path to a research career at another leading research institution. Simple. But this approach works for the very few, even for those so lucky to find themselves on this "gilded" path. This book is for everyone who has struggled or will struggle (which, in the end, is probably everyone). Our goal is to delineate a variety of alternative approaches that you can use to reach your destination, regardless of your starting point.

And yet, research is not a path for those who want to be told what to do. There is some structure, but there is undoubtedly no blueprint. Research involves the exercise of many (sometimes seemingly unrelated) skills. Many wonderful texts emphasize some of the skills and abilities needed to find success (we provide lists of recommended readings in our chapter notes). In conversations among researchers, much attention is paid to the "raw intelligence", "insight", and "vision" necessary to succeed. Emphasis on these "rare" skills makes the path of research seem less attainable. It seems to suggest that "average" people cannot succeed. We eschew these perspectives. We focus on skills we believe can be learned and made accessible to many. **Part III** of the book is dedicated to exploring these skills.

But even well-polished skills are no guarantee of success. Unfortunately, success in the research journey can sometimes breed a desire to emphasize the individual talents and unique characteristics of those who succeed. Often underappreciated are the qualities of character and attitudes that lead to success. A hiker can have the best boots, maps, and expert guides and still fail to progress up the mountain. If she loses hope or lacks patience, no amount of talent or preparedness can make up for it. **Chapter 5** explores the importance of qualities and attitudes in academic research success, a framework we use to breathe life into what might otherwise be a shallow collection of "tips" in **Part III**.

But arguably most important of all, no one needs to walk the path alone.

You walk the path in the company of others. Most advice we have seen about the research process is directed towards individuals: “This is what *you* should do or consider”. But in reality, much of academic research is done by groups of people and not individuals. Just like a journey up a dark mountain is dangerous to undertake alone, the same is true of a journey into research. The story of the lone genius who succeeds based on raw talent and boundless energy is largely a myth. Most of us need encouragement, inspiration, and help. Each chapter in the remainder of the book ends with a story of young researchers learning to succeed with the help of others. We hope this may serve as an example to you. If you don’t have a cohort of people there to support you on your academic journey, we feel getting one is your first order of business. We discuss some more of the dynamics of collaboration and support in research in [Chapter 11](#).

* * *

We sincerely hope that you succeed in your research journey, taking the path in front of you to your desired destination. But we hope for more than this. I (Chris) used to walk to school atop a hill banking a busy road. I was not the only one. There was a neat little path traversed by scores of children making the journey to and from school. Who did the path belong to? Was it mine because I walked it? Certainly no. Does it even belong to the first person who ever walked it? Hardly. The path belonged to the community; any one person does not own it.

The path you walk to success is not entirely your own. It is owned by the community of researchers—past, present, and future. Individual success is a great accomplishment but is, in a sense, not surprising. Success is an outcome of a process that has already worked for at least a few of us. We are interested in more. We are interested in elevating communities of young researchers who succeed together. In particular, we hope those who benefit from this book will turn around to help the next generation. Some of the students we talked to about these topics while preparing this book have already started to help the next generation. We see that they are not content with their own victories. Together, we can enrich the community of researchers as a whole.

Chapter 2

The process of acceptance

The analogy of the path in the previous chapter shows the broad scope of the research endeavor. The purpose was to welcome new travelers and invite new vistas. This chapter presents a practical narrowing of this overall vision. Here we start our deliberation of possible destinations of the research journey. In particular, we make one key acknowledgment. Anyone can discover amazing truths in a journey up a forested mountain. It is something else entirely to get paid to take the journey. The latter means finding others who are interested (and willing to pay) to hear of your discoveries.

2.1 An analogy to art

We find it apt to introduce another analogy to research,¹ that of artistic expression. Artistic expression is a common enough experience to us all that it can illustrate some of the challenges and tradeoffs of pursuing a career in research. The analogy can also bring you back to an earlier time in your life when art (and arguably research) were more of an everyday experience.

Art as a human activity

Have you ever wondered why we teach children art? Is it because we want to find that “diamond in the rough” who becomes the next famous artist? Is it

¹This book contains numerous overlapping analogies to research. We ask you to accept mixed metaphors on occasion.

because drawing helps children visualize objects that translate into a career as a scientist or engineer? Or is it simply that artistic expression is part of what it means to be human? Art is a universal human expression. Some of the earliest demonstrations of humanity are cave drawings. Art is tied to culture and identity.

Should we leave art “to the professionals”? Hardly. Of all the art hanging on walls in the world, it would not be unreasonable to guess that most of done by children, not professionals. For a parent, their child’s art captures something of their experience; it is a piece of that person. That is worthy of celebration.

Art as a career

And yet we must acknowledge that pursuing a career in art is different than producing art for a parent to hang. To pursue art as a career, someone must pay you for your work. In the long run, that “someone” needs to be a stranger, not a family member or friend. No matter how much the artist pours into it, art is only a basis of a career if it leads to a sale.

This does not mean that all professional arts are “sellouts” nor that professional art that earns a livelihood does not express the artist’s humanity. One would have to be a cynic not to acknowledge the human accomplishment behind the world’s most famous (and expensive) paintings. An honest observer must also accept (at least to some degree) the sincere artistry behind enduring product logos like the Nike “swoosh” or McDonald’s “golden arches”. It’s simply that human expression is not the only consideration when it comes to art as a career.

Having a career in art is notoriously difficult. The image of the “starving artist” is an enduring cultural warning for a reason. Most self-respecting programs in fine art include courses on having a career as a working artist. The ability to sell art to others can sometimes be as breathtaking as the ability to produce it.

What is “good” art?

Is the work of a professional artist is “better” than the work of an amateur? From a certain perspective, the answer is “no”. A poem written for you by a loved one could be more valuable to you than all of Shakespeare’s sonnets.

But from the perspective of art as a career, “better” must at least partially be measured in dollar amounts. A professional’s art is “better” because it provides the artist with a profession!

So how do we decide what is “good” art? From the perspective of art as a human endeavor, “good” art captures the emotions or sincere thoughts of the artist and cheers or inspires those who witness it. From the perspective of art as a career, “good” art earns a livelihood for the artist. Of course “good” art from the career perspective is likely to be “good” art from the perspective of art as a human endeavor. People who purchase art also seek sincere thought and inspiration. The point is that sincerity and inspiration are not *sufficient*.

Research as a human endeavor

What does all of this have to do with research? Like art, we teach children to do research from a young age. Children search for answers in library books (pretending not to use Wikipedia), run classroom experiments, and make observations about the world in journals. Is this done to identify those children with talent in research who might be an academic one day? Certainly no. Like art, research is a human endeavor, a universal human experience. Those around children are familiar with their ferocity in making sense of their world. The cliché of a child constantly asking and re-asking “Why?” to their parents is a cliché for a reason. What is a more pure expression of the research spirit than this?

The confluence of curiosity and awe found in childhood is hard to recapture for grown-ups. Not unlike artistic expression, enthusiasm for research and discovery often diminishes with age. And yet, research remains a constant in our lives even as we age.

So what do we exactly mean by “research”? For now, let’s think of research as raising questions and finding (maybe partial) answers to those questions that require some “digging”. This “digging” requires examining the world, learning about what others have said, and examining your own thoughts.

Every human being engages in research under this definition. Each person faces many challenging circumstances and must make important life decisions that require some degree of “research”. Take, for example, the decision of who (or if) to marry or what home to buy. All would agree that careful deliberation and investigation is the wise path here. Research of some form is required.

Research as a career

But despite research being a universal human experience, pursuing a career in research is a different story. As a career, someone must pay you to “dig”. Professional researchers have found a way to turn their process of learning, investigation, and discovery into a paying gig. Professional researchers have found someone sufficiently interested in their discoveries to pay for the privilege of being associated with their research outcomes.

This book is about becoming a professional researcher. There is more than one way to be a professional researcher—for instance, working for a government agency or an R&D department at a company—but our primary focus is on research careers in academia in the context of universities (or otherwise similar research institutes). This is not to say that some of the ideas we present here are not valuable for nonacademic professional researchers; it is more that we are more or less ignorant of their circumstances. In the spirit of sticking to what we (somewhat) know, we will speak from experience in the academic research context.

What is “good” research?

From the perspective of research as a human endeavor, “good” research finds (at least partial) answers to questions that are important to the researcher. Consider the intractable problem of friendship. Few can argue that having a true friend is not among the rarest and most valuable things you can experience. This leads to the pivotal research question: “How do you form a true friendship?”² Regardless of how you understand the question, few can dispute that essentially everyone in the world has experimented and generated some knowledge to answer it. “What is the purpose and value of mathematics?” is another question almost universally pondered from experts to elementary school children.

From the perspective of research as a career, “good” research earns a livelihood for the researcher. It is really that simple. In the case of academic research, the research done by the researcher must be of sufficient interest to employers at universities or institutes to be hired, promoted, and tenured. This focus on career does not mean that professional researchers do not an-

²Of course, this raises its own questions like how to define a “true friendship,” but we digress.

swer questions that they find important. It simply means that answering important questions is not *sufficient*. The professional researcher needs to find a way to be paid; otherwise, she is not a professional.³

Audience

The key distinction between research (or art) as a human endeavor and research (or art) as a career is one of audience. The audience of research as a human endeavor can be small. Suppose you learn a method for making new and deep friendships. You may simply relish in this valuable knowledge yourself: an audience of one. You may try demonstrating and explaining these methods to your other close friends and family. This is an admittedly larger, but still relatively small, audience. The leap comes in moving from an audience of friends and family to an audience of *strangers*, particularly strangers who can support you financially. The audience for “professional” art is, for example, companies looking for graphic designers, wealthy art collectors looking to beautify their homes, teenagers eager to download and listen to the latest album, etc. But what is the audience for “professional” research? Who is doing the “buying”?

The peer review system

To understand the nature of audience in academic research, you need to understand the academic employment system. Academic employment has roughly three components: (i) research, (ii) teaching, and (iii) service to the university and academic community. Academic institutions differ in how much weight they put on these three components. At essentially all of the famous universities an average person on the street will have heard of, research is weighed most heavily. Since this book is about the academic research process, it is also our focus.

³By “professional” we mean someone who is paid for what they do. A non-professional is one who does something, say research, for free. It’s like the difference between an amateur athlete and a professional athlete. It does not speak to the quality or importance of the work. Someone can do research that is indistinguishable in quality or impact from someone paid to do research (or of even higher quality or impact) when not being paid to do so. The point here is that such a researcher needs to sustain themselves from efforts other than research. This book targets those who aim to sustain themselves by being paid to do research.

How do you distinguish yourself in terms of research to get hired, promoted, and tenured at an academic institution? How is research evaluated among academics? There may be several barometers, but the two primary ones are most often (i) the publication of papers⁴ in reputable outlets, and (ii) the securing of research funds, typically from government agencies or companies. The relative importance of publication and funding varies by academic discipline. Still, there is an implicit “hierarchy” of sorts in that research funding is often contingent on publishing high-quality papers. For this reason, our primary focus is still on publishing papers. In other words, we view *published papers* as the primary “currency” of an academic research career since published papers provide the most direct translation into job offers, promotions, raises, and tenure.

As a brief aside, it is worth stressing that this emphasis on papers is well-justified. Papers raise important questions, share ideas, explain findings, etc. Papers are one of humanity’s main tools to build and formalize human knowledge. Publishing a paper is an admirable achievement worthy of celebration. Writing papers to build a career is only a part of the story—it just so happens to be the part of the story we are emphasizing in this chapter.

So, how to get your paper published? First, you need to do the research, then you need to write it up,⁵ and finally, you need to submit the paper to an outlet (typically a journal or conference). At an outlet, there are editors and reviewers who evaluate the paper. In the vast majority of cases, these editors and reviewers are *other* researchers. This is the system of *peer review*. Who better to evaluate the quality of research than other researchers who are intimately familiar with the process?

This book will not weigh in on the relative strengths and weaknesses of the peer review system. We have no real reason to quibble with it: Peer review is a long-standing system that has overseen the publishing of many of humanity’s most significant discoveries. But we think it is important to stress that the peer review system for allocating “currency” is, in meaningful ways, different from how professional artists are paid. To at least some extent (and often to a large extent), the work of professional artists is judged by nonprofessionals. A musician can have “critical acclaim” but still be a flop with the purchasing

⁴We acknowledge that publishing books plays an important role in some academic fields, but we must admit to knowing less about these fields. We stick to papers in the spirit of sticking with what we know.

⁵More on the first two steps in **Part III** of the book.

public. Conversely, artists can be reviled by other artists and enjoy a massive following (and proportional wealth). The “artist’s artist” is not guaranteed a successful career. But when it comes to research, there is typically no “public” out there directly buying papers. The “researcher’s researcher” — by the construction of the peer review system — is nearly guaranteed success.

In conclusion, the main audience of academic research is the community of academic researchers. Accordingly, success in academic research as a career is to be accepted by the academic research community. This acceptance comes in the form of published journal papers and the spoils that come along with that: academic positions, promotions, raises, research funding, and tenure. By contrast, an unsuccessful research career is one where the community does not accept the researcher, who eventually becomes isolated. Isolation signals the end of a career in research, no matter how profound the researcher’s discoveries. Writing about the scientific process, Bruno Latour describes something similar in his seminal *Science in Action*:

Fact construction is so much a collective process that an isolated person builds only dreams, claims, and feelings, not facts one of the main problems to solve is to interest someone enough to be read at all; compared to this problem, that of being believed is, so to speak, a minor task.

“Pursuing your passion”

We end this section with a short diversion. The most potent cliché in career advice —academic or otherwise—is the adage to “pursue your passion”. Of course, we are not the ones to dissuade someone from pursuing a driving passion. Having a passion for your work is a blessing because it can provide you with much energy and optimism. Striving to find passion in your work should always be pursued and applauded. But we recommend caution in accepting the facile formulation that pursuing passion is sufficient, or even necessary, for success in an academic research career.

Indeed, many career paths do not seem to require much in the way of “passion” to succeed. Consider the job a long-distance truck driver. Do you need to have a passion for trucking to have success in a career as a truck driver? Certainly not. But does this mean truckers are all disaffected and going through their work aimlessly? According to the classic ethnography *Pedal to the Metal*

by Ouellet [17] on the workplace culture of truckers, this is also not the case. Many truckers are driven by a sense of duty, pride, and the opportunity to provide for themselves and their families. Maybe relatively few children dream of being a trucker, but it is valuable and proud work nonetheless.

When it comes to careers in research, the adage to “pursue your passion” or “pursue your interests” seems to have a strong pull. I (Chris) have heard some suggest that academia is not a place for someone without a passion for research. I always found that a little awkward because I am not sure I myself have a strong passion for research. I do research and am happy to do it. But a passion? Hard to say. I really do have a passion for teaching university students. Research is “part of the package” of the job that allows me to pursue this passion. I wonder why a researcher’s work is really any different than that of a truck driver.

2.2 The process of acceptance

In the remainder of this chapter, we provide a framework for thinking about the process of being accepted into the academic research community, which, as we described in the previous section, is the key barometer of success in an academic research career.

To start our deliberations on the process of acceptance, you may ponder for a while about a time you were accepted into a community. This could be a sports team or a group of friends. Why were you accepted? Do the most talented players get selected for sports teams? Not necessarily; there is always the factor of getting along well with teammates. Can a group of friends accept someone highly distracted by their own interests and passions? We are not optimistic.

Acceptance often involves some form of reciprocity. In the process of acceptance into a community, you must pay attention to the interests and needs of that community, not just your own. The adage of “pursuing your interest” must be tempered by an awareness of the community you aim to be accepted into and what are its interests and priorities. This is another way of saying that *audience matters* in academic research.

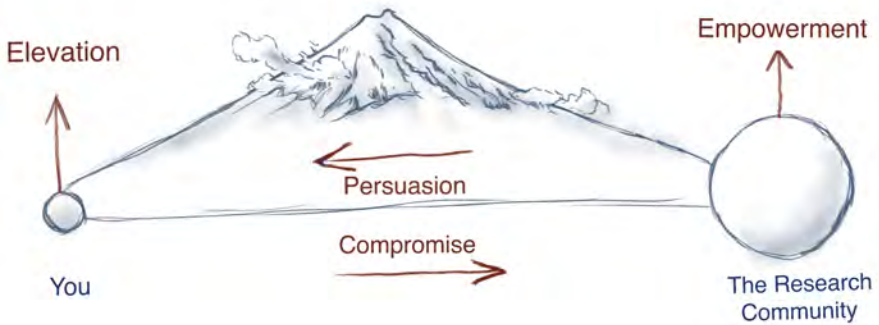


Figure 2.1: The process of acceptance.

A model for acceptance

We hope this discussion has led you to an awareness that success in research as a career involves understanding the interests and priorities of the research community you hope accepts you. We visually present this process of acceptance in [Figure 2.1](#). We will describe its features throughout this section. Let's start with the small circle on the left of the figure representing "you", the aspiring researcher. The larger circle on the right represents the community of researchers. Acceptance occurs when the "you" circle gets incorporated into the larger community circle.

Now, the starting point always has "you" at some distance from the community. No one is *born* into the research community; there is always some distance to be traversed. However, you must acknowledge that not everyone has an equal starting point. In this respect, "academic lineage" is critical. The more related you are to the gatekeepers of the academic community (hiring committees, prominent professors, journal editors, reviewers, etc.) in terms of common undergraduate institutions, graduate institutions, overlapping sets of collaborators, or research advisors, the closer your small circle is to the large circle.

This is not to suggest a conspiracy of academic nepotism is at play. It is simply to suggest that the closer your ties are to gatekeepers of the community, the more likely you are to have common interests, common perspectives, a common language, and common values. All of these smooth the pro-

cess of acceptance. Those with weaker ties may have divergent interests, perspectives, languages, and values. Traveling a great distance is certainly not insurmountable—the academic system strives to be meritocratic. But traversing long distances requires effort, diligence, and a sober awareness of the potential enormity of the task.

The four forces in the process of acceptance

Let's return to [Figure 2.1](#) to examine the “forces” moving the small circle towards the larger one. We delineate four forces: (i) persuasion, (ii) compromise, (iii) elevation, and (iv) empowerment. Persuasion and compromise are forces that draw the small circle and larger circle towards each other. Elevation and empowerment are forces that draw the small and large circles upwards to the apex of the mountain. As the small or large circles ascend to the peak, they become closer to one another. At the highest level of elevation and empowerment (i.e., at the peak), the horizontal distance between the individual and community vanishes. If the small and large circles ascend partially, the distance between them (to be traversed by persuasion and compromise) is shortened. Let's examine the four forces in turn.

Persuasion

Persuasion is a force that brings the community closer to “you” by convincing the community that you are worthy of acceptance “as is”. The force is at play when you go on the job market in search of jobs or in the process of convincing reviewers to accept the revision of your paper. Your work is done, your paper is written, and your CV is what it is. However, all of this does not perfectly speak for itself; you must make a persuasive case for acceptance. We discuss the persuasive force in much more detail in [Part III](#).

Compromise

Compromise acts in the opposite direction as persuasion. Compromise is when “you” move closer to the community through concession or adjustment of expectations, perspectives, or ideals. This force is at play when you adjust your passion for pursuing an exciting topic from the community's perspective, even if you do not have a burning interest in it. You also see compromise when authors change their paper to appease an editor's or reviewer's wishes,

even when they do not believe their paper is improved with the requested changes.

We do not intend to put a strong negative connotation on compromise, as if compromising is a failure of principle or character. Indeed, the ability to compromise is essential to maturity and a key ingredient in any functioning relationship. There can and should, however, be limits to compromise. If you give up too much of yourself to fit into the group, you may face a crisis of identity and purpose.

Elevation

The first two forces have their limitations. You can only be so persuasive, based on where you are “as is”, and you only have so much to “give” in compromise before you lose track of yourself. So what is left?

Of course, you can always improve yourself and your research by strengthening your capabilities.⁶ We call this the process of “elevation”—a movement of the small circle towards the apex of the mountain. Indeed, all else being equal, an individual with greater research capabilities is more easily accepted by the research community. Note also that someone with stronger capabilities may find it easier to compromise without losing their integrity.⁷

It is worth pointing out, however, that we model “elevation” not as a direct (horizontal) movement of the individual to the community, as it acknowledges that self-improvement in research capabilities is not done solely for the goal of acceptance by the community. Indeed, you can engage in self-improvement and maintain little interest in being accepted by others. Also, even as your capabilities become stronger, they do not automatically make you acceptable. Talent alone is often not enough to be accepted, especially for a community protective of its membership.

⁶We carefully define what we mean by a “research capability” in [Chapter 5](#). For now, a rough impression of its meaning suffices.

⁷We concede that it is logically possible for an individual to “descend”, moving farther away from the apex of the mountain. This is possible, for instance, if a researcher loses confidence or becomes depressed. Our focus here is not to belabor discussion of the direction of “descent”, preferring to direct attention to what leads to “elevation”.

Empowerment

Empowerment describes the movement of the community toward the apex of the mountain. Empowerment is a process whereby the community is edified, irrespective of its decision to accept you or not.

We use the word “empowerment” to emphasize the idea that the community becomes “empowered” to gain an appreciation of the work and character of the individual without the need for either persuasion or compromise. One example of this is the evolution of many academic communities to being more sensitive to the historical legacy of discrimination against certain groups of people in academia. This leads to the community being closer to an individual who may have traditionally been impacted by discrimination.⁸

But can an individual really empower the community? We believe they can. For example, you can become involved in efforts to increase diversity in the field in general. This desire to see an increase in diversity need not be driven by self-interest; it can come from a sincere desire to see done what you believe is right.

Other examples of empowerment are more subtle. For instance, you can engage in expository work that raises interest in your research area by elucidating its major contributions and controversies. You need not even mention your research specifically. To some extent, this can be viewed from a lens of persuasion, but it might be cynical to view this entirely in this light. You can involve yourself in this expository work simply out of respect for the historical achievements of those in your research area and a sincere desire to share your enthusiasm with others.

2.3 Putting it all together

As a form of summary, we provide an example of young researchers exploring the ideas in this chapter. The story is fictional but inspired by real-life interactions with many young researchers over the years. The primary purpose is to provide

⁸As with elevation, we want to acknowledge there could be a “negative” direction, something like “disempowerment”. This is where the community becomes alienated from the individual in some way based on forces possibly outside of the control of the individual themselves. A classic example is when a research community moves away from a research topic towards new topics farther from the individual’s expertise. This direction is not our main focus.

you with an example of how natural it can be to discuss your approach to research with those you trust.

As a child of working-class immigrants, Samuel was not the usual candidate to have an academic career. Of the friends in his neighborhood, it was rare to graduate high school, let alone get an undergraduate degree at the local community college, as Samuel did. It took Samuel seven years to finish his undergrad, often working full-time to support his family while juggling coursework. Luckily, Samuel met an enthusiastic young professor—Prof. Green—a year and a half ahead of his graduation. Prof. Green did her Ph.D. at the flagship state university and saw great promise in Samuel’s abilities. She recommended that Samuel pursue a master’s degree and Ph.D. in the department where she graduated, offering her help to write a reference letter and connect Samuel to the right people. Samuel felt both honored and uncertain. To his surprise, he was admitted to a research-oriented master’s program upon graduating with his undergraduate degree.

Not coming from an academic background, Samuel worked hard to elevate his understanding of the academic profession. Luckily, Samuel was a quick study. He soaked up as much wisdom as he could from the faculty in the department, his dissertation committee members, and his fellow students. In a short period, he went from knowing nothing about academic research to feeling confident that it was a career he wanted to pursue. Samuel quickly transferred to the Ph.D. program.

Even though he was learning a lot from others, Samuel continued to hold onto aspects of his upbringing and culture that he worked hard to incorporate into his research vision. Samuel felt most comfortable getting information through verbal communication. In Samuel’s culture, oral traditions played a pivotal role in the sharing of knowledge, so it was natural for Samuel to be drawn to qualitative research methods like semi-structured interviews and participant observation. However, Samuel’s primary research advisor—Dr. Brooks—knew how difficult it would be to get an academic job in her field using these methods. Through repeated conversations over several months, Samuel came to accept Dr. Brooks’s view that research using econometric methods on large-scale data sets was a much-preferred path. Samuel quickly realized on point this methodology point he had to compromise to gain traction in the academic job market.

However, on the question of research direction, Samuel felt he could not

compromise. Coming from a strong religious background, Samuel was adamant that examining religious practice should play a prominent role in his analysis. This was not a popular dimension of emphasis among researchers in his department or the field in general. Dr. Brooks and several of Samuel's committee members were skeptical. Undeterred, Samuel worked hard to find a unique data set on religious practices in his target population. He spent months connecting this data set to more standard social indicators.

Several of Samuel's committee members became increasingly interested in the religious dimension during this period. Samuel did not need to convince them; they started to see the possibilities this new line of inquiry could offer. One committee member even started collecting data on religious practices in another population of interest. Despite this, Dr. Brooks continued to worry that Samuel was squandering too much of his precious time exploring a risky and potentially unpopular direction. Dr. Brooks was nonetheless supportive and worked with Samuel to finish his dissertation work and get ready for the job market.

Through her professional connections, Dr. Brooks brought Samuel's research to the attention of several departments looking to hire assistant professors. It was only after Samuel presented a convincing and polished mock job talk that Dr. Brooks started to be optimistic about Samuel's chances. Although unable to get an assistant professor job on his first try at the market, Samuel secured a postdoc at a well-known school that received a large grant from a donor to study the role of religion in contemporary society.

This example serves to demonstrate all four forces of acceptance. At the outset, Samuel was far from academia. Over time he was able to *elevate* himself to appreciate the possibilities that an academic career had to offer. Along the way, Samuel had to *compromise* on some things and adjust his expectations of others. However, by staying true to himself, he could *empower* others around him to widen their vision of research possibilities. Finally, he was able to use his skills of *persuasion* to gain the admiration of some of his skeptics and land a postdoc position that put him well down the path of academic acceptance. The story also illustrates how luck and circumstances can play a pivotal role in academic success, particularly connections with influential people and happening upon a topic of emerging interest at the right time.

2.4 Reflection questions

The questions below are provided for you to reflect on what you have read in this chapter. If possible, it is beneficial to discuss these questions in a group of your peers.

1. Why are you pursuing (or considering pursuing) a career in research? What other options did you consider?
2. If you did not need to worry about having a financially viable career and could pursue whatever you had a passion for, what would you do? Would it involve research?
3. Describe a personal discovery you made outside of the academic context. How is this discovery meaningful to you?
4. If you did not need to worry about publishing, what research question might you pursue? What methods would you use?
5. Describe a group or community that you have been accepted into. How did it happen?
6. Have you ever been a part of a group that decided to accept a new member? What was the process like?
7. How might your answer to the previous two questions inform your journey of acceptance into the academic research community?
8. In what ways are you persuasive? Provide concrete examples of your ability to persuade others.
9. Describe a time you had to make a compromise. What do you think about that decision now?
10. In what ways, if at all, did you relate to the story of Samuel and Dr. Brooks? What do you take away from the story?

Part II

Conceptual framework

Part II presents a conceptual framework that bridges our understanding of “success” in a research career (i.e., acceptance into the research community) from **Part I** with much more practical considerations in **Part III**. The purpose of the conceptual framework presented in **Part II** is to provide vocabulary and taxonomy to describe, and put into perspective, the practical challenge of developing research capabilities. We view this “training in concept” as an integral part of elevation (using the terminology introduced in **Figure 2.1**). We also believe an understanding of this conceptual framework can help a young researcher more easily understand and interact with the research community—prerequisites for making an active effort to empower.

Having said this, we want to reiterate (as we have in the preface and introductory chapters) that we are not concerned with developing a rigorous theory of research to stand alongside academic discourses on research methodology or the philosophy of science. We are merely seeking insights into what may help young researchers on their paths to success. We ask you to take only from what they read what you feel is valuable to that journey. The goal is not to provide authoritativeness but relatability.

Part II consists of three chapters. **Chapter 3** describes what we call “research aesthetics”: the dimensions of beauty one can come to appreciate in a piece of research. An exploration of these research aesthetics can benefit your journey to develop your “taste” in research and understand the “tastes” of others. **Chapter 4** presents a taxonomy of canonical research orientations that you can make use of to flesh out your vision of your research career. Finally, in **Chapter 5**, we introduce the concept of *capability*—which includes elements of skills, qualities, and attitudes directed towards a clear purpose—that serves as the language to describe the research process in **Part III**.

Chapter 3

Research aesthetics

3.1 What is interesting?

What makes something interesting? Ask yourself: What (besides research) interests me? Typical examples include sports, music, engaging conversation, the lives of celebrities, history, travel, etc. But why are we interested in these things?

I (Chris) enjoy following professional basketball in the National Basketball Association (NBA). Why do I find it interesting? Well, there are many dimensions.

First, like all “live” sports, basketball happens before your eyes. What you are seeing has never happened before and will never happen again. There is only one NBA championship per year, and never once with precisely the same players or scores. I could read about what happened in the games later, but then I wouldn’t have experienced it in its time. When I watch “live”, I play my part in “creating” the game. Who knows, maybe if I cheer loud enough, it will affect the outcome.

Second, basketball is unpredictable. I can participate in watching paint dry “as it happens”, but the outcome is very predictable. The paint dries. But in a game of basketball, I don’t know the result. I may have some ideas or predictions, but my thoughts and predictions have often been wrong.

These last two points are about the experience of watching the game itself. But my interest goes beyond this temporary excitement. Basketball is an apt allegory for many things that I aspire to be better at—believing in myself, having others I can rely on, having others rely on me, feeling accomplishment,

and overcoming adversity. There are universals in the narrative of sport that carry over to the narrative of my life.

This universality of sport also comes in handy in daily life. Knowledge of sports is a powerful icebreaker and leveler when interacting with strangers. “How ’bout those Bulls?” The triumph or failure of a sports team is part of the story of the school or city it represents. The local team is a context for community and connection.

And yet, when it is all said and done, basketball is just putting a ball into a basket. On the back of this simple act, we build teams, identities, narratives, and dreams.

All of this sounds a little overly poetic. If I am being frank with myself, my initial interest in sports comes from a less lofty place—I am interested in sports because my mom was interested in sports. This was something that she and I shared over many years. Her favorite sport was hockey. I like hockey too, but I gravitated towards basketball. But the reasons I like basketball are the same reasons she liked hockey.

What does all of this have to do with research? Part of acceptance by the academic research community is that this community becomes interested in what interests you. It would be best if you tried to learn how to draw others in—your interest alone is insufficient. But having a developed sense of what interests you is entirely necessary. Becoming interested in things is a skill. It is a precursor to drawing others into what interests you.

The good news is that there is an intellectual tradition of examining the principles of what makes something interesting or compelling. This tradition seeks deep connections between truth, beauty, and goodness. Ralph Waldo Emerson writes in his classical essay “Nature” [6]: “Truth, and goodness, and beauty are but different faces of the same all.” How can something true be ugly? How can something good be false?¹

We work here with the assertion that truth (as we imagine being uncovered in the research process) is inextricably linked with beauty. In other words,

¹One might say that some truths are, in fact, ugly. Consider the truth of rape in our society. How can this truth be considered beautiful? What may be even uglier is pretending that such things do not happen. But we think a higher statement, closer to what Emerson intended, is that there is something “false” about rape, even when there is the truth in the fact that it happens. These acts are not the “true” nature of human beings, but some aberration. We leave this debate about the nature of truth, goodness, and beauty here. Our purposes are more superficial than a deep philosophical investigation warrants.

what makes research feel more “true” is intimately connected with what makes research more beautiful.

So what makes something beautiful? We have already delineated a few of the dimensions of beauty in the description of basketball above. More generally, aesthetics is a branch of philosophy that deals with the nature of beauty and taste. The word “aesthetic” refers to a set of principles underlying a particular theory or view of beauty, often in the context of art. We use the term “aesthetic” roughly in this way by delineating six “aesthetics” or principles that we believe are commonly used to describe the beauty of research. We fully admit to not being scholars of aesthetics, nor even lay practitioners. We are simply using this framework to organize our understanding of the nature of beauty in research.

3.2 The six research aesthetics

Our six aesthetics are captured in [Figure 3.1](#). There are no claims that this is an exhaustive list nor that there isn’t some degree of overlap between the six. These “aesthetics” are derived from our personal reflections on experience and from informally inquiring with researchers in several fields by asking them what makes research “good” and “interesting” (more on learning the aesthetics of others in [Section 3.3](#)).

Novelty

Research has novelty if it feels new and lively. Researchers use words like “fresh”, “seminal”, “hot”, or “path-breaking” to describe novel research. In the description of basketball above, the novelty aesthetic correlates most strongly with the appreciation of the “live” nature of sports, being in its time, and being a part of something really “happening”.

In many ways, novelty is the easiest research aesthetic to accept and understand. Research is about the process of discovery—raising questions that don’t have answers and then answering them. Researchers often gain stature in an academic community by “getting in early” on a new direction that takes off. Their papers become highly cited, and if influential enough, their name becomes part of the field’s lexicon.

As far as we can tell, novelty is universally valued in academic research.



Figure 3.1: The six research aesthetics

But we want to raise one alternate perspective on novelty that may further ground the concept and speak to some of its limitations.

First, novelty in research is defined very globally. Observe that if you discover something entirely on your own, it is novel to you. Sadly, if anyone, anywhere, and any time before, made that discovery, it is no longer *globally* novel.

Consider a child learning to ride a bicycle. From the child’s perspective, the freedom of riding a bike is entirely novel and exhilarating. A cynic might remind the child that many people have ridden bikes before, so it is not much of an accomplishment. Pointing this out may greatly (and arguably, unfairly) diminish the child’s joy. There are genuine *personal* discoveries needed to keep the bike from falling, but this is not a *global* discovery of humanity.

One of the more sobering and confusing things you may experience as a researcher is the realization that someone already came up with your “new” idea. This realization can deeply color the natural joy you feel when discovering something on your own. This highlights the difference between research as a human endeavor and research as a career, as described in [Section 2.1](#). In research as a human endeavor, a “local” discovery can be as meaningful, and sometimes even more meaningful, than a “global” discovery. In research as a career, “local” discoveries may be worth very little unless they are made on a

path toward a “global” discovery.

This is not to say that certain important ideas are not “rediscovered” without knowledge of the earlier results. This type of “rediscovery” is prevalent across academic disciplines. It can sometimes take years, or even decades, for fields to realize that there is an earlier expression of their ideas in another literature.

Dimensions of novelty

In what ways can research be novel? Let’s consider briefly five *dimensions* of research and how novelty can appear in each:

- (i) *setting*, referring to the specific subject area of the research
- (ii) *question*, referring to the question the research aims to answer
- (iii) *data*, referring to the empirical evidence used in answering the research question,²
- (iv) *results*, referring to the research findings or conclusions, and
- (v) *method*, referring to tools, techniques, or principles used to conduct the research.

With respect to setting, novelty can arise in starting a new subject area or directing an existing subject area onto a new course. In business research, for example, the emergence of the “sharing economy” was established as a totally new research setting within marketing, finance, and operations, at the beginning of this millennium.

With respect to question, novelty arises in asking something no one has ever asked before. This new question can be in the context of a new setting or a new question in an established setting. In the sharing economy example, you can ask what matching algorithms are more useful in connecting buyers and sellers at scale. A new question about a classical topic might be to ask how a well-studied but data-intensive algorithm performs when data is scarce.

²We acknowledge that not all research involves “data”. There is no assertion here that every piece of research must have all five dimensions.

With respect to data, novelty arises when a new source of information becomes available. This can be through advancements in technology, gaining access to the database of a secretive company, or through observation of a traditionally distrustful community.

With respect to results, novelty arises when the research demonstrates a new finding or makes a novel conclusion. It is important to stress that a new question does not immediately imply that the answer to that question is a new result. In particular, the question can be “superficially” new, easily handled by the answers to “old” questions. It’s even possible to have a “new” answer to an “old” question if previous answers were deficient.

Finally, concerning method, novelty arises when new tools or approaches are developed. There can be novelty in method even when the question and answer are well-known. In mathematics, it is common to see papers that prove old results in new ways, illustrating a new method (and, therefore, a new perspective). Of course, novel methods can give rise to novel questions, and sometimes even novel settings, when the new method opens the door to questions not previously conceivable.

Patterns of novelty

A research paper can be novel along each dimension to differing degrees. However, depending on the research field—indeed, an individual researcher within a given field—different dimensions of novelty may be weighted differently in an overall assessment of “freshness”.

For example, in empirical research driven by observations, the novelty of the data and question can take on significance, sometimes more than the results. Indeed, if a new data set is the first to allow you to ask an interesting question, then the answer to that question is not necessarily the showcase of that work. Similarly, some areas may work with relatively standard methods (possibly borrowed from neighboring fields), and so novelty is sought more in coming up with new results derived using existing tools. Conversely, in the mathematical sciences, novelty in method can be highly emphasized.

All of this is to say that there is no universal “formula” for novelty; it depends on the field and the distinctive tastes of those involved. Two researchers in the same field may disagree on the degree of novelty of the same paper.

Too much novelty?

If novelty is good, is even more novelty better? Not necessarily. Like all research undertaken for the purpose of a career, research must find its audience in the research community. The problem with research that is “too” new is that the potential size of the receptive audience may be small. This is particularly problematic for novelty in setting. A setting that is too new may find itself “between” academic disciplines and so without an audience.

It is not that everyone within an academic discipline is dismissive of things that are too “new”. Editors and reviewers—who ultimately decide on what to include as active research of an academic discipline—take it upon themselves to judge the quality of the papers they handle. One difficulty with something too “new” is that it can be hard to judge its quality. This is especially true of a very new method, where too few practitioners of related methods may be available to evaluate its soundness.

Ironically, a strong emphasis on novelty may have drawbacks on research progress. A high premium on novelty discourages efforts to replicate previous experiments, challenge empirical findings with new and different datasets, or write papers and books that popularize, synthesize, or summarize past research. Lack of replication and verification can mislead researchers to accept facts or perspectives that may later turn out to be unfounded. New students in an academic field can acutely feel the lack of sufficient expository material that explains findings coherently and consistently. You may wade through dozens (or even hundreds) of papers—with inconsistent terminology and notation—before you even get a whiff of the cutting edge.

Usefulness

A piece of research is useful if it can be used to answer an important question or solve an important problem. This definition has some inherent ambiguity because it begs further clarification of what is meant by “important”. If the notion of “important” is weak, then the usefulness aesthetic is very permissive. Indeed, every paper that answers a question can be considered “useful” on its own terms if you allow every research question to be considered important.

So who decides what is important and, therefore, what is useful? We will discuss three rough audiences who may decide what is useful.³ First, for an ap-

³Again, there is no claim that this is an exhaustive list, simply relevant for exploring the

plied area of research, there are the practitioners and policy-makers who deal with the subject “in practice”. For example, there is the academic field of marketing, and then there are working marketers at real organizations. Another example is economists who study macroeconomic trends, and then there are policy-makers and regulators who are responsible for aspects of the economy “in the wild”. From the lens of practitioners, something is useful if it helps to answer a question or solve a problem that is important in their work. In other words, the importance of a question or problem is governed by its practical implications.

The second audience is even more vague, what we call “society at large”. A question is important to this audience if it has broad societal implications, such as improving health or reducing poverty. For example, the discovery and distribution of a vaccine for a virus and the cure for a particular type of cancer. The clearest case of usefulness from the perspective of this audience is when even an average person in society can appreciate the importance of the problem answered in the literature.

The third audience is the academic research community itself. Here, the importance is determined by the collective opinion of the research community in the context of undertaking further research. For example, in many fields (notably in mathematics), well-known “open problems” are agreed upon as being important. If someone were to solve a significant open problem, it would represent a breakthrough in thinking that could be used to solve other related problems. In a very concrete sense, solutions to these open problems are important because they would be useful in the “business” of writing more papers. Another example may be “paradoxes” in existing theory, the resolution of which might open the door to new areas of research or projects of clarification for existing theory. A third example might be the discovery of a new method—say, a new method for gathering data—that would open the door to many new studies.

Researchers as spokespeople

There is another aspect to our consideration of the academic audience when deciding usefulness. The first two audiences—practitioners and “society at large”—sound good in theory, but there are problems in using them to judge the usefulness of research. Practitioners and “society at large” are typically not

definition of “usefulness”.

members of the academic research community. It is common for academics to be “spokespeople” for practitioners and “society at large” when judging the usefulness of research. Because practitioners and “society at large” typically do not review academic papers, academic reviewers must speak on their behalf.

An illustrative example is the standard of “managerial insight” that has become a common criterion for publication in management journals.⁴ Under this conception, research on a management topic should be sufficiently “insightful” or “useful” to a practicing manager. In practice, this standard is judged by other management researchers, who may not themselves be practicing managers.

This spokesperson role is, in many cases, entirely necessary. As mentioned above, it is an “easy” case to judge the usefulness of a piece of research when even a non-expert can recognize its significance. Expertise is often required to judge the *eventual usefulness* of an idea to society.

Too useful?

So, can a piece of research be “too useful”? Let’s consider this question audience by audience.

Can something be “too” useful for a practitioner? There is a risk of this when the academic research is *highly* practical, say, solving a specific business or policy question in fine detail. There is a risk of clouding the distinction between “research” and “practice”. If a researcher’s work is very similar to the work done by practitioners, why doesn’t this researcher just become a practitioner?

It is even harder to imagine that something can be “too” useful for “society at large”. But here is when our understanding of the idea of researchers as spokespeople comes into play. There can be a lack of agreement between the researcher as a proxy for “society at large” and “society at large” itself. One example is development as an academic discipline. Researchers may feel that a specific type of new technology or procedure can help the poor people of a developing nation when new technology has unintended consequences that only further dependence and undermines development.

Tensions on how to assign usefulness can also arise when a researcher acts as a proxy for practitioners. For example, in fields that leverage mathemati-

⁴See, for instance, [4].

cal techniques to analyze applied models of potential interest to practitioners, researchers may undertake tremendous effort to emphasize the usefulness of their model to practitioners (with researchers often serving as their spokesperson) and much less effort to explain the veracity and logic of their mathematical techniques. Of course, it is possible that a careful explanation of the mathematical technique could be more useful for other researchers in their efforts to produce papers. This reveals the tension between usefulness from the perspective of doing more research versus usefulness for some conceptual external audience. This can be a source of confusion among young researchers in applied fields.

To give an example, I (Chris) have a distinct memory of sitting in a talk where the presenter talked about how some inventory decision-makers should make decisions in a particular way. I suddenly realized that no one in this room has ever or likely will ever make an inventory decision. But you may write papers about making inventory decisions. Why is the speaker talking to us as if we are inventory decision-makers? Why doesn't he talk to us as if we are people who write papers about making inventory decisions? This may sound like a subtle difference, but it is a consequential difference, in my opinion. If the audience is full of inventory decision-makers, they probably do not care how the speaker came to her ideas; they want to know how their ideas impact their decision. By contrast, if the audience is full of people who wish to write papers about inventory decisions, all the details of how the speaker got the results in the paper would be helpful to them.

Finally, there is also the possibility of being “too useful” when it comes to the audience of other researchers. A researcher may have struggled for years to make a discovery and may be less than willing to describe their methods to other researchers fully. Revealing the method in detail is, in a sense, “too useful” and may dilute the researcher's competitive advantage in producing research papers. In another example, an empirical researcher may have gone to great lengths to collect data on their subject. It would undoubtedly be “useful” to other researchers to publish the data in full (even the parts not yet used in their published work), but it would be unwise for their careers.

Difficulty

This brings us to our third aesthetic principle: difficulty. Research displays difficulty when it feels hard to come up with or reproduce. Some researchers

may feel pride in the fact that “few people can do what we do” because others lack the necessary intelligence, training, or resources.

In some fields, clever use of advanced mathematics is taken as a proxy for difficulty. Due to the density and clarity of its concepts, mathematics has long been used as a type of barometer for intelligence. In empirical work, the necessary and challenging work of building trust with subjects to gain access to proprietary data sets or collecting observations over long periods of time is a sign of difficulty. In other cases, research requires expensive tools and machinery—simply gaining access to them can be considered difficult.

Difficulty is highly valued partly because it signals effort and justifies the need for advanced degrees to conduct research. Indeed, if anyone could do research, why do you need to go through so many years to earn a doctoral degree? Difficulty is an effective lens for communicating the rare abilities of the researcher and what sets them apart from others. What is the value of research if not to succeed where others have failed?

Dimensions of difficulty

Levels of difficulty can be assigned to each of the five dimensions of research introduced in [Section 3.2](#): setting, question, data, result, and method. We leave it to you to ponder the implications of this categorization.

The assigning of difficulty can sometimes give rise to the unfortunate comparison of whole academic fields to each other. Indeed, it is not uncommon for researchers in one academic community to compare their field favorably to others in terms of difficulty; for example, because it uses more mathematics or more sophisticated empirical methods. We believe such cross-field comparisons are problematic because assessing the difficulty of undertaking work that you are *not* intimately involved in is challenging.

One perspective on why it is hard to judge difficulty, especially in unfamiliar fields, is the concepts of *ex-ante* difficulty and *ex-post* difficulty. Some problems are very difficult to state and get a grasp on, even if, in the end, they have a simple solution. We call such problems *ex-ante* difficult, meaning they are difficult at first but seem easy in retrospect. For example, mathematical modeling exercises in economics can take on this flavor. A master economic modeler can make a problem look very simple, but if you approached it independently, you would struggle to describe it.

By contrast, some problems appear easy when first encountering them,

but a complete or detailed solution may be challenging to grasp. We call such problems *ex-post* difficult. The classic example here is something like Fermat's Last Theorem, which is easy to state and entirely believable (indeed, why doubt it?) but notoriously difficult to prove to total satisfaction. Every academic field has its own combination of *ex-post* and *ex-ante* challenges that an outsider might fail to appreciate at a first, or even fifth, glance.

Too difficult?

At any moment, anyone on the planet can ask any other person at least one question they cannot answer. Indeed, consider the question: "When I was eight years old, playing against myself in an imaginary game of basketball on 5 July 1991, what was the final score?" There are, in fact, infinitely many questions that are essentially impossible to answer. There is no upper bound on difficulty.

So consider again our perspective that the practical purpose of research (as a career) is to become accepted into the academic research community. Something can be too difficult if there is a tiny audience of people who care. Indeed, who cares who won an eight-year-old imaginary basketball game? Even when others might be interested in a research result, the method used to find it or the language used to describe it may be too difficult for others to follow.

An apt example comes from the world of skateboarding. One of the most important tricks in skateboarding is a kickflip: a skateboarder jumps with her board and "kicks" the board in the right way so that it flips before she lands on it again. Every skateboarder at every level (beginner to professional) regularly works on improving their kickflip.

A natural extension of the kickflip is the double kickflip, where the board flips around twice before landing. Indeed, you can go into triple and quadruple flips using the same principle. And yet, although considered difficult, triple kickflips are not a common, or even well-respected, skateboard trick. Very few skateboarders even practice them. One reason is simple: a triple kickflip gets closer to the physical limitations of what is possible with a skateboard. As a consequence, almost everyone who does a triple kickflip does it in roughly the "same" way—just barely! In a sense, a triple kickflip is too difficult to reveal the individual artistry of the skateboarder. Even worse, the human eye can hardly tell the difference between a double and a triple, let alone a triple and quadruple.

The same is true of research. The number of people trained to appreciate profound and rigorous mathematical proof may be few. For the rest of us, it is hard to tell how hard it really is. It's like a triple kickflip; it all looks "the same" to the untrained eye. You may prefer a middling level of difficulty (like a single kickflip) that can be used to show your style and originality in exactly how you pull it off. It is the type of nuance that people can see and appreciate.

Generality

Research exhibits the aesthetic principle of generality when it speaks to a broad range of phenomena, capturing some universal "essence". Just like a circle in nature as an example of the idea of the perfect Circle, generality searches for timeless universals.

Generality is valued because it feels powerful and helps you get the sense you are discovering patterns and insights that go beyond common experience or simple observation. Generality can cut across time, speaking not just to a particular time or place but to all times and all places.

Every high school student of physics experiences the feeling of generality. The simple formulas of force and position appear to be magical. They are as true as when they were discovered; they will apply 100 years equally as well as today. They are timeless truths.

Every empirical researcher is, similarly, on an endless quest for causality: does A cause B ? Finding a pattern relating A and B in a single dataset, or even across five datasets, is not so interesting. Researchers strive for some intrinsic connection between A and B that is "above" any particular instance.

Dimensions of generality

One can define a notion of generality to apply to all five dimensions of research introduced in [Section 3.2](#): setting, question, data, result, and method. We will not belabor how to interpret generality in each of these dimensions but instead only highlight two: generality of method and generality of result.

A classic example of the generality of method is the simple maximization principle, often learned in high school: to optimize a function, you take its derivative and set it equal to zero. This general principle lies behind both complex theories (where it is extended to a myriad of different situations) and in wide applications (many papers that use mathematical modeling in the social

sciences, for instance, rely on this basic principle in their analysis).

Of course, it is not easy to come up with a principle as general as calculus. Researchers nonetheless go to great lengths to describe the generality of their tools or approaches. Empirical researchers who describe an identification strategy take time to describe other scenarios where the strategy will also work. Applied mathematicians may describe a new tool for analyzing a particular partial differential equation and spend several paragraphs explaining where it may find application, preferably in a broad (seemingly unconnected) array of settings.

Related is the notion of generality of results. A classic example of this is in the topic of groups in abstract algebra. A group is an abstract concept that provides a common language for seemingly disparate topics, including symmetries of geometric objects, divisibility of integers, and operations with matrices. Any result you prove about a group has broad implications in geometry, number theory, and linear algebra.

Sometimes, the “general” structure behind many seemingly different phenomena only comes to light after many years of these theories developing in relative isolation. For example, ten papers make specialized arguments to establish the mathematical structure of ten different problem domains. A researcher comes along later to discover that all ten papers use essentially the same argument. Putting this argument in a “clean” setting (using abstraction or re-framing), the mathematical structure in these ten examples can now be seen as instances of one, more general, structure.

Patterns of generality

Let us examine two approaches to the concept of generality that, at first glance, may feel contradictory. This discussion illustrates how a deep appreciation for how others conceive of generality is a nontrivial and consequential endeavor.

Consider a mathematical model for decision-making in a business context, for example, deciding the price of a good to be sold on a webpage. A model that assumes that the demand function for the item is *known* is a *limited* model, in the sense that this condition is unlikely to be fully satisfied in practice. Most businesses do not know their demand function exactly. To make the model more general, the researcher aims to relax as many assumptions as possible, allowing, for example, demand to be learned over time. A model becomes *more general* if it allows a broader range of inputs and still

provides guidance on how to set prices. Of course, as a model becomes more general in this way, it becomes harder to analyze mathematically, and the quality of its solutions may degrade with complexity.

Contrast this with an alternative scenario. A researcher is using a mathematical model to help explain why grocery stores only change prices on a weekly basis instead of daily or hourly. What are the possible reasons for this choice?

In this case, the goal is not to set a model that gives precise guidance on setting prices exactly. If you used a model with many model parameters to study this scenario, it could be hard to pinpoint exactly what was causing the difference in pricing time scales. A model with few, but explanatory, parameters would be preferred. This simple model can more clearly reveal some key drivers of the pricing decision.

The latter form of generality is akin to the logical principle known as Ockam's razor. According to this principle, a conclusion of a logical argument is more robust (i.e., more general) if fewer premises give rise to the same conclusion. A model with five parameters that offers clear reasons for a difference in pricing time frames—hours versus weeks—is more explanatory than a model with fifty parameters that does the same.

Someone in the former mindset—a model is more general if it can handle a wider array of inputs—looks at the work of someone in the latter mindset—a model is more general if it can explain the target phenomenon with fewer inputs—and asks: how can this simple model be used to determine price in practice? Conversely, the researcher interested in explaining the phenomenon of long time scales for price changes looks at a complicated model with many parameters and asks: how can such a complicated model provide me with clear insight?

All of this underscores that it is important to understand the purpose of your research to know what notion of generality best suits the situation. Is the researcher interested in a robust method to determine optimal prices in many scenarios? Or is the researcher interested in a robust result with a simple enough logic that holds in many related situations despite many “superficial” differences?

Too general?

Can a method or result be “too general”? At first glance, this seems counter-intuitive, but it again depends on the question and goal of the research. In the example of groups in abstract algebra, this theory is powerful because it unifies many disparate mathematical phenomena. The downside, however, is that important things can be lost in abstraction. Indeed, someone interested in matrix analysis may find little help studying a book on group theory, even if many of the results apply to groups of matrices. The researcher may be interested in the “details” that have disappeared at the level of abstraction of groups.

At the extreme, universals risk feeling like platitudes or tautologies: they have no “bite”. An example of this is the idea that “every decision is an optimization problem”, meaning every decision must act on certain variables in the face of constraints to maximize or minimize certain objectives.⁵ You can view this as a statement of the immense applicability of optimization theory,⁶ but probably a safer perspective is that the statement is tautological: you can *define* every decision as an optimization problem, but there is no guarantee that this definition is helpful in any way. The assertion that “every decision is an optimization problem” can essentially be translated to mean that “every decision that can be fruitfully modeled as an optimization problem is ... an optimization problem”.

Surprise

Surprise takes us into the realm of the dramatic. Research exhibiting surprise is sometimes called “counter-intuitive” because it goes against commonly-held beliefs and can alter a reader’s perspective. The classical example is the Copernican assertion that the Earth is not the center of the universe. The assertion is indeed quite counter-intuitive. We all experience an Earth that feels stable beneath our feet and a sun and stars that appear to move across the sky. It is not straightforward to conclude that precisely the opposite is true: the Earth is moving, and the sun and stars are stationary.

Surprise is valued in research because it highlights how human thought

⁵Indeed, we define an optimization problem in [Section 3.3](#) to study aesthetics, providing evidence for this point.

⁶As an optimization theorist, I (Chris) find this assertion to have a nice ring to it.

can be dramatically flawed when seen in a more accurate light. It speaks to an evolving understanding of human knowledge and how great leaps can be made abruptly. The wielder of research surprise can be an entertainer of sorts, like a mystery novel writer who springs “twists” and “turns” on unsuspecting readers. Surprise can raise the dramatic quality of a paper to be more readable and thought-provoking.

If the surprise is sufficiently deep, such as the Copernican assertion of a sun-centered solar system, it can precipitate a paradigm shift that changes a field.

Patterns of surprise

While there can be elements of surprise in all dimensions of research—setting, question, data, result, and method—the dimension easiest to associate with surprise is “result.” A surefire way to create surprise is to provide a new and unexpected answer to an old question in a way that better explains the data than any previous answer.

In a compelling article [2] on operations management research, Cachon formulates surprise as follows: “what was thought to be X is really Y ”. Cachon goes on to delineate several possible categories for what X and Y could be, such as, “what was thought to be *simple* is actually *complex*” or “what was thought to be *small* is really *large*”.

In the construction of surprise, arguably, as much attention must be given to establishing the status quo (X) as the “twist” (Y). The audience needs to buy into X for the twist to be surprising. Otherwise, the “twist” feels more like *deus ex machina* than a satisfying or revealing turn of events. In the case of Copernicus, the status quo needed little justification; it was entirely standard to believe that the Earth was the center of the heavens. In many other cases, the audience must first be sold X and only then sold Y .

Timing is also important: you cannot spring the same “surprise” over and over again. Only when put into a historical context does the revelation that the Earth is not the center of the heavens maintain its surprise. Movies that heavily rely on twist endings don’t have a lot of re-watch value.

Too surprising?

All of this leads to a natural skepticism for a result that feels *too* surprising. Common sense is sometimes (maybe even often?) correct. Consider when

the illusionist David Copperfield disappeared the Statue of Liberty on television.⁷ Common sense says this is not possible ... and common sense is correct. The trick remains entertaining, but such a grandiose reversal of common sense is *too* surprising to be credible. Purveyors of surprise in their research also need to be wary of such suspicions.

Sometimes profound truths are entirely unsurprising. One of the foundational statements of human society is the following: “My mom loves me”. It is deeply consequential to the life of a child for them to come to understand the truth of this statement. And yet, it is hardly surprising. That a mother would love her child is common sense. It is a lack of love that is shocking.

It is sometimes a nontrivial matter to prove the veracity of common sense. Much profound research has been undertaken to show that common sense is well-founded. An example is the “China Study” on the health effects of traditional Chinese diets [3]. Scientists can sometimes take hundreds, if not thousands, of years to appreciate the health benefits of certain traditional cultural practices. But the Chinese grandmother’s response to the study may simply be: “duh”.

Simplicity

This brings us to the last of our six aesthetic principles of research: simplicity. We say a research idea has simplicity when it is easy to communicate and grasp but says something powerful. The image for simplicity in [Figure 3.1](#) illustrates the classical quotation attributed to Archimedes: “Give me a lever long enough and a fulcrum on which to place it, and I shall move the world.” What could be simpler than a lever? And yet, if applied correctly, it is tremendously powerful.

Research that exhibits simplicity is valued because it can be clarifying. It summarizes profound ideas in a clean and understandable way. Simple ideas can be popularized beyond the academic community and, when successful, enter into popular consciousness. Examples include “the invisible hand” from economics, “unintended consequences” from sociology, and “paradigm shift” from the philosophy of science, among many others. These simple ideas can form the identity (especially to outsiders) of entire research communities.

⁷A YouTube search will suffice to find some footage of this illusion.

Comparing simplicity to other aesthetic principles

By this point, we have introduced so many aesthetic principles that you might be having a hard time distinguishing them. If some of the principles feel roughly identical to you, don't let it stress you, they are meant neither to be independent nor exhaustive. For example, you might argue that simplicity is just some variant of generality or surprise. Indeed, the lever is a general principle, but it has surprising power when taken to its extremes.

However, we hope each principle may illustrate some nuance not entirely captured by the others. For example, with generality, there is no particular requirement that something very general needs to be "simple" in the sense of being easy to communicate or grasp. Ample evidence for this is abstract algebra in mathematics. This theory can be pretty general, but it would be a stretch to call it simple. It can be difficult to ground the ideas at high levels of abstraction. It is easy to lose "grasp".

Similarly, something that is surprising need not be simple. Indeed, constructions of surprise often have the feature of showing that the status quo (X) is *too* simple in some way. A classic example of this is Einstein's theory of relativity and its marvelous conclusions that an object can become *shorter* at high speeds, something that can not be captured by "simpler" models of physics. This is a surprising conclusion, but it would be a stretch to call the explanation simple.

Patterns of simplicity

Why are we attracted to simplicity? There are many reasons, but let us reflect on one consideration here. Simplicity plays with three related concepts: justice, profoundness, and accessibility. If something is truly profound and yet inaccessible, how could this be just? Suppose you genuinely believe that there are deeply profound things that will always be inaccessible to most people. This is grounds for a form of elitism seen in secret societies that allegedly rule the world "from the shadows". Conversely, if you believe strongly in justice, this seems to inevitably lead you to a program of making the profound more and more accessible.

This was the essence of the Reformation movement in 16th-century Europe: If salvation must be bought from the Church at a dear price, is this not a grave injustice? Muhammad (peace be upon Him) was deemed a criminal by the leading tribes of Arabia because He aimed to destroy the idols that ce-

mented that group's power. By controlling the idols, these leaders could keep the masses from their source of salvation. In both cases, the revolutionary message was simple: Each person has the means to attain salvation. Salvation is both profound and accessible to all.

There is this sense in which things are complex because they are poorly understood, and if you understood them better, they would become simpler. Part of Copernicus's discovery was a clean way of explaining the "contrary" motions of planets. Previous theories had complex (and sometimes illogical) ways of describing the motions of planets in order to preserve the belief that the Earth was stationary. Many important results in mathematics have newer and easier proofs than when they were initially discovered. There is a sense that the path towards "true" understanding is also a path towards simplicity.

Too simple?

Of course, it's possible for something to be *too* simple if it doesn't explain much, trivializes, or misleads. As an academic, it can be pretty fear-inducing to have your work marginalized as being overly simplistic. Indeed, you take a risk in presenting your work as "simple" and "straightforward" since it runs afoul of the difficulty aesthetic (more on this below). This phenomenon may explain, at least to some extent, the casual criticisms of academics as living in ivory towers, speaking only to themselves and not lowering themselves to the public.

3.3 Developing an aesthetic profile

We return to the research aesthetics throughout the rest of the book. In particular, **Part III** provides practical advice for developing a facility to highlight your research's aesthetics. For now, we deliberate on the question of developing your own taste or aesthetic profile. This is an important part of elevation, as illustrated in **Figure 2.1**. Gaining a deeper appreciation for your own research taste helps you better understand the preferences of others and, by extension, the research community you hope accepts you.

Coherence among the research aesthetics

It is likely obvious to you by now that it is difficult for one piece of research to meet a high standard on all six research aesthetics. This is an entirely unrealistic standard. You would like your work to be novel, useful, difficult, general, surprising, and simple, but likely you need to compromise. In this compromise, it is helpful to think of which of the six aesthetic principles you value the most. Maybe you like the idea of surprise, but you rank usefulness more highly. Perhaps you are not so concerned with difficulty as long as it is surprising.

More than simply ranking the preferences, there are times in which it feels that the different aesthetic principles may even conflict. We illustrate this point with three instances.

Consider usefulness and difficulty, particularly when considering usefulness from the perspective of practitioners. To amplify the perception of the difficulty of a research project, a researcher may seek to demonstrate how few people can produce this type of work. And yet, for this research to penetrate into practice, it needs to be understood by many “non-experts”. Something can be too difficult to be useful. Of course, one way around this is when a researcher acts as a proxy for practitioners. This may allow them to find ways to justify both difficulty and usefulness simultaneously.

There is also a natural tension between difficulty and simplicity. Finding ways to demonstrate that something can only be done by the well-trained few—and easily understood by the many—is a tightrope walk. However, it is not impossible. For example, you can demonstrate the challenge and ingenuity involved in *coming to* an easy-to-communicate conclusion. Once the conclusion has been established rigorously, a simple and sufficiently accurate heuristic can make the reasoning of the conclusion clear enough for “the many”.

Finally, consider the tension between generality and surprise. A typical construction for surprise is that common sense fails in some important way under certain conditions—the more restrictive the conditions, the more potential there is for surprise. And yet, the more restrictive the conditions, the less general the finding is. Indeed, you can argue that the following formulation is roughly true under many circumstances: “common sense” is “essentially correct most of the time”. When this formulation holds, deviation from common sense (that is, surprise) must necessarily restrict the “general” case in

some substantive way. This is not to say that this restriction is not interesting or important, but it does infringe somewhat on the principle of generality.

Optimizing for aesthetics

Acknowledging the inherent tensions between the aesthetics, you must work within tradeoffs to find their “sweet spot”. To find your aesthetic profile, you can consider different weights for the various aesthetics. For instance, you might find that usefulness is the most important aesthetic for you—e.g., you want your work to make a meaningful impact on the non-academic world—but you also value difficulty and generality, maybe half (or so) as much. This will help guide you to the types of projects you work on and how you write up your results.

Another approach is to formulate your taste as an optimization problem. You can set lower and upper bounds on some of the aesthetics and choose to maximize one or more of the others. For example, if your passion is for your work to be clearly understood by others, you may want to maximize simplicity but acknowledge that simplicity can go too far on its own. Accordingly, you can put a lower and upper bound on difficulty—you want it to be difficult enough to show rigor but not too difficult to compete with the cleanliness of the message. Surprise might not be your main concern, but you enjoy it when you read results in other papers written in a way that emphasizes elements of surprise, so you decide to put a lower bound on surprise, and so on.

Of course, you must acknowledge that a given piece of research may never meet these bounds. For instance, even if you highly value surprise, you may have invested heavily in a project that is not that surprising. This calls on you to adjust your “ideal” aesthetic and see what aesthetic elements can be emphasized with what you have.

One of the benefits of understanding a wide array of aesthetics is that it allows you to “see” the beauty in many things. So while you decide on what you like (and why you like it), you can also learn to appreciate why others like something different. When faced with a scenario where your work does not perfectly meet your aesthetic ideal, it may meet the aesthetic ideal of others. This may allow you to “save” a research project you might otherwise abandon or discard. The fact that someone else may find it beautiful may be enough to motivate you to finish it.

Some “standard” aesthetic profiles

In the spirit of educating the young researcher on the coherence of the six research aesthetics, we examine a few aesthetic profiles that the authors have come to understand through our interactions with other researchers. The list is hardly exhaustive, but hopefully representative of some of the research preferences you might see “in the wild”.

We have mentioned already hinted at a few aesthetic profiles in passing. The first is what you might call the “applied mathematics” profile, which either aims to maximize difficulty or have some high lower bound on difficulty while also valuing generality and usefulness to varying degrees. Often the standard of difficulty here is measured in the originality and complexity of the mathematical arguments found in the research. Surprise and simplicity are valued—as long as the underlying arguments are sufficiently challenging—but are more likely to be “happy accidents” than make-or-break criteria for the success of a project.

Going in the other direction, you can come to what you might call a “pure mathematics” mentality that highly prizes generality and difficulty, often in the form of difficult-to-grasp abstraction (and frequently at the expense of practical usefulness and simplicity). The balance between generality and difficulty plays out differently within subfields of mathematics, with some subfields more interested in “theory building” and generality, while other subfields more highly prizing clever problem-solving and difficulty.

Contrast this with what you might call the “formal models” perspective popular in certain subfields of social science and management.⁸ Mathematics arises in the form of mathematical models that are studied and analyzed to understand the underlying social or managerial context, but mathematics itself is not the main attraction. The use of mathematics forms a basis of difficulty, but mathematical difficulty is effectively upper-bounded to make room for the aesthetics of usefulness, surprise, and simplicity that are more likely to be valued.

In management, usefulness and simplicity can be seen in the desire for papers to illustrate “managerial insights” that can be communicated (in principle) to working managers. In other social sciences, mathematical models can be used to make clean empirical predictions, again focusing more on the

⁸“Applied theory” is a more common name in economics that describes a similar paradigm. In other areas of management, they might call it “stylized modeling”.

simplicity value of mathematics in clarifying logic and less so on the ability of mathematics to communicate difficulty.

Finally, there is what you might call the “empirical” mentality, which aims to find simple, clarifying, and possibly surprising conclusions based on data without as much concern for displaying difficulty through the use of mathematics. Of course, not all empirical research is mathematically trivial. It presents its own difficulties aside from mathematics, particularly in finding unique and powerful data sources and coming across the “right” empirical observations to make sense of the available data. Generality is also highly valued for uncovering clean and logically tight conclusions that can explain patterns in the presented data and, by implication, related scenarios. Empirical research builds generality inductively from particular instances toward general conclusions, while mathematical modeling exercises tend to work deductively.

As we said, this list is far from exhaustive, and there is no claim that it perfectly represents even those researchers that identify with one or more of the mentalities listed here. The value for you is not to over-generalize based on these profiles but instead to provide a window of what others “might” be thinking and how this thinking could be quite different from your own.

When I (Chris) was a young researcher, I found myself often puzzled about what excited people about certain papers and lines of research and not others. I grew up more in the “pure mathematics” camp, so I found it challenging to understand how other management researchers could muster excitement about a project that used basic mathematical reasoning. It took me some time to realize that many researchers were not as enamored with mathematical difficulty as I was. They were genuinely interested in other aesthetic principles. In my arrogance, I thought it was maybe because they were less gifted or trained in mathematics. This turned out to be highly erroneous. Just because someone is not interested in mathematical difficulty does not mean they cannot produce it.

Once I become aware of alternate perspectives, I found it easier to talk to other researchers and champion their work. It also helped me think of ways of describing my work in ways that could be more interesting to others. Over time, I came to respect and even admire the various aesthetic positions we detailed above.

We sincerely hope you sense no subtle hierarchy of aesthetic preferences in the above descriptions. We genuinely believe there is no such hierarchy;

each profile has its own coherent logic.

Learning aesthetics from others

We must admit something else about the aesthetic profiles detailed in the previous section. These are only educated guesses on our part because getting someone to share their aesthetic viewpoint frankly is very rare indeed. This is why papers like Cachon's [2] are so valuable. You don't need to agree with his perspective, but you can certainly appreciate his willingness to share his views openly.

So how do you learn the aesthetic viewpoints of others? This often requires reading between the lines in papers (which we demonstrate in [Chapter 6](#)) or paying attention to what excites people during presentations (a skill we explore in [Chapter 7](#)). But it is much more common to hear things negatively expressed. Many questions raised by audience members in academic talks are of the doubting or questioning type, and most referee reports of papers spend the vast majority of their words emphasizing deficiencies rather than things the reviewers liked. Of course, even these negative opinions can point towards underlying preferences, but some sleuthing is needed to put together what would satisfy skeptics.

Several possible explanations exist for the broader prevalence of negativity than positivity when receiving feedback. One possibility is that it is simply easier to describe what you do not like than what you do. Enjoying something is different than explaining what you like about it. Moreover, claiming to like something closely aligns you with it. You stay more independent if you point out how things could be improved.

A second explanation can be related to the old adage by Tolstoy [23]: “Все счастливые семьи похожи друг на друга, каждая несчастливая семья несчастлива по-своему.” In English, “All happy families are alike; each unhappy family is unhappy in its own way.” When you are firmly planted in your aesthetic preference, it is easy to assume others have the same preference, so it need not be discussed. We all recognize great beauty without words. I (Chris) fell prey to this type of thinking when I assumed everyone was most interested in complex mathematics. It would be best if you did not take for granted that others have the same concept of beauty as you, even within a narrow field.

3.4 Putting it all together

As a form of summary, we provide an example of young researchers exploring the ideas in this chapter. The story is fictional but inspired by real-life interactions with many young researchers over the years. The primary purpose is to provide you with an example of how natural it can be to discuss your approach to research with those you trust.

Another week, another heated conversation between Damon and Marquita in the Ph.D. student lounge. Each week their department has an outside speaker come to give a talk. Their department is multi-disciplinary, so the students there are exposed to many different approaches and ideas. Because of this, none of the talks are ever a unanimous hit with the audience. Inevitably, Damon and Marquita find themselves with opposite opinions.

Damon: The talk this week was horrible. Did the speaker really think we believed his model? It made no sense.

Marquita: I thought it was interesting. Certainly a difficult setting, and yet he was able to make progress. What more do you want?

D: Forcing a hamburger through a straw is difficult and equally as pointless. Why doesn't he just come out and tell us he did this work to show off his skills?

M: Damon, calm down. Now you are just being rude. He was using his knowledge and training to study a problem that challenged his way of thinking. What could be wrong with that?

D: I guess I feel we have all this privilege and opportunity to sit down and think all day, so why not think of something useful to society?

M: I am part of society, and I find it useful because it inspires me.

D: Well ...you know what I mean.

M: No, I am not sure that I do.

D: If we only do research that inspires other researchers and never impacts the real world, what good is that?

- M: What does the “real world” mean? Presumably, you mean it to exclude me and other researchers. He came to give a talk to a group of other researchers. To inspire us. And now you are upset that he did precisely this? Was the “real world” (as you call it) sitting in the audience expecting something different?
- D: No, there are just people like you sitting there who only seem to care about themselves and their precious “inspiration.” Meanwhile, the world is burning.

With the conversation overheating, Marquita gets up to pour herself a third cup of coffee for the day, thinking about what she should say next.

- M: Damon, don’t get me wrong. I am all for impacting and helping people out there. But I can do so by volunteering to help people learn mathematics, which I do every week. Some of them have even started undergraduate degrees in our field! It’s easy to help a real person; why do I need all this research?
- D: But helping a few people here and there (and I am happy it makes you feel good) is not real impact. How does this change the world? And why do we need to study five years of a Ph.D. for only this kind of impact?
- M: But Damon, you are making my point for me. I like research that shows ingenuity and feels deep. I don’t expect it to be helpful to society at large because I do that outside of my research. I like it for what it is, and understanding “what it is” requires me to study for five years in a Ph.D. program.
- D: I am glad you have such a small vision for yourself—congratulations!
- M: Calm down, Damon, you are getting me upset again. If discussing why I like research bothers you, I am happy not to discuss it anymore.
- D: No! This is fun. And you know I am partially kidding, right?
- M: Yes, of course. Damon, I know you! You like to amp up the drama to make your point. I like that.

At this natural break point in the conversation, Damon and Marquita get up to get some cinnamon rolls from the nearby cafe. As they walk to the cafe, they continue their discussion.

D: Going back to the talk, you claimed to like the ingenuity in the talk. I guess I didn't see it. Nothing surprised me in what he said.

M: Why is it important that it be surprising?

D: Why am I listening if everything shared is common sense? If he was so smart and the work was so hard, shouldn't it surprise me somehow?

M: I don't see why. It can be very clarifying to turn something complex into something simple. Why does the conclusion also need to be surprising?

D: I see your point. Maybe I just find that kind of work uninspiring. What good is it to make something complex into something simple if it just says the same thing but in different words?

M: Simplicity is insightful on its own, is it not? But I must say, I did find the results he presented in today's talk surprising. I have worked on related questions before, and his approach was unexpected to me.

D: So, it's surprising in some narrow sense to a few people who are deeply into the problem. Again, not sure if that's very meaningful.

M: What are you expecting? That it's surprising to some school children down the street?

D: Of course not. I guess I was expecting it to be surprising to me.

M: Oh, I get it. It's only meaningful if *you* find it surprising. Do you honestly believe that your surprise means more than mine?

D: Well, if you put it that way ...

As Damon and Marquita neared the cafe, their conversation quickly shifted to their impending comprehensive exams. Enough time was wasted talking about research philosophies. Onto the real business of staying in the Ph.D. program.

3.5 Reflection questions

The questions below are provided for you to reflect on what you have read in this chapter. If possible, it is beneficial to discuss these questions in a group of your peers.

1. Describe something (besides research) that you find interesting. Why do you find it interesting?
2. Describe something (besides research) that you find beautiful. Why do you find it beautiful?
3. What, if anything, do your answers to the previous two questions have to do with the type of research you are interested in?
4. Can you learn to be interested in things? If so, how?
5. Describe a personal discovery you made that is important to you but is not “novel” in a universal sense. (That is, other people made this discovery before you.)
6. In what ways do you feel useful to your family? Your friends? Society-at-large? How might a career in research make you more useful to your family? Your friends? Society-at-large?
7. What, if anything, have you done in your life that you feel is not useful to anyone?
8. Describe something difficult that you have achieved in your life. Why is it important to tackle difficult things?
9. Do you like to be surprised (by, say, a surprise birthday party)? Why or why not?
10. Have any research papers, courses, or textbooks surprised you? If so, provide a concrete example.
11. Is there something that took you a long time to learn that, in retrospect, is actually quite simple? How did it feel when you had this realization?

12. As of right now, what is your research aesthetic profile? That is, how do you weigh: (a) novelty, (b) usefulness, (c) difficulty, (d) generality, (e) surprise, and (f) simplicity in terms of relative importance to you? (If in a group) How do your research profiles compare to each other? Do any of the differences surprise you?
13. In what way, if at all, do you relate to the story of Damon and Marquita? What do you take away from the story?

Chapter notes

We wanted to mention the papers [2] and [24] that started on a journey of thinking about what was “beautiful” in the field of operations management. Our thoughts on the nature of truth and beauty in science were greatly influenced by [15].

Chapter 4

Research orientations

In [Chapters 1](#) and [2](#), we detailed how there are many potential paths to a successful research career. In [Chapter 3](#), we saw that one of the reasons for the multiplicity of paths is a multiplicity of perspectives of what makes research beautiful. In this chapter, we explore additional multiplicities of perspectives into the purpose and nature of research.

We maintain the overall perspective—detailed in [Chapter 2](#)—that success in research as a career entails acceptance by the research community. However, such acceptance, more or less depending on the person, can be considered the practical destination and not always the driving motivation of a research career. Similarly, although the working artist must sell their art to survive, they need not consider “selling” to be the entirety of their calling.

Even among successful academic researchers (i.e., those who the academic research community has accepted), we see various motivations and perspectives that guided them to acceptance. Not every successful research approaches research in the same way. What people believe is happening as they undertake the task of “research” varies widely. In this chapter, we examine this variety of research purposes, motivations, and approaches.

4.1 The six research orientations

This exploration will present six representative (if extreme) *research orientations* as illustrative examples of how you could weather the journey toward research success. These research orientations are illustrated in [Figure 4.1](#).

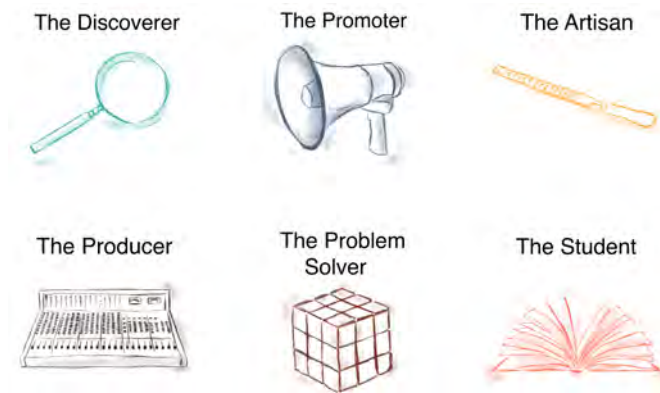


Figure 4.1: Six research orientation

We want to stress that the orientations do not represent any one person we have known. Any resemblance you may associate to an orientation with a real-life person is accidental. These examples should be viewed as caricatures of a sort, taken to an extreme to examine particular different perspectives in sharp relief. The hope is that everyone can relate—at least to some degree—with each of these six extremes.

The array of orientations captured in [Figure 4.1](#) is not meant to be a scientifically grounded taxonomy but simply a framework to communicate insights. Indeed, the goal of discussing these six orientations is to enrich your vocabulary of acceptable and acknowledged research motivations and approaches.

Depending on your field, there can sometimes become a dominant *orthodoxy* for the “correct” way to think about and pursue research. Other perspectives may be judged as less pure or perversions of some ideal. Our goal here is not to judge the six orientations. We make no effort to rank or elevate one over the other. Indeed, this is precisely the opposite of our intentions. Our goal is to develop an appreciation of diversity. We will be happy to know that you begin to see many possible ways forward in some “mixture” of the orientations, or after reading about them, realize that your way must be entirely different.

In our discussion of the six research orientations, we consider three dimensions: (i) motivation, (ii) approach, and (iii) how they conceive of setbacks and challenges. You will notice that this description does not include specific skills, talents, or character traits. We discuss these matters in greater

detail in [Chapter 5](#) and throughout [Part III](#).

The Discoverer

The Discoverer is motivated by the unknown: “To boldly go where no one has gone before!”¹ The Discoverer is not content with the status quo answers of their academic field; they are searching for something deeper, something closer to an ever-illusory truth. Where others see a cloud of confusion, the Discoverer sees opportunity. Where others may try to limit what a research field should be about, or circumscribe what is possible to know, the Discoverer aims to go beyond known limits and forge new possibilities.

For the Discoverer, risk is inherent in the research enterprise. When dealing with the unknown, you must take stabs and place bets. You cannot be content with the “standard” techniques or perspectives because these, more likely than not, take you to familiar places. It is impossible to perfectly pack your luggage on a journey into the unknown.

For the Discoverer, papers and other forms of research output are only milestones of a journey toward the truth. Truth and discovery are their own rewards. A research career allows the Discoverer the freedom to explore.

For the Discoverer, because research involves risks, setbacks are inevitable. If the path is easy to traverse and free of danger, others would have discovered its truth already. The fact that a journey is full of challenges proves the journey is worthwhile.

The Promoter

For the Promoter, the true payoff of research is when an idea or discovery has changed people’s minds or behavior. For the Promoter, discovery itself is a way station. The final leg of the journey brings you to a forum of conversation and persuasion, where doubt is conquered by certitude, and skepticism gives way to faith. Justice only prevails when the truth is heard and accepted. Good ideas do not sell themselves; they must be taken to the people.

For the Promoter, research starts with an audience. The dominant questions are “What are people saying?” and “What can I say that will change what they are saying?” The Promoter must stay aware of the controversies and “up”

¹Quoting the classic opening line of Star Trek.

on the literature. The Promoter always asks themselves in the course of a research project: “Is what I have enough to be convincing to others?” or “Is what I have worthy of injecting into the conversation?” The Promoter is excited to share their learning with others, seeing this as a kind of “payoff” to their endeavors.

For the Promoter, setbacks and challenges in the research process become part of the narrative. Success, as well as struggle, can be made part of the story that convinces the audience. When a result is weaker than imagined, the Promoter asks themselves: “Is there a way to turn this apparent weakness into a strength by changing the story or perspective?” The Promoter may find themselves working backward—not so much “How can I discover something interesting” but rather “How can what I discovered be made interesting?”

The Artisan

The Artisan approaches research as a craft, like a carpenter who makes chairs. A beautiful chair may not change the world (and why would it need to?), but it can help a few people meaningfully. A well-made chair can support someone their whole life. It’s okay if a chair is not groundbreaking; it does its job regardless. Such a chair has its own meaning. The carpenter makes chairs with pride and to the best of his ability.

For the Artisan, research is not too much different. Each paper can be likened to a beautiful custom-made chair. The Artisan is okay if the paper is not that groundbreaking; they care that it does the job. A well-made (and well-published) paper can support the career of one or more researchers their whole lives. It puts food on the table. That is enough to be meaningful.

For the Artisan, the research paper is a monument to the craft of the researcher. The reality of the paper is evidence of diligence and honor. The job is to produce papers, just like the carpenter’s job is to make chairs. A carpenter only ever serves a few people—they do not become known to strangers like an actor or a musician—but if all carpenters disappeared from the world, society would soon come to a halt. The Artisan takes his work in a similar light. The work of the Artisan does not need to become known to strangers for them to feel part of a meaningful collective. For example, the academic field of biology has greatly impacted the lives of many, even if a any one biologist toils in obscurity in support of his career and family.

For the Artisan, research is a craft that can be honed. Sure, there are gifted

carpenters, but this does not stop many from learning carpentry. Research, like carpentry, is a skill. The Artisan looks carefully at specifications sought by their customer (here, other academic community members serving as peer reviewers) and produces according to spec to the best of their ability.

When an Artisan faces setbacks or challenges, they may curtail their ambitions. They may ask: “Can I deliver with a slightly simpler design?” The Artisan finds joy in the simple things and returns to tried-and-true methods. The Artisan trusts their training and experience. When facing difficulty, the Artisan is a student of the craft by learning from others. The Artisan appreciates the skill of the master and seeks to emulate it and adapt it to their circumstances.

The Producer

The Producer builds on an analogy to a “producer” in the context of music. A music producer works with artists to help them bring out their best work. They flesh out ideas and crystallize emotion. Iconic examples include George Martin with the Beatles and Rick Rubin with the Beastie Boys—the names behind the names that have shaped popular culture. In the context of research, the Producer thinks of research as a process that brings out and crystallizes the ideas of a group of researchers. The Producer is not necessarily motivated to discover something on his own (like the Discoverer) or to change others’ minds (like the Promoter); the Producer’s motivation is to witness and capture the talents and inspirations of others.

The Producer acknowledges that talent and inspiration alone are not enough. These do not flower or mature without guidance and structure. Much attention needs to be paid to setting up the conditions for success, to finding the right process or system that brings out talent. To the Producer, publishing papers is evidence that the system is working. But one or two successful papers are not enough—no matter how impactful—to solidify a pattern of high achievement. Groundbreaking research is not necessarily the goal; it could be considered a byproduct of the interaction between talent and empowering conditions mediated by the Producer.

For the Producer, research is always a collaborative effort. Truth is to be witnessed and nurtured in the context of relationships and systems. Thus, if a project fails or stumbles in some way, it is not necessarily the case that the idea was not good; it could simply reveal some flaw in the relationships or the sys-

tem. Maybe poor communication needs to be fixed to move forward. Maybe new resources need to be brought in. Perhaps the workflow and responsibilities need to change in some way.

The Problem Solver

The Problem Solver is motivated by intellectual challenge. Why do people want to climb Mount Everest? The stock answer is: “Because it’s there”. It represents a challenge to the human spirit. Others may have gotten to the top before, but everyone who does so gets tested. The Problem Solver enjoys the process of pushing their limits. It would be nice if the problem that challenges them is important or novel in some way, but this is not always necessary. In some cases, the problem is simply a means to apply themselves.

The Problem Solver approaches research head first. If a path towards progress is closed, they open another one. The Problem Solver is tireless in considering new directions and new angles. Questions and doubts must be fought back; optimism drives a prolonged search. The real victory is overcoming your doubts.

The Problem Solver views setbacks as evidence that the problem is worthy of their effort. Without setbacks, a problem presents no challenge. When someone asked Albert Einstein how he could solve so many intractable problems, he replied: “It’s not that I’m so smart, it’s just that I stay with problems longer.” For a Problem Solver, it can sometimes be that truth bends to will. And a will that is not tested is a will that is not strong.

The Student

The primary motivation of the Student is to learn and to know. The Student runs out of classes to take before they feel they are done learning what’s out there. Research allows them to make their “own” classes by reading new papers and learning new tools. It would be nice to discover something new, but even if the Student only ever learned more and more about what was already known, they would be content. What haunts them is the feeling that they don’t know something that they maybe should.

The Student approaches research as a venue to apply their existing knowledge. When presented with something new, the Student tries to classify it as something known. That gives them a sense of accomplishment and that all

is right with the world. Research makes things tidy and complete, and that is soothing to the Student.

For the Student, a setback in research is a double-edged sword. On the one hand, it shows them that there are limits to their knowledge, exposing them to the fear of not knowing what they should know. On the other hand, the Student can take a minor hiccup in a research project to justify reading a whole book. Learning the book is probably of greater interest to the Student than solving the problem at hand. The research project is the chore; the reward is the opportunity to learn.

When the Student faces a challenge in research, one of their first steps is to search for answers in published books and papers. The Student feels there is a good chance that the answer must be “out there”. Unlike the Problem Solver, who looks inside for more strength and more ideas, the Student looks externally for teachers and lessons to guide their way forward.

4.2 Variety within orientations

We do not intend to make the orientations appear monolithic or rigid. We admit that their descriptions in the previous section were written in broad strokes. Within a given orientation, we expect much variation.

In this section, we explore variation in the following four dimensions: (i) outcome- versus process-driven, (ii) outward- versus inward-looking, (iii) risk-averse versus risk-seeking, and (iv) topic- versus relationship-driven. Again, these distinctions are not meant to be dichotomous; a researcher can be “risk neutral” by balancing risk-aversion and risk-seeking behavior, and so on. We reiterate that the goal of discussing these distinctions is not to classify or rank but to allow for a rich discussion of the variety of possible approaches.

Outcome versus process focus

First, let’s discuss a researcher’s tendency toward outcome or process. Someone who is outcome focused cares deeply about the final destination of the research journey: discovering something new, publishing a paper, getting a job, or getting tenure. For someone highly oriented towards outcomes, “the ends justify the means”. For someone focused on process, the journey matters more than the destination. Joy in research comes from thinking, meeting, and

documenting more than the final discovery or end product. Someone with a process orientation will be delighted when a paper gets published, but possibly more as a memento of the journey rather than the purpose of the journey itself.

This comparison between outcome focus and process focus gains more richness when we add the lens of the different orientations. It seems natural to think of an outcome-focused Discoverer. For the Discoverer, the purpose of research is to discover something new. One ground-breaking result can justify years of struggle. If this endpoint (genuine discovery) is not reached, the Discoverer may be disappointed or even disillusioned. Is it possible to have a process-focused Discoverer? Although it may seem less natural, this is not implausible. A Discoverer can dedicate themselves to a particular methodology or perspective. An exciting discovery serves to validate the Discoverer's process. Ultimately, for a process-focused Discoverer, it is attachment to validating a given process that motivates their search.

Similarly, for the Promoter, the end goal of research is to change minds. For an outcome-focused Promoter, failure to do so makes the Promoter question the value of their work. For a process-focused Promoter, resistance to accepting their ideas could be interpreted as a failure in the methods for persuasion, the perfection of which might be construed as the ultimate purpose.

For the Student, research provides an opportunity to learn and gain more knowledge. The goal of knowledge acquisition is never-ending. There is always more knowledge to gain due to the long history of human discovery and the fact that new knowledge is constantly being created. In this sense, the Student's journey has no end or final outcome. However, a Student could nonetheless be somewhat outcome-oriented, particularly if they view gaining knowledge of certain prescribed areas of knowledge as "make or break" for their sense of worth.

The Producer orientation also seems to lend itself to process. As we discussed already, for the Producer, one or two successful outcomes—like a ground-breaking discovery or a well-cited paper—may not be their end goal and does not justify their endeavor. The question remains whether the next interaction with a researcher or group of researchers produces success. A Producer who only succeeds with specific collaborators has not perfected their production process. But an outcome-focused Producer is, of course, possible. This may arise when the Producer has their own research vision and views the participants as a means to achieve that vision. The outcome-focused Producer

acknowledges the need for a team and a process but only finds justification for them in the ultimate success of their own large-scale research program.

An outcome-focused Artisan is naturally drawn to finishing projects and creating stability in their career through consistent output. On the other hand, a process-focused Artisan takes their craft and method seriously, regardless of the outcome. An outcome-focused Problem Solver can be slavishly devoted to “solution” at all costs. A process-oriented Problem Solver cares about the meta-game of developing skills in solving problem after problem. A string of solved problems is the result of an accomplished problem-solving process. Indeed, a Problem Solver who has only ever solved one problem—regardless of its importance or difficulty—may have gotten lucky and could easily get stumped on the next problem.

Outward looking vs. inward looking

Researchers can also differ in outward-looking or inward-looking perspectives on their work. By outward-looking, we mean that they are motivated by external validation or distinction. The researcher is motivated, for example, to be accepted into the research community not just because it is necessary for success in an academic career but because they seek personal acknowledgment of their talents by that group of people. An inward-looking perspective gravitates towards enrichment, personal challenge, and self-satisfaction as their own rewards. For an inward-looking researcher, being accepted into the academic community is only a necessary step in their research career.

This distinction between outward-looking and inward-looking perspectives (sometimes called *extrinsic motivation* and *intrinsic motivation*) differs but is related to our earlier discussion of outcome and process orientations in the previous section. For example, someone can be outcome-focused but nonetheless have an inward-looking orientation. They want to produce results for their own edification and are possibly indifferent to what others think of those results.

One orientation that seems to embody an outward-looking perspective is the Promoter. The Promoter is naturally focused on others, seeing that their ultimate goal is to change minds. An outward-looking Promoter seeks validation and thrives on feedback from those they are conversing with. Of course, an inward-looking Promoter is nonetheless possible. Changing others' minds is indeed an immense personal challenge; an inward-looking Promoter might

want to prove to themselves that their ideas can be made to shape the thoughts of others, even if they are not driven by gaining the admiration of others.

For the Student, an inward-looking perspective is quite natural. The desire to learn comes from an internal curiosity and to develop an ability to embrace the world's knowledge fully. But an outward-looking Student is also possible. For such a Student, the thirst for knowledge is partially driven by a fear of rejection or being "left out" of conversations they want to be a part of due to a lack of preliminary knowledge.

As for the Artisan, the pattern of an outward-looking perspective versus an inward-looking perspective closely follows our discussion in the last subsection. An outward-looking Artisan focuses on the work as a means for reward, while an inward-looking Artisan focuses on the sanctity of the craft as their source of pride. Similarly, for the Problem Solver, an outward-looking perspective would focus more on solving problems as providing evidence for the acknowledgment of their talent by others. By contrast, an inward-looking Problem Solver is driven more by personal triumph.

Risk averse versus risk seeking

Another dimension we consider is a researcher's risk profile. A risk-seeking researcher considers directions with a high potential for discovery or impact on the research community but could also "flop" if the necessary levels of luck, hard work, and inspiration (in some mixture) are not involved. In terms of our path analogy, a risk seeker is more likely to go onto "untraveled trails" that may lead to new places, but could also be perilous.

By contrast, a risk-averse researcher travels more familiar paths and takes on projects with more dependable prospects for success. The trade-off is that risk-averse paths are more likely to produce the familiar. Too much familiarity makes it hard to stand out and gain acceptance from the community.

The orientation most easy to associate with risk-seeking behavior is the Discoverer. Indeed, the Discoverer is motivated by tackling the unknown, which necessarily involves going into places with greater uncertainty and more risk. However, a somewhat risk-averse Discoverer is not inconceivable. Someone could be motivated by discovering something new but travels far down well-trodden paths before venturing afield. The risk-averse Discoverer may be drawn to the unknown in terms of motivation, but in practice, only ever ventures a short distance off trodden paths out of a sense of fear.

On the other end of the spectrum is the Student. It seems easiest to associate the Student with risk aversion. The Student is more drawn to knowing than discovering, so worn paths seem the natural home of the Student. However, sometimes the Student can become ambitious in the sheer number of worn paths they hope to traverse. A risk-seeking Student may not be that drawn to the unknown but can take risks in attempting to learn a wide variety of deep topics.

The Artisan appears to follow an analogous pattern to the Student. Because the Artisan focuses more on the craft of research—not being in search of a breakthrough or making a “big splash”—it seems more natural to associate them with risk aversion. If you view publishing papers more as your job than some God-given mission or calling, why take chances with potentially overly ambitious projects? However, it would not be unimaginable to come across a risk-seeking Artisan. When an Artisan becomes comfortable with their craft and their career trajectory is relatively stable, they may consider taking on more ambitious projects, not necessarily to discover something new, but to test their own mastery of the research endeavor. For every nine chairs a carpenter makes, she might try one or two wood sculptures to push the limits of her skills.

For the Promoter, whose goal is to change minds, it is not clear that something entirely new and daring is the best vehicle. There is often a small potential audience for something too different. Therefore, exploring a dramatically new direction, with its associated risks, may not pay off for the Promoter. On the other hand, work that is “too safe” and clearly derivative in some way also has a small potential audience. Can you really have said to change the mind of another when the change is only superficial? A risk-averse Promoter prefers to stick to more familiar territory but possibly present it in a new light or to new audiences to reduce the chance of it being considered derivative. A risk-seeking Promoter seeks a “big splash” to dramatically change the viewpoints of others and is willing to take on the associated risks.

Finally, the Producer walks a delicate balance between unleashing the full creative freedoms of his team and keeping in mind that the work must meet certain standards and expectations. Every successful musical album has at least a few “hits”, which are meant to have broad appeal, and likely a few “album cuts” that allow the artists to push their boundaries. The risk-averse Producer steers their team more toward more standard output with a more predictable reception. In contrast, a risk-seeking Producer may be more fasci-

nated with the potential and unbridled creativity of their team. The latter has some chance of being “genre-defining” and influential but also has a heightened chance of reaching potentially no audience at all.

Topic-driven versus relationship-driven

A final source of variation we consider relates to how researchers decide among research directions—whether it is driven more by interest in the topic or based on their relationship to others. A topic-driven researcher follows a trail of ideas and hunches, even if that trail leads toward or away from people. By contrast, a relationship-driven researcher finds comfort in the company of others and, when deciding what path or problem to tackle next, prefers to get a sense of who is going with them before stepping out.

It is important to stress that this distinction between a topic-driven and relationship-driven approach is related—but not identical—to some of the distinctions made in previous sections. For instance, a researcher can be inward-looking and relationship-driven simultaneously: their choices in what to study are influenced by who else is willing to explore them, but they are not necessarily motivated by impressing or getting validation from these people. They look to follow a path that interests them but in the company of others.

You might instinctively associate three orientations with a topic-driven approach: the Discoverer, the Problem Solver, and the Student. In the case of the Discoverer, they focus on uncovering the truth, and the truth is not in need of friends. When you think of many of the world’s great discoverers, their triumphs are often cast as individual journeys. Newton was by himself when the apple fell on his head; Einstein was a lone and idiosyncratic genius. The Problem Solver is easy to cast in a similar light. You solve a Rubik’s cube in teams. It is also easy to conceive of the best friend of the Student being books and papers.

However, as is a common theme in this section, there are equally valid (if less romanticized) ways to understand the motivation of these three orientations. The Discoverer may be motivated by uncovering the truth, but their choice of where to search for it is highly influenced by people they know. The Discoverer may be interested in discovery but drawn especially to discoveries in their neighbors’ backyards. Similarly, a Problem Solver may tackle a problem individually but wants to devote their energies to problems identified as being important or difficult by others. Their choice of problem, then,

is an outcome of their relationships. Finally, as discussed above, a Student may be driven to gain knowledge in certain areas out of a desire to belong to a group, not entirely out of their thirst for knowledge or deep curiosity for a given topic.

The Promoter and Producer are easier to associate with being relationship-driven. The Promoter desires to change minds and be part of a conversation, which necessarily brings them into relationships. A good Promoter is aware of their audience and “works backward” from what would interest their audience, naturally giving rise to a relationship-driven choice of topics. Similarly, since the goal of the Producer is to get the best out of those they work with, it would be natural to work on the ideas that excite these people, leading naturally to a relationship-driven approach. However, topic-driven Promoters and Producers are also conceivable. A topic-driven Promoter may have a fascination with a given topic that they believe *should* be appreciated by others. This topic-driven Promoter “works forwards”: given their topic of interest, they think about how others can use this topic to change minds. A topic-driven Producer has a clear topic in mind but must assemble a team to execute it. The analogy here is to music producer Lou Pearlman—the creator of the Backstreet Boys and N’SYNC—who envisioned the success of the boy-band concept in the 1990s and needed performers to fill the roles.

Finally, the Artisan seems difficult to naturally associate with being either topic-driven or relationship-driven. The Artisan does research as a craft and as a job that provides for themselves, their families, and possibly their co-authors and their families. This motivation speaks little to the choice of research direction—the Artisan works on whatever project is in front of them, as much out of duty to the craft as out of interest in the project itself. Because the Artisan views their work in down-to-earth terms (a paper is like a chair that only a few people may ever sit on), the choice of research direction is arguably downplayed more than for any other orientation.

4.3 Putting it all together

As a form of summary, we provide an example of young researchers exploring the ideas in this chapter. The story is fictional but inspired by real-life interactions with many young researchers over the years. The main purpose is to provide you with an example of how natural it can be to discuss your approach to research

with those you trust.

As a child, Amani had a deep curiosity for the world. Her two favorite objects were engines and insects. In school, Amani was always a high performer, getting into an engineering program at a well-known college. When she first entered her program, she enjoyed the material she was learning but always maintained her curiosity about the limits of what she was learning. She continued to read and explore on her own outside of classes. However, as a conscientious student, she developed a reticence for indulging her curiosity beyond what she was required to learn to do well in her exams.

Over time, Amani developed a joy for mastering her coursework and exceeding expectations in exams. In Amani's last year of her undergraduate degree, she started to think about what she wanted to do after graduating. She loved learning and the joy of mastery she got from courses, but she also felt it might be an opportunity for her to finally indulge her curiosity in things unknown. She applied and got accepted into a good Ph.D. program.

This was when some of her uncertainties and hesitancy began. Based on her undergraduate success, Amani had developed an orientation you could describe as an outcome-oriented and risk-averse Student. She had become focused on getting high grades, so she had shied away from courses that interested her but had a reputation for strict grading. This was Amani's practical orientation. But in her imagination and ideals, she was something entirely different. Deep down, she was an inward-looking and topic-driven Discoverer who still had unresolved curiosities since childhood. Her struggle was finding a way to balance her practical orientation and idealistic orientation.

She found her coursework bittersweet for the first two years of her Ph.D. program. She continued to do well in courses but could not seem to connect them to her long-term curiosities. She was reluctant to start a research project that strayed too far from what she felt comfortable with from her coursework, but this felt woefully far from where her curiosity took her.

Luckily, Amani developed a friendship with another Ph.D. student, Farhad. Farhad would best be described as a risk-seeking and relationship-driven Problem Solver. He was attracted to tackling problems beyond his reach but needed direction on the right problem to tackle. His greatest fear was to be alone with too many problems to decide between. Luckily, both Amani and Farhad were assigned cubicles next to each other in the Ph.D. office. They would often share their experiences and challenges in doing meaningful research.

Over time, Amani convinced Farhad that it would be an exciting and chal-

lenging problem for him to bridge what they were learning in their coursework with some ideas she had been brewing. Amani and Farhad started working together. Through the process, Amani discovered she had some talent for bringing the best out of Farhad. Farhad would remark how he could work longer and harder with her encouragement and curiosity driving him. Because Amani was so thorough in her coursework, she could understand and clarify Farhad's problem-solving attempts. Through the process, Amani took on a third orientation, a process-oriented Producer who could keep their joint projects organized and moving forward. Together, Amani and Farhad brought her vision to life through a well-received paper. Amani drew on aspects of four orientations—the Student, the Discoverer, and the Producer in herself, and the Problem Solver in Farhad—to finally reach her goal of being a published researcher.

4.4 Reflection questions

The questions below are provided for you to reflect on what you have read in this chapter. If possible, it is beneficial to discuss these questions in a group of your peers.

1. Which of the six research orientations (the Discoverer, the Promoter, the Artisan, the Producer, the Problem Solver, and the Student) do you resonate with? [There is most likely more than one you resonate with].
2. How, if at all, has your research orientation changed over time? What events or realizations led to these changes?
3. Are there aspects of your orientation to research not captured well by the six research orientation orientations? If so, what are they?
4. Do you consider yourself to be more outcome- or process-oriented? How do you know? Does it depend on the situation?
5. Do you consider yourself to be more outward-looking or inward-looking? How do you know? Does it depend on the situation?
6. Do you consider yourself to be more risk-averse or risk-seeking? How do you know? Does it depend on the situation?

7. Do you consider yourself to be more topic-driven or relationship-driven?
How do you know? Does it depend on the situation?
8. In what way, if at all, do you relate to the story of Amani and Farshad?
What do you take away from the story?

Chapter 5

Research capabilities

This is the last chapter of three that introduces our conceptual framework for research as a career. Here, we focus on building a coherent model for describing the various capabilities involved in undertaking research. So far, the emphasis has been on the nature of beauty in research (Chapter 3) and the orientations of the researcher (Chapter 4). The focus from now until the close of this book is on the acts involved in doing research as a career.

We discuss the research task in terms of capabilities. A *capability* is a capacity to think and act in a well-defined way and according to a well-defined purpose. The next part of the book (Part III) describes six distinct research capabilities – reading, listening, creating, writing, speaking, and collaborating. The previous chapters have provided you with many thoughts about the purpose of doing research, but we need to place this purpose in the context of these more concrete research capabilities.

5.1 Defining capability

Let's start our investigation with a careful definition of *capability*. As we just said above, a capability can be defined as a capacity to think and act according to a well-defined and motivating purpose. To understand what this definition means, you may examine an example of what we would consider *not* to be a capability.

Consider a child who is learning mathematics. The child is presented with the notion of addition and is told the mechanics of adding two num-

bers. Then, they are told to mimic this act of adding two numbers ...over and over again. Thus, you can see many children “acting” addition.

It may be somewhat straightforward to ask someone to mimic the act of adding numbers and remembering how to “carry the 1” (although even this is easier said than done). But learning this mimicry is insufficient for the child to “think” in terms of addition. Many students have difficulty with addition word problems. There is a gap between knowing the mechanics of addition and thinking about a seemingly unrelated situation in terms of the notion of addition.

Even more challenging to get across to children is the purpose of learning addition. You can tell them stories about adding up the price of apples and oranges or other contrived examples, but children often have difficulty relating. These examples seem made up because ...they are. It can sometimes take children years to learn the purpose of addition beyond making their teacher and parents happy. Some never learn.

Acting and thinking are distinct, and neither is very meaningful if done for a shallow purpose. Our educational systems are most adept at developing students who can “act” the part. Mimicry is highly rewarded. Teachers show the students concrete skills and examine students on their precise execution. Of course, some teachers are adept at encouraging thought, but such encouragement often has limitations. Deep thought can lead to doubt and questioning authority. To manage the classroom, teachers might unconsciously limit the range of ideas open to the student.

It is the teaching of purpose that is most often neglected in traditional modes of education. Returning to the example of learning mathematics, children know that the most apparent reasons to excel in school are to get praise and attention from teachers and parents, impress classmates, and secure better future education and work. These traditional notions of purpose provide minimal comfort to a student who is struggling. A purpose driven by the promise of “good outcomes” (like praise and future success) can seem out of reach for someone scraping by and thus can even become demotivating.

To address purpose, we need to go beyond acting and thinking. We need to consider character and belief. Developing a deep sense of purpose requires an examination of your own nature and worldview. Accordingly, we will examine qualities of character (things like honesty, patience, etc.) and attitudes (beliefs about one’s self and the world) as crucial building blocks of capabilities.

Research is not easy

“Research” cannot be captured as one giant capability. Research is a complex amalgam of many component capabilities. Research is not easy. It takes years to develop a facility in research and a lifetime to improve.

When faced with such complexity, it is tempting to address it by discussing collections of skills and bodies of knowledge that need to be absorbed to succeed. Such an exploration is highly valuable and is pursued in courses in Ph.D. programs worldwide. This chapter, and indeed this whole book, is not aimed at replacing such an exploration.

But we must all acknowledge that skills are insufficient to guarantee success. It is easy to become distracted, confused, unmotivated, and sometimes depressed. Someone with immense intellectual talents needs patience to wield their skills. Someone with both talent and patience must believe it is worth investing these talents in risky research projects that may either define a field or fall flat, depending on the whims of a capricious audience.

The strongest value-add of this chapter (and the remainder of the book) will be in our discussion of qualities and attitudes. We will not ignore questions of skill development (in part because it is impossible to talk about attitudes without constructing some base of skills to develop beliefs about), but we will not be content with providing a list of skills or practices that we claim lead to success.

It is also not as if qualities and attitudes are never examined by students and mentors of research. However, qualities and attitudes are most often dealt with unconsciously or implicitly. Probably the most discussed quality is one that we do not even emphasize in these pages: “being smart”.

We have met very few students who felt adequate in terms of “smartness”. The lack of “smarts” is a common reason students give themselves for why they are struggling. Certainly, both of us (Chris and Runshan) have never felt adequate in terms of being “smart”. There have been thousands of days we woke up wishing we had someone else’s much more clever brain. Over time, however, we learned this fixation on being smarter was largely a distraction, or worse, an excuse. There are many other human qualities that you can learn and develop that will help you succeed. We try to focus on those.

As for attitudes, these are often learned by osmosis. You learn beliefs in application. A good example is when a student regularly attends a research seminar in their department and notices that no one ever seems excited or in-

spired by the talks that are given. Instead, the attendees raise many doubts in their questioning or privately complain about the speaker's lack of depth, creativity, or preparedness after the talk. This builds a strong belief in the student's mind that most research is bad and is set up for negative scrutiny. This belief breeds a tendency to take fewer risks out of fear of facing public ridicule.

Like in our research aesthetics and orientations discussions, we are not here to promote certain qualities or attitudes over others. Just as people have different skills, they also have different qualities and attitudes. It is unnecessary (and probably impossible) for someone to possess all of the qualities that people admire. We aim to discuss some qualities and attitudes we have found (in ourselves and others) that help or hinder the research process.

5.2 Skills

There is no debate that skills are critical for developing research capabilities, so it is worthwhile to deliberate on them here. We will not belabor a careful definition here; the common-sense definition of "skill" in your mind will likely suffice. It is worth stressing the distinction between skills and capabilities as a whole. We are thinking about skills as distinct acts or approaches that are, in themselves, devoid of much direction or purpose. For example, reading the English language is a skill. Reading and interpreting a poem is closer to a capability because it goes beyond mere language into the making of meaning.

It is worth stressing here that not all skills are learned formally. There are *de facto* skills and knowledge that are most likely not written about anywhere. These are passed down from mentor to mentee, often orally. Indeed, academic training at the Ph.D. level is designed as an apprenticeship. Much of a researcher's training happens in the context of a relationship with an advisor in response to challenges that arise.

A good example is the skills related to responding to negative reviews after submitting to a journal, particularly when a paper is rejected. Dealing with rejection and negativity is not often discussed in books or research courses, partly because of the hurt feelings that discussing rejection can arise. Researchers may want to avoid publicly discussing their past "failures".

I (Chris) had a paper rejection that I was not prepared for in my training to that point in my career. This rejection nearly caused me to drop a whole promising research direction out of fear of future rejections. Looking back

now, the rejection wasn't actually that bad! I have had far worse since then. Over time I developed the skill of quickly retooling and sending the paper on to the next outlet without taking on too much of the negativity of the reviewers. I learned the skill of differentiating problems with my paper that need fixing and the negative impressions of someone who does not like my paper that cannot really be addressed.

No amount can be written to replace this apprenticeship model of training, nor would that even be desirable. However, we have endeavored in these pages, particularly in **Part III**, to provide thoughts on skills that might otherwise only be shared from person to person.

5.3 Qualities

Qualities are those “good” dimensions of your character that you learn about when you are young and spend the rest of your days trying to live up to (or not). A fairly comprehensive list is found in **Table 5.1**.¹ We will not venture to carefully define all of these qualities, so we suggest those who are unfamiliar to look them up online.

Some may see these qualities and assume each comes in fixed quantities for every person. For example, some people are inherently patient, while others are impatient. While you may agree that many of your qualities are formed when you are young, we believe it is possible to practice patience (for example) and become more adept at it. Just like skills, some qualities come more naturally than others. But, just like skills, you can improve your qualities with attention and practice.

As we explored in the context of aesthetics in **Chapter 3**, tensions between the qualities make their expression a balancing act. For example, it is hard to be both flexible and persistent. Flexibility suggests a willingness to change direction; persistence suggests sticking with a direction. When to be flexible and when to be persistent is an interesting tension that all of us have faced. The construct of a capability puts the qualities in a context that helps you

¹Many of these “qualities” are taken from the list of virtues found on the Virtues project website <https://www.virtuesproject.com> that I (Chris) came across when I was teaching spiritual education classes for children while an undergraduate student. I was surprised when the same content turned up in my son's Montessori public school curriculum some 15 years later.

Category	Qualities
(i) Sense of self-worth	confidence, courage, humility
(ii) Feelings towards others	curiosity, empathy, gratitude, wonder
(iii) Interacting with others	assertiveness, reliability, respect, trust, unity
(iv) Facing challenges	detachment, forgiveness, hope, joy
(v) Self-discipline	flexibility, orderliness, persistence, patience
(vi) Coherence	honesty, integrity, justice, and sincerity

Table 5.1: Qualities relevant to research

make sense of these tensions.

We are not here to speak moralistically about how important these qualities are in some vacuum. Each of the qualities we discuss is neither necessary nor sufficient for success in research as a career. We want to explore them in analyzing research capabilities and emphasize how they can be helpful to concepts in the journey of improving yourself as a researcher. We treat these qualities as means to achieving success in research as a career, not as ends in themselves (although such a pursuit seems nonetheless admirable, just not our interest of this book).

Categories of qualities

We could delineate many human qualities, but we focus on a relatively small collection we felt was most pertinent to research. We organize them into six rough categories: (i) sense of self-worth, (ii) feelings towards others, (iii) interacting with others, (iv) facing challenges, (v) self-discipline, and (vi) coherence.

Regarding (i), a sense of self-worth refers to how you view your own talents. This serves as a foundation for the other qualities that follow, as they contribute to how you face the world around you.

As for (ii), when you develop positive and constructive feelings towards others, you have natural reasons to build connections and bonds. This creates more opportunities and openness for learning, exchange, and collaboration.

When you interact with others in a research context (category (iii)), qualities such as assertiveness, respect, and trust allow you to get more out of the interactions. Solid qualities in this category may lead to constructive and long-

term relationships that can be the foundation of a successful research career.

Of course, research is full of setbacks, including conflicts with other researchers. Category (iv) concerns qualities that help you deal with challenges that arise in a way that does not stifle progress or extinguish optimism.

“Detachment” is a less-discussed quality in this category, so we will comment on it briefly here. When you are detached, you can “step back” from your failures (or even past successes) and put them in a context that helps you move forward instead of “getting stuck” by overly associating yourself with the fruits of your work. We find this to be particularly important in research. You may pour a lot of effort, and even your identity, into your work. In this case, a rejection of a paper can feel like a personal attack on your reputation. Detachment, although difficult in this situation, is important for moving on and either resubmitting or jettisoning the paper.

The fifth category (v) of attributes is important to every job that entails long-term, self-directed projects. These are the qualities of self-discipline, like orderliness, persistence, and patience. In academic research, there is typically no “boss” telling you what to do, at least not in fine detail. Daily progress relies very much on your determination and persistence.

Finally, we call the sixth category (vi) of qualities “coherence” because they relate to how your work resonates with your values and character. Is what you emphasize in your research coherent with your views of yourself? Are you honest with your readers and yourself about the limitations of your work? Do you have the integrity to follow your original research design even when the results are not what you expect? These questions of coherence are important when you think about research because sacrificing your coherence to “succeed” can sometimes lead to hollow or shallow victories later in your career.

Six prevalent qualities in academic research

We explore six research capabilities across six chapters in **Part III**. In those chapters, we refer to many of the qualities in **Table 5.1**. However, six qualities showed up again and again in our reflections. Due to their prevalence, we thought we would discuss them in some depth here to illustrate the general importance of qualities in research.

Patience

Research cannot be rushed. This is a simple statement with many dimensions. First, understanding something new cannot be rushed. You cannot pretend that you know; you must actually know. Pretending to know can only build a house of cards that collapses when you most need shelter. Going from pretending to actually knowing takes effort and sweat. Patience is essential to true understanding.

But understanding is insufficient for research success. You must be able to explain what you have understood to others. But this explanation cannot be rushed. Nothing is more frustrating than talking to someone who explains something to you in a way you do not understand, but they have no other way to explain it. Their response to your confusion is to give the same explanation, either faster or slower, depending on their level of patience. To explain yourself (in a paper, presentation, or even in a research meeting with some collaborators), you may have to revisit your ideas from many angles and illuminate them in different ways. All of this takes patience.

Finally, it is not enough to understand what is known and explain what is known to others. In research, you have to discover or invent something new. This requires creativity, and creativity is famously allergic to scheduling. No formula guarantees novelty. You need the patience to be there when creativity eventually arrives. Sometimes, this wait can seem like forever.

In my (Runshan's) experience, it is not uncommon to restart a project completely, even multiple times. Sometimes we spent months developing a model and later realized that the model did not fit the research purpose. Sometimes a discovery in the data would completely invalidate our previous analytical results. What I learned from my co-authors is that the best way to respond to a need to restart a project was simply to embrace it. Research is not a linear process. Sometimes it may feel that you are not making progress for a long time, but in reality, all the discoveries, including those seemingly useless ones, are essential. This work is impossible without patience.

I'm also reminded of the first few times I shared my research findings with others. I thought I was being perfectly clear, but others were apparently very confused. Initially, I got frustrated and annoyed. I couldn't see how they could possibly not understand my findings. Later, I realized that I had been too familiar with my own analysis. I ignored that my audience did not have the same context as me. After I learned from other people's perspectives, I

gradually realized what I had missed in the explanation. It takes non-trivial effort to communicate with others, especially about complex ideas, models, and analyses. All of this takes patience. It all goes slower than we hope.

Curiosity

But what is the source of all of this patience? When you start a project, you may have a lot of energy to work out details, find explanations, and push for more discoveries. After your paper has been rejected a couple of times, and you need to revise it a fifth time to send it out, initial excitement fades. What gives you the energy to patiently revise it one more time?

One quality that plays a role here is curiosity. The power of curiosity is the question: “Why?” Curiosity goes beyond excitement; it goes to deeper explanations; it wants things to be clearer and clearer; it wants to discover the “essence of essences.” Why, why, why, why. By awakening your curiosity, you see a paper’s rejection as another puzzle to solve: “Why is this work not resonating with the reviewer? What aspect of my paper did I not yet polish enough for others to see its beauty?”

I (Chris) am often fascinated by the curiosity of the ancient Greeks like Plato and Aristotle. Their curiosity about the world leads them to ponder the profound and mundane alike. One of my favorite examples of this thought is the Platonic Ideas. Every horse you see is a mere imperfect representative of the Idea of a Horse, perfect in its essence. Thus, no matter how much you ponder a given horse—study its nature and conditions—there is yet more to understand about the Horse. Curiosity about the Horse can never be exhausted because a perfect understanding of the Horse is impossible.

By analogy, each research paper can be likened to an imperfect representative of the Idea of a Paper. You are searching for greater and greater perfection in your papers, but you will never fully comprehend the ideal Paper. This yields an endless fountain of curiosity about the profound nature of research papers.

Humility

But this drive towards perfection fueled by curiosity leads to its own challenges. Indeed, one of the frustrating things about research is that it is never done. It would be nice if, for only a while, you could rest on your laurels and

not have to come up with something new. But, alas, there is always more to learn and more papers to write.

Humility helps you face the reality of the unending and unrelenting nature of progress in research. You are humble when you accept your current reality and recognize that you can always improve. When you have pride, you find yourself hiding. Full of pride, it is painful to learn of new vistas that challenge your current understanding. When you are humble, you accept new and challenging ideas as a natural outcome of your limited understanding. In other words, humility allows you to learn and to always be on the lookout for paths of improvement. Confucius said it best: “三人行，必有我师。”² There is no perfect English translation, but a reasonable one is: “When you talk with two others, at least one is your teacher.”

Our understanding of humility does not include the implication that some draw that humble people are shy and unwilling to show their strength. In our understanding, when someone is humble, they are honest with themselves and others about what exactly are their strengths and weaknesses. They are happy to share their talents when appropriate but not expecting praise for those talents. They can recognize, champion, and learn from the talents of others.

An interesting application of the quality of humility can be seen in how someone approaches giving an academic talk. How can you give your talk confidently? One approach is to prepare for a long time, make sure everything is perfect, and anticipate (and have a good answer) for every question. When someone has done this full (and admirable) preparation, they have earned a high degree of confidence. However, this confidence can be shaken by an errant question from the audience or a technical difficulty that messes up the slides. How can someone be confident even under these conditions?

Consider what happens when you approach the talk from a stance of humility. Someone who is humble also prepares the talk and thinks about the questions, but they do not fear being wrong or making a mistake. When you are humble, you accept your limitations and mistakes, so every opportunity is an opportunity to improve. You can be confident because you have nothing to hide.

²This quote is from Confucius's *Analects*. For a popular translation into English see [25].

Courage

Humility may give you confidence, but sometimes you need more than confidence; you need courage. Courage not only overcomes a fear of being wrong, but it also gives you the energy to show others you are right. You may have thought to yourself:

Who am I to be a part of this research community? I see all of the accomplishments and accolades of the people who came before me, and I am ...well, me.

It takes courage to stand up and say that you belong to the same community as the greats and even more courage to knock on the door to ask to be invited in.

There are many dimensions to courage. It takes courage to jump out of an airplane with a parachute. It takes courage to give a plenary talk at a large conference. But we want to emphasize three dimensions of courage that show up in the daily life of the working researcher.

First, it takes courage to doubt. When you read a beautifully written and well-published paper, it can be hard to give yourself the courage to doubt its explanations, even when you do not fully follow them. You may pretend you understand and not allow yourself to doubt. This is all the more common when someone is making an argument in front of you in person. It is far easier to pretend you understand what they are talking about.

When you doubt, you risk the possibility of saying something naïve that makes you look less than prepared. However, if you never doubt, you can fall into the trap of only pretending you understand things and never taking the time to actually understand things (a peril discussed earlier in this chapter). Doubt, not imitation, is on the path to certitude.

Conversely, it takes courage to believe. It is also easy to get into the mode where you doubt everything. A big part of our jobs as academics is fishing out and exposing bogus claims. But this can also be taken to an extreme when you can only find doubt and problems with everything you see. If you don't believe in anything you see, you only ever have shaky ground to stand on. It reminds me (Chris) of my time in high school when it was safer to say that you hated every song you heard than to claim you liked something. The risk of liking something was that thing could be made a target of ridicule. In my high school, it took courage to believe in things.

Lacking the courage to believe is especially problematic when it comes to your own work. Unable to summon the energy to believe even your own ideas, you can waste months in unproductive, isolated stupors of stalled research. This ultimately relates to maybe the most important dimension of courage: the courage to be yourself. We spent a long time in previous chapters emphasizing the importance of audience in establishing a successful research career. Success in research does not follow the formula of “just being yourself and doing what interests you”. However, there needs to be enough of yourself in your work to bring it to life. Putting yourself into your work takes courage because others may reject or minimize it.

I (Chris) have had so many papers rejected by now that I have gotten used to it. But I would be lying if I said it still doesn't sting. A little piece of me dies when my paper dies at the hands of the reviewers. If I did not dare to put anything of myself into my papers, I would maybe feel the sting of rejection a little less, but then I am unsure if acceptance would even be worth it. If my work was pure imitation and not authentic to me in at least some way, it would all feel quite hollow.

Hope

As we laid out throughout this book, you need others to *accept* you to succeed. This leads very naturally to our next quality: hope. You are doomed if you have no hope that others will accept you under any realistic circumstance. Engendering real hope in your heart is an essential quality for undertaking a career in research.

Hope is a vehicle at every stage of the research process. Without hope, incremental steps in research feel meaningless. With hope, no step is too small or unimportant. “I woke up today and read the introduction of a paper before breakfast.” Yes! “I sat through a talk without falling asleep.” Yes! With hope, these are important stepping stones. Without hope, these may even sound pathetic.

We should differentiate true hope from the delusion of false hope. We are not here to advocate a false hope that denies an often harsh reality. But it is unfair to ascribe every aspirational feeling of hope to be false hope. Hope is something to practice and develop, even when you face setbacks. Indeed, hope is most useful when you face setbacks.

One characteristic of academia is the sparsity of positive feedback. It may

take years to publish a paper, and it is common to get multiple rejections and criticisms before the paper is finally accepted. Most paper reviews consist of lists of things the reviewers *do not* like about your paper. Very little space is given to what people like.

I (Runshan) have experienced major setbacks in all the projects I have worked on. The hope that I would one day, and in some way, make interesting and important discoveries helped me through periods of heavy negative feedback. The fuel of hope kept me working.

I (Chris) typically work under the hopeful assumption that the top outlet in an area will consider my papers until I am proven otherwise (typically through rejection). A few (okay, many) rejections do not evaporate my hope that my next project may be considered at the top outlet. As I keep trying, I learn better how to place my papers. My hope grows as I learn more.

Empathy

Empathy means that you can feel what others feel. When they are confused, you can sense their confusion. When others are excited, you can be lifted by their excitement. At first glance, empathy may seem to have little to do with research. However, understanding and resonating with how others feel is very useful. When you understand and feel the confusion of others, you quickly learn if your arguments are clear or unclear. When you sense the excitement in others, you can quickly learn what aspects of your research are resonating.

When you are weak in empathy, you more easily find yourself misunderstood and underappreciated. For example, when you think an idea is straightforward and don't sense when you are confusing others, it can be natural for your audience to "tune out" leave you dissatisfied. In really bad cases, people may even feel offended when your explanations show little regard for their confusion. When you are cut off from the feelings of others, it is much harder to bring them joy. This makes it harder to bring them closer to you, and, ultimately, accept you.

5.4 Attitudes

Attitudes are the hardest component of a capability to define. A rough idea is that attitudes are beliefs or dispositions that impact your ability to do research

effectively by shaping your responses to people, situations, and ideas. That is a mouthful, so let's illustrate it with a few examples.

A commonly held belief is that, in their heart of hearts, people are inherently selfish. This belief impacts your ability to do research. For example, when thinking about collaboration, a strong belief in selfishness might stress that people will only collaborate with you if it benefits them. If you have a negative view of your skills, this can translate into a belief that: **“No one will want to collaborate with me, I have nothing to offer”**. This idea can lead to academic isolation, which is often detrimental to success.

Consider how another belief might change the situation. Something like: **“There exist people who want to help others, even if it takes sacrifice.”** Under this belief, even if you have a poor assessment of your skills, you can nonetheless search for collaborations with someone who might be willing to help you develop your skills.

The previous example concerned beliefs about the nature of people. This next example illustrates how beliefs about success can impact our approach to research. Consider a belief something along the lines of (in its extreme version): **“It is better to be dead than to be irrelevant”**. This belief practically translates into the notion that unless you do research at a top university, you are better off not doing research. This creates an “all-or-nothing” mentality that puts tremendous pressure on the success of each research project and often leads to disappointment.

Consider how the following attitude would change this situation: **“Research is a big, human-wide endeavor, all of us have a role to play”**. With this attitude, working at a top university would be great, but working at a lower rank institution achieves roughly the same thing—contributing to a human-wide endeavor. This attitude can still evoke effort to succeed but has less force in creating disappointment if a research project does not work out as planned.

As a final example here (many more will come in [Part III](#)), consider the impact of the attitude: **“It only matters what the experts will think of this work”** when writing a paper or giving a presentation. This is a somewhat pragmatic attitude, given our emphasis in these pages on the importance of acceptance in research success. But this attitude can lead to approaches that make research hard to understand. For example, you are considering whether to explain some development more clearly in your paper. You might feel there is a downside in explaining things “too” clearly because it might look obvious

to an expert in the field. The attitude “it only matters what the experts will think” can result in papers that are overly prosaic. A paper written in this way is less likely to be embraced by a broader base of the community.

Consider how the following attitude would turn things around: **“Each research paper is an opportunity to invite someone new to get excited about my work”**. Of course, the experts in the field are important, but so are Ph.D. students and new researchers who could become excited through your work. This attitude justifies explaining things a little bit more clearly to keep more people with you. Experts might even appreciate your efforts to clarify things and excite more people about their area.

It is hard to create a table of attitudes as we did for attitudes in [Table 5.1](#). Qualities have a certain “timelessness”; what is good about the human character has been understood for millennia. Attitudes, by contrast, are more situational and a product of their times.

On a final note, it is useful to recognize that beliefs can sometimes shroud positive qualities. For example, honesty may be at the core of someone’s character, but they believe that competition is so intense these days that only people willing to bend the truth in their favor can succeed. This belief may cause them to act dishonestly, against their nature.

5.5 An example capability: Mathematical maturity

Having looked at the parts of a capability in isolation, it is important to consider how they come together to form a whole. By associating skills, qualities, and attitudes, you add to the richness of your understanding of each. You also see that many practical considerations make your expression of capabilities contingent and imperfect.

In this section, we look at an illustrative example that is an important research capability in its own right, depending on your academic field. This example gives you a more complete picture of how purpose, skills, qualities, and attitudes interact in a specific context.

I (Chris) am a great reader of prefaces of texts, particularly math texts. It, in part, comes from the insecurity about whether I will be able to follow the book’s content, having long felt inadequate about my mathematical training. A common prerequisite found in such prefaces is neither technical nor very

specific: “mathematical maturity”. This prerequisite is common to books in analysis, algebra, combinatorics, and applied mathematics alike.

Here is a typical definition of mathematical maturity: “a certain combination of technical skills, habits of investigation, persistence, and conceptual understanding” in the context of excelling in high-level mathematics.³ In other words, mathematical maturity is a *capability*. Let’s examine this notion within the framework developed in this chapter.

Purpose

What is the *purpose* of having mathematical maturity, and why is it so often stressed in mathematical texts?

Mathematics is written in a very “vertical” style. If every point was explained from the beginning, each text would take on enormous proportions. To keep things manageable and focused on new concepts, some amount of detail needs to be supplied by the reader. These texts assume the reader, even if not exposed to some of the ideas in the book, will be able to read, understand from context, and fill in the gaps. The purpose of mathematical maturity is that it allows the student to undertake the task of reading and understanding mathematics without too much trepidation.

Skills

The skills involved in mathematical maturity are vague. It depends on the mathematical subject area. However, patterns can be identified.⁴ Some skills are more concrete, like:

- identifying and stating mathematical patterns,
- carrying out generalizations,
- recognizing a valid proof and detecting “sloppy” thinking, and
- moving back and forth between the geometrical and the analytical.

³Taken from <https://blogs.ams.org/matheducation/2019/04/15/precise-definitions-of-mathematical-maturity/>.

⁴The following lists were generated by perusing https://en.wikipedia.org/wiki/Mathematical_maturity.

Others are more abstract:

- improving intuition by abandoning naive assumptions and developing a more critical attitude,
- spotting, correcting, and learning from mistakes,
- recognizing and appreciating elegance, and
- drawing a line between what you know and what you don't know.

These are important skills, but you would be hard-pressed to find a course that directly teaches them. You will not find too many lectures titled “Spotting, correcting, and learning from mistakes” (although maybe there should be). Mathematics courses (like many programs of study) are primarily organized around subject areas. Despite no direct course on the above skills, they are learned implicitly through reading texts, attempting problems, and making mistakes.

Qualities

Some of the skills listed in the previous subsection waded into territory we have labeled qualities. Consider, for instance, the skill of “learning from mistakes”. The quality of humility undoubtedly plays a vital role here. “Abandoning naive assumptions and developing a more critical attitude” requires courage. “Carrying out generalizations” would be impossible without some degree of assertiveness.

While skills of mathematical maturity are typically not taught directly in classrooms, and even less so are the qualities that support them. While it's conceivable that a math teacher could offer a lecture called “Spot, correct, and learn from mistakes”, it seems even more implausible for them to offer a lecture titled “Humility”.

When I (Chris) was learning mathematics in my undergraduate degree, I reached a plateau. I kept getting stumped on homework and exam questions. This starkly contrasted with another student in my class, who always seemed to “get out” of every corner a problem put him in. In my eyes, he was magical.

After many frustrating days and nights, I sat down to ask myself: How can he solve these problems, and I cannot? We did homework together sometimes, and I thought carefully about how he approached his work. While

making this comparison, I came to a realization: I lacked trust and persistence. Whenever I saw a new problem, I was convinced that I did not have the tools to solve it. I would spend a lot of energy and time imagining how the question was unfair. My skills did not matter; I was not using them to attack the problem.

By contrast, my classmate always had faith that everything we needed to solve the problem was in our hands. He never felt the questions were unfair and wasted no energy complaining. By the time I settled down from my first fit of indignation, he would have already solved the problem.

I did not trust myself, and I did not trust the tools I had at my disposal. My lack of trust made my skills worthless. But it was more than trust. It's not like I didn't try to use my tools *at all*. But I gave up on them very quickly. It's like trying to cut down a tree with a single swing of an axe. The fact a single swing does not suffice does not mean an axe is not the tool for felling trees. But it does tell you a lazy person with an axe is not a sufficient tool for felling trees. I needed to learn persistence. I had the tools and could learn to trust them, but I needed the extra ingredient of persistence to turn trust into results.

Attitudes

So, you may ask why I (Chris) gave up so easily when using my mathematical tools? The answer had to do with my beliefs. I believed the following strange concoction of logic:

Only smart people are good at math. Smart people do math without much effort because, ..., well, they're smart. If you have to work hard to solve problems, you are not smart. Therefore, if you work hard, you are not good at math.

If this logic sounds preposterous and circular, it's because it is. But you would be surprised by how common the logic is. I have seen it in myself, my students, and even my children.

For some reason, many societies have made a strong connection between mathematical ability and natural intellectual gifts. It's like there is an implicit message out there that mathematics should come easily or not at all. This debilitating attitude is at the core of what makes mathematics so difficult for

so many people—they are convinced they are not smart enough to do it. Faced with our own stupidity, it can be pretty hard to try, let alone undertake the sophisticated skills listed earlier in this subsection.

Over time, I (Chris) have realized that my “smarts” have little to do with my ability to do math. The more I think about how smart I am (actually, how not smart I am), the less mathematics I can do. The following attitude has helped me:

Mathematics is an ability that can be developed through trust and persistence. It is not a sign of genius. When approached correctly, mathematics is accessible to all.

With this attitude, I have engaged in mathematical thinking for at least a couple of hours a day over the last 20 years.

5.6 A dynamic view of capability

In the previous pages, we have implicitly assumed that skills, qualities, and attitudes can be changed and improved over time. In fact, this whole book is optimistic about how all three components (skills, qualities, and attitudes) can be elevated, often in unison. When it comes to skills, we all readily accept that they can be improved. Our whole educational system is primarily based on this notion. You may also hear about “attitude change”, although this is harder than changing skills. When it comes to qualities of one’s character, however, changing them can feel monumental, if not impossible.

Luckily, there is something quite obvious about qualities that should not be overlooked. They are not binary. No one is perfectly patient or perfectly impatient; everyone is along some continuum of different levels of patience. Also, different situations bring out different levels of patience in us. Qualities are situational and not absolute.

This simple idea is often violated in how we talk about people’s qualities. It is tempting to label someone as either “humble” or “arrogant” when, in fact, humility is a continuum that everyone falls into, and it depends on their situation or circumstance. In this light, qualities can constantly be improved, at least to some degree or in some context. A jump from the binaries of “arrogant” to “humble” is indeed a daunting task, but to advance in humility to some degree in some area is much more attainable.

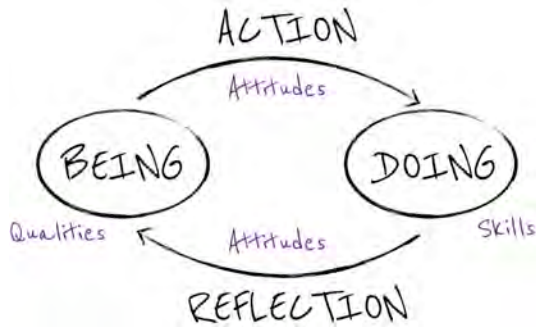


Figure 5.1: The learning mode: Being, doing, action, and reflection

Being and doing

One reason that skills may seem more malleable, and qualities less so, is that skills are something you “do” while qualities are something you “are”. Even saying this enters us into the classical debate in the philosophy of the mind. Behaviorism sublimates “being” as a mere account of “doing”. Fatalism suggests the opposite—who you are determines what you do. We prefer not to venture into these deep philosophical waters. Our intentions are far more pragmatic.

We are interested in giving a practical understanding of the interplay captured in [Figure 5.1](#). In this formulation, there is something “inner” about you that we call *being* that captures your qualities. *Action* expresses “being” into something “outer” in the world. This action is moderated by your attitudes and your skills. Your beliefs permit or constrain how you express your qualities, and your skills shape the impact your actions have. Consider again the person who highly values honesty but believes deceit is necessary to survive in a career. Such a person can take deceitful actions even when, deep in their being, they cherish honesty.

Of course, there is the reverse direction. What you do and have done changes who you are. This proceeds through a process of *reflection*. Again, how you reflect on your actions is colored by your attitudes. To give a small example, suppose a speaker loses an audience when speaking at a conference on a technical topic. The attitude that “no one follows technical talks” might not cause the speaker to reflect on the qualities that undermine his ability to be understood by others, such as patience or empathy.



Figure 5.2: The six research capabilities

We like to represent the relationship between being, doing, action, and reflecting dynamically, as in [Figure 5.1](#), to suggest that it is an ongoing and iterative process of continuous improvement. You are always in the cycle of being, doing, acting, and reflecting, at any moment in your lives. The task of improving our capabilities is a daunting one, but you have many opportunities to do so incrementally.

5.7 The six research capabilities

In the following chapters, we detail what we have learned through a repeated process of action and reflection on developing our research capabilities. We organize our learning around six broad research capabilities: reading, listening, creating, writing, speaking, and collaborating. See [Figure 5.2](#). So much can be said about each capability (much of which we have learned the “hard” way) that we have devoted a chapter to each capability in [Part III](#).

The “life” of a research project, from start to finish, follows reading, listening, creating, writing, speaking, and collaborating. First, you need to read and listen to get ideas by looking at papers, talking with your mentors, and listening to academic presentations. Next, you need to do something creative with those ideas that one might call “novel research.” But this is not enough. You must write your research into papers and present those ideas to the right au-

dience. These latter two capabilities are typically the grounds for acceptance and the “showcase” of the fruits of your labor.

The focus on creating, writing, and speaking is not new to us. You can find specialized books on each of these topics (we provide pointers to our favorites in chapter notes throughout [Part III](#)). Discussion of reading and listening is not commonly discussed as this is considered more “passive” and maybe there is less to say here. We emphasize them because they are foundational to how people start the research process. Discussion of collaboration is also too often overlooked. We believe relationships are the key to academic success, so we emphasize collaboration more than usual.

The main distinction, however, between our approach and more standard approaches is the emphasis that reading, listening, creating, writing, speaking, and collaborating are *capabilities*. The capability framework emphasizes issues (like purpose, character, and belief) that we believe are essential to research success but are not discussed systematically in the references we have found. By the end of the book, we hope you become familiar with the capability framework’s nuances and value.

5.8 Putting it all together

As a form of summary, we provide an example of young researchers exploring the ideas in this chapter. The story is fictional but inspired by real-life interactions with many young researchers over the years. The main purpose is to provide you with an example of how natural it can be to discuss your approach to research with those you trust.

Julian is a fourth-year Ph.D. student who was always a top performer during his undergrad degree. His parents stressed education highly and always saw how happy they were when he got high grades. However, after his first three years of Ph.D., he realized that the skills he developed for getting high grades were not translating into research success. His purpose for undertaking a research career remained consistent with his undergrad purpose—to delight and bring honor to his parents—but this purpose was not driven him to success as it once had.

He started to reflect on how his beliefs were maybe impeding his progress. During his undergrad, he got high grades by finding solutions online to every practice problem in the textbook (even problems from extra textbooks) and

memorizing their solutions. During the exam, he was often already familiar with the questions that were given. When it came to research, Julian adopted a similar approach. He read many papers, found connections between them, and then connected a few dots. However, his advisor was never very excited about his output, as it showed little originality.

In frustration one day, Julian started to talk to a Ph.D. student in a nearby cubicle, Yi. Yi was in her fifth year and had already secured a position at a prestigious research institution. Yi was known to always have time to talk to others who were struggling with their research. Julian sheepishly approached her.

Yi: Julian, good to see you. You seem stressed.

Julian: Is it that obvious? Yes, I keep reading papers and reading papers trying to make connections, and it never excites anyone, especially me. Honestly, I can't read any more than I am now. Really not sure what to do.

Y: Why are you reading so many papers?

J: How else am I supposed to come up with ideas? I am trying really hard here!

Y: Settle down, settle down. I can see you have worked hard. Indeed, you are amazingly patient to read so many papers. I can read at most one a week! But let me ask again, why are you reading so many papers?

J: That's what I've always done. I keep reading until I find the answers. Just like in coursework, I do every problem in preparation for the exam.

Y: But that's your problem right there, Julian. What makes you think there are answers to be found? Worse yet, what makes you think you can find answers in papers?

J: I guess I just thought the answers would be obvious if I read everything and knew everything.

Y: But obvious answers are not good! What good is doing all of that work if you only come up with something obvious?

J: Well, when you put it that way ...

Y: Research is different than classes. In classes, you impress people by showing them you know everything they expect you to know. In research, people don't care so much about that. They want you to show them something they *don't know*.

J: But where can I find something that others don't know?

Y: Julian! You are still not getting it. You can't *find* things others don't know in papers! By definition! You have to create it!

J: Create it?

Y: Stop reading papers and start thinking about what *you* want to say. Can you do that for me?

J: But I have never thought much about what *I* wanted to say.

Y: Now is the perfect time to start!

This story demonstrates that Julian is someone with immense skill but harbors an attitude towards research (that great ideas are found in papers rather than springing from the human mind) that limits his effectiveness. Changing attitudes is not straightforward, but one helpful way is to discuss your attitudes with people you trust. Maybe Yi and Julian can continue to discuss together to evolve their attitudes towards research.

5.9 Reflection questions

The questions below are provided for you to reflect on what you have read in this chapter. If possible, it is beneficial to discuss these questions in a group of your peers.

1. What are some of your best skills? (They can be related to research or not.)
2. Describe one of your mentors. What are their best qualities?

3. Which of the qualities listed in [Table 5.1](#) do you find most compelling? Why?
4. What are some attitudes that you developed when you were young? Do these attitudes impact you in your research, either positively or negatively?
5. Describe an instance of how you changed one of your attitudes. How did this change come about?
6. Describe one of your capabilities. Detail the purpose, skills, qualities, and attitudes involved. (This capability can be related to research or not.)
7. Would you describe yourself as “mathematically mature”? Why or why not?
8. Which of the research six research capabilities—reading, listening, creating, writing, speaking, and collaborating—are you most confident in? Which of these capabilities do you think needs the most improvement?

Chapter notes

The concept of “capability” borrows ideas from FUNDEAC, as described in [7].

Part III

The research process

Part III applies the framework in **Part II** to the different elements of the research process. In particular, we look at the six research capabilities presented in **Chapter 5** summarized as reading, listening, creating, writing, speaking, and collaborating. This part consists of six chapters, one for each of these capabilities. Each chapter includes a discussion of purpose and a delineation of skills, knowledge, qualities, and attitudes.

A feature of these chapters is that multiple purposes for exercising capabilities are offered. There is more than one purpose to read, write, and present a paper. This variety helps underscore the richness of the research process.

As in the previous chapters, each chapter finishes with a fictional narrative that explores the journey of a young researcher. This narrative serves to illustrate some of the key points and adds humanity to what might otherwise feel like a theoretical exercise. In these remaining chapters, these fictional narratives are longer and more detailed than before. We encourage the reader not to skip these narratives and hope that some of the discussion rings true to their own experiences.

Chapter 6

Reading

Reading is one of the fundamental human capabilities, as it facilitates extended conversations with the past. This is essential to do in research. We must build on what others have said and discovered.

Reading serves you in many ways, and it is certainly not isolated to research. A research career requires a lot of reading outside of the research context. You read textbooks for teaching; you read emails; you read the news, etc. There are commonalities to these forms of reading, but reading in the research context takes on a flavor all its own.

Even the foundational skill of basic reading comprehension—recognizing, understanding, and interrogating words, sentences, and paragraphs—is not trivial. A preponderance of early education is directed toward developing this ability. The ability to read takes many years to mature and is likely impossible to perfect. We assume you are comfortable with this foundational skill of basic reading comprehension but probably need advice on directing this toward reading for research.

When discussing reading as a capability in research, we will focus on reading research papers. Of course, reading textbooks and monographs, and other forms of academic output is also important, but in many academic disciplines, most of your reading energy goes into papers. Reading papers is a daily activity for the working researcher.

6.1 Purpose

We undertake the reading of research papers for different reasons. For example, you may be asked to read a paper to evaluate it for publication in a conference or journal. Although this context is important, it will not be our primary focus. Our focus is on the act of reading research papers to further your own research career. Despite this narrowing, there remain multiple purposes for reading a paper.

To learn what has been done

As discussed in [Chapter 3](#), novelty is a key dimension of beauty in research. But just because something is new to us does not make it “globally” novel to the community. You must carefully sift through the literature to ensure your research idea is truly novel.

“What has been done” has multiple dimensions. As detailed in [Chapter 3](#), there are six dimensions of novelty: setting, question, data, results, and method. To get a complete picture of the landscape, you must grasp what has been done along all six dimensions.

Getting a complete picture of the history of a particular research question is a difficult job. In practice, researchers limit their search of the literature to make it more manageable. It is practical, for example, to limit the search by “fixing” one or more of the six dimensions of novelty. For instance, it is common for an important topic to be studied in multiple disciplines. A great example of this is human decision-making, which is a topic of interest in many fields: economics, psychology, sociology, and operations research, to name just a few. Each field approaches the problem using different methodologies to answer particular types of questions. You can limit your search within one of these disciplinary traditions to make things more manageable.

To learn about what has *not* been done

Many novice researchers get into the following trap: they keep reading papers trying to understand what has been done until they inevitably get overwhelmed. There is already too much that has been done! The trap here is that you also need time and energy to think about what has *not* been done.

Some researchers we know like to think of ideas by going into a room by

themselves. Once they have an idea they like, they read the literature to see if others have had this idea before. This is a defensive approach to the literature.

Other researchers are more like detectives; they read papers searching for “hints” about what might be next. A paper may pose an open question. Maybe a paper makes an assumption you have some hope of relaxing. Perhaps two papers appear to be saying different things, but you have some hope of putting them both in a single, illuminating context. This is a more offensive approach to the literature by using your reading of papers to generate novel ideas.

To learn about a building block for your research

The two purposes mentioned above for reading papers are to understand the overall research landscape: what has and has not been done. The focus there is on the novelty aesthetic of research. It is natural to build a purpose for reading papers out of other research aesthetics. The one that immediately comes to mind is “usefulness”: Maybe you can read a paper to learn something useful for your research idea. Examples include papers on methodological tools (statistical techniques, mathematical ideas) or early papers that describe phenomena simply and clearly before later papers added layers of complexity.

For example, you often need to study an “old” paper far from the novelty frontier because it forms a foundational building block that the current literature takes for granted. This “building block” paper may even be from another field. Examples of this are numerous, including a seminal economics paper used in a recent series of marketing papers or a mathematics paper that introduces a tool used to prove results in current machine learning papers.

Of course, some “building blocks” can be found in textbooks or research manuscripts, and these can often be an excellent place to start when you are catching up on background material. However, it is important to note that not all textbooks are written for “quick” consumption of a small piece of content. For example, you may need to return to the start of a long book to get the right notation or context for later chapters. I (Chris) remember being a young researcher and convincing myself I needed to read a large stack of dense mathematics texts before I could do anything new. This was, of course, utterly false because often, I only needed a few ideas from a large book (often in the first couple of chapters) to make progress.

I (Runshan) had a similar experience. I used to believe I needed to take a whole class for every tool I may use in a research project. It turned out that

sometimes the most valuable part of a class was the reference list of papers it pointed me to. By reading these reference papers, I could bypass spending time learning things I did not need and focus on a few specific questions that were most helpful.

I learned about interesting public datasets and creative identification strategies by reading papers. Although I learned standard econometric tools from classes and textbooks, the specific research designs in other papers usually inspired me to find useful contexts and data variation in my setting.

A paper does not have to be novel in all dimensions to be helpful. When facing challenges that are not essential to your main contribution, it is often helpful to check how others handle them. Very often, their approaches may also provide inspiration for your problems.

To learn what published papers “look like”

Like all types of writing, research papers have both form and function. Sometimes you read papers as much for the “packaging”—how ideas are explained or how the papers laid out—as the actual content.

This may sound a little superficial, but we believe that the layout and form of research papers can communicate a lot. In some fields, papers have lengthy introductions that aim to give a thorough historical telling of the problem. In others, the style is to get into new things right away. Some areas prefer the paper first to summarize their findings in lay terms. In others, such summaries would be considered “fluff” worthy of the chopping block. Some fields like to see acknowledgments on the front page of working papers so that others can see who has implicitly “vetted” the work. Other fields put acknowledgments at the end and maybe only after the paper is published.

The choices a community makes on how they like their research to look is not something you can learn by introspection. It can only be understood by reading and internalizing the aesthetic of papers in your field. It can be hard to learn conventions from a single paper; you often need to look at patterns across many papers. This is a different type of reading than when you try to “dig deep” into the content of a single paper.

In this reading style, you can also learn “shorthands” for explaining ideas or describing phenomena that would otherwise be cumbersome. Seeing how previous papers have described the setup of a related problem can save a lot of time when trying to think of “nice” ways to say things. We are, of course, not

advocating plagiarism of any sort, but instead, to write in a way that seamlessly uses commonly-accepted terms, phrases, and standard explanations. Not only does this practice make your papers easier to read, but also easier to write.

To learn what authors like

So far, we have treated papers as a source for learning about novelty (what has been done and what has not been done) and using papers as tools to learn techniques you need in your research or writing. But this leaves all of the other research aesthetics from [Chapter 3](#) unexplored—can you learn about those by reading papers?

When you read a paper—notably a series of papers by an author or group of authors—you start to get a sense of what they find beautiful and worthy of their effort. Knowing how difficult it is to write a paper, particularly one published in a premier outlet, it is natural to ask, “What about this paper gave the authors the energy to write and revise it for publication?” Someone, at some point, must have thought this was a cool thing to do.

We believe it is essential to keep this positivity about the work of others. Too often, we see academics focus on the flaws of other people’s research. This is natural, of course, due to the drive for novelty. You must see the flaws or incompleteness in others’ work to carve out space for your own. But, taken too far, you can develop a pessimism toward research in general. Researchers like to produce and read certain types of papers more than others, and you can learn about what they like by examining what they produce.

A good example is reading a series of papers to identify what topics, methodologies, or styles of results are currently “hot” or “heating up”. It is helpful to ask yourself what makes this topic or methodology “hot” and what excites people about it. There are often hints for this written into papers—especially in introductions, conclusions, and passages where writers summarize the literature—where authors justify to themselves and others that writing and reading this paper is worth their time.

To learn what reviewers and editors like

The previous subsection overlooks one crucial reality: papers must be accepted by others to be published. All reputable research outlets are *peer-reviewed* (we described this in detail in [Chapter 2](#)), and so the published works of these

outlets represent the combined views of the authors, editors, and reviewers for the outlet. As many working researchers know, what appears in print is often a compromise of the author's original vision and the suggestions of the team that reviews the paper.

As authors respond to the review team, how a paper changes during the publication process can tell you a lot about the tastes of reviewers and editors. The reviewers tell the authors what parts of the paper they find ugly and suggest ways to make it more beautiful. This process of suggestion and response is primarily done “behind the scenes” in private exchanges between authors, reviewers, and editors. This is one of the sources of the wisdom of established researchers: they have seen a lot of the “behind the scenes” and understand how papers shape up to become publishable. They have learned the taste of the reviewers and editors of their field through experience and can write new papers in a way that suits that taste.

This process is not entirely opaque to the young researcher, however. A common practice nowadays is for authors to post “preliminary” versions of their work on online repositories like arXiv or SSRN. By comparing these preliminary versions with the published version of the paper, you can glimpse one side of the “behind the scenes” process of revising a paper for publication. Sometimes, the published version may be much longer than the preliminary one as reviewers ask for additional extensions or “robustness checks”. In other cases, the published version may be shorter to meet page-length requirements, so it can be instructive to see which parts of the paper were jettisoned.

The idea here is that you can read the evolution of a paper to understand the nature of the interaction between the authors and those judging the work of the authors. This can reveal the preferences of both sides and give you a broader sense of the taste of researchers in their target research community.

6.2 Skills

“Digging in”

Most research papers are written to persuade. You must make the case to reviewers and editors that your paper is worthy of acceptance in a research outlet. Some of the most accomplished papers are so definitive that you may find it hard to find any “room to stand” to doubt, or even have curiosity, about

the subject. To get the most from papers, you must have the courage to “dig deep” below the surface of the argument presented to you.

Going beyond the surface is not an easy task because it requires active reading. It is different from reading a novel where the purpose is to be taken away by the author into a world of their creation. This is not the purpose of reading in the research context. You must insert yourself as a character in the author’s narrative and ask, “What am I getting from this?”

However, being empowered to ask this question is not an invitation to expect easy answers or convenient takeaways. You will only get superficial insights if you insert yourself superficially into a paper. This can be true even if you spend a long time reading a paper without connecting it to your research ideas. You can get completely immersed in a paper but only become more convinced of its greatness—and often, concomitantly, your inability to use or understand it—if you do not provide space to allow yourself to develop your thinking on the subject.

This is visually illustrated in [Figure 6.1](#). If you superficially read a paper, the result will be a superficial understanding. If your independent thinking of the subject remains shallow, often no amount of investigation will enhance your insights.

One approach to get beyond this is to look at the title of the paper, the titles of the sections, and maybe the first couple paragraphs of the introduction, and then try to think for a while: “What can I say about this subject?” In our experience, if you spend some time—say, thirty minutes—trying to answer this question, you will bring out your natural intuition and sources of confusion. You will develop a much clearer sense of what you hope the paper can address.

With this preparation, you are much more immune to “losing yourself” in the narrative set up by the authors. Because you have reached some of your own conclusions and confusions, you will look at the paper’s results with greater intrigue.

A word of warning here. “Deeply” reading a paper is often not a light exercise. Some papers can take days or weeks to “get to the bottom of,” and some may be impenetrable given your current preparation. In other words, a “deep” reading is a nontrivial commitment. I (Chris) once read a single paper on and off for over six months and still felt I do not understand it. Yet, somehow I am still convinced that it was worth the effort. To this day, I continue to pick it up now and again to see if it is clearer to me this time around.

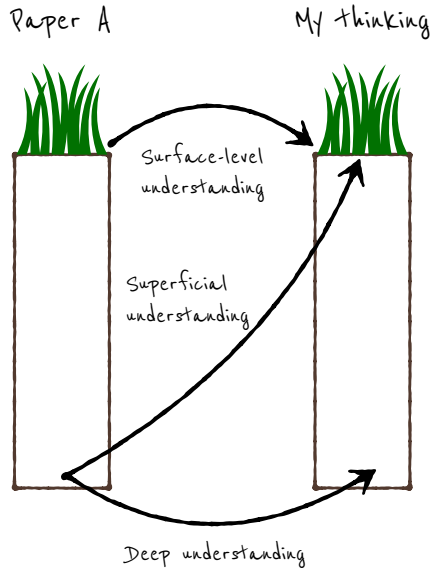


Figure 6.1: “Digging” into a paper

Deciding how deep you need to go

The previous subsection described some skills and insights you might employ to dig deeper into a paper. But, as we explained, digging into papers can take a lot of time and energy. Before taking a paper and committing your energy to a deep dive, it helps to think about the purpose of reading that paper.

In the first section of this chapter, we gave six purposes for reading a paper. Not all of these six purposes require the same level of depth. You can get a surface-level appreciation for what a paper does and does not do relatively quickly, and this may suffice for sorting out if a research idea is sufficiently novel to look into. Getting a sense of what a published research paper “looks like” lends itself to a surface reading; going too much into the details can even distract you from appreciating the form. You should not feel bad if you give a paper a cursory read if your purpose is something other than understanding it deeply.

When I (Chris) was a young researcher, I was advised to “read at least one paper a day”. When I first heard that comment, I thought it was utterly impractical. In those days, I convinced myself that a paper was worthy of reading deeply or not at all. In retrospect, I consider that a big mistake. I read too few

papers, and even the ones I read deeply, I did not have a good sense of the context of their results. Often, to get the context for research and what makes a particular line of questioning interesting, you need to read a series of papers at greater or lesser depths.

I still do not follow the advice of “reading at least one paper a day,” but I started to appreciate the intent behind the advice. Reading a paper a day can help you with several of the purposes described above. You learn about a variety of writing styles and ways of presenting ideas. It could be an excellent habit for systematically learning an appreciation of the research aesthetics.

So how do you read a paper at different depths? Reading abstracts and introductions may suffice to understand what has been done. You accept that your grasp of the paper’s content is not perfect, but deep enough to catch a “flavor” of what they do. Of course, it can be dangerous to *only* read abstracts and introductions when trying to learn from the literature. One way to summarize what a paper covers is to say, “It takes Classic Paper A and adds a new twist which gives rise to a new type of result”. This type of classification is difficult if you do not understand Classic Paper A deeply.

It can also help to see how other people summarize the content of a paper you want to understand better. For example, your goal is to understand Paper A because it appears to be somewhat seminal in your area of interest. You have not decided yet if it is worth a deep dive, but you want to gain more perspectives on it. You can search online and see what papers cite Paper A. Say it is cited by Paper B. You can look at how Paper B describes the content of Paper A. If you are lucky, Paper B may go into some depth about how Paper A is used, understood, or appreciated by other researchers. For example, it might be that only one part of Paper A (say, its first major finding) is well-known, with its cumbersome findings found later in the paper being less significant. This can help you target your appreciation of Paper A without investing heavily in its less relevant parts.

A harder task is to learn what has *not* been done. It is not very common for papers to delineate their weaknesses in detail. Indeed, one could even accuse some papers of obfuscating their flaws. This is a natural part of the publishing process of papers that leads to the accentuation of the positive aspects of a paper. However, this means that for you to figure out how to “fill in the gaps” or “go beyond” a paper requires more digging and careful reading. Finally, the greatest level of depth is required when attempting to solidly learn a building block.

Finding the right paper to read

The previous subsections spoke about skills to navigate a paper sitting in front of you. An arguably equally challenging is deciding what paper to sit in front of. Research can feel exhausting and endless if you cannot find the right papers to read. One of the important things a student learns from their research advisor is what papers to start looking at.

“Right” again depends on your purpose for reading. If your goal is to get a sense of what has been done or what papers should “look like”, it is almost hard to find a “wrong” paper. This is, in part, because your purpose lends itself to a surface-level reading that is often not a huge time investment. On the other hand, learning a building block or sussing out “unsolved” problems takes a far greater time commitment. More effort is justified in selecting the “right” paper to invest in.

One tool greatly helps search for the “right” paper: a computer program that searches the content of published and working papers. At the time of writing this book, the best tool in this regard is Google Scholar. You go on Google Scholar and type in some keywords of importance to you and look at the results. This is not a book on doing careful literature reviews; we leave that to other sources. We only want to mention a few hints we have picked up over the years using a tool like Google Scholar.

First, it is good to go beyond simple keyword searches. Other things we find are important to discriminate on are (a) outlet of publication, (b) number of citations, and (c) year of publication. Let us describe each in turn.

Not all papers have the same fate in the research world. Some never get published, some get published in lesser-known journals, while others get published in prestigious outlets with brand recognition. Two things are important to understand about the difference between these fates. First, it is not always the case that a paper published in a prestigious journal is “better” than an unpublished one. There is great variety in the quality of research, and the editorial process has a lot of variance. However, when searching for a paper worthy of a tremendous effort, we recommend paying attention to the prestige of the outlet. These papers can be more heavily reviewed (more people have vetted their results) on average, which makes their findings more reliable on average. We do not recommend isolating your attention to *only* look at papers in prestigious journals, but it is often a good place to start.

This brings us to the second point about the different publishing fates of

papers. If your goal is to publish a paper in a particular outlet, it makes sense to read published papers in that outlet. You want to read papers that you aspire to write. It is important to recognize here that outlets are not entirely about “quality”; they are also about “community”. If you don’t recognize the names of people who publish papers in the outlet you hope to publish in, you are likely targeting the wrong outlet (or you need to learn more names). It is a sign that you are distant from the community you hope to belong to. Researchers can develop elaborate narratives for why the journals *they* publish in are somehow the most prestigious ones. Targeting the community you are closest to is a good idea, as this will often yield the greatest chance of success in publishing.

Next, let’s examine the question of citation count. On the one hand, citation count gives you some idea of how “central” the paper is to the academic discussion on the topic of interest. If a paper has a high citation count, it means that many people have read and appreciated its content. Such a paper is a good candidate to read to get a context for what has been done and possibly learn important or standard tools and building blocks. On the other hand, reading papers with very few citations can also be helpful. Papers with few citations are less well-read in the research community and so may possess ideas that are more controversial and “off the beaten path”. These ideas on the periphery can assist you in coming up with a novel idea. This is especially true of working papers that have not yet been published. For such new papers, it can be impossible to tell if this paper will be a “classic” down the road, but it can also tell you about the field’s latest thinking. Reading new papers is a great way to learn about what is exciting other authors in your field.

Finally, consider the publication year of the paper. The last paragraph highlighted the importance of reading “new” papers. This is especially helpful in understanding what is “hot”, but there are additional benefits. Reading the introductions and literature reviews of recent papers is a good way to learn about other papers you may want to read. The newer the paper, the more complete its landscape of available research.

It is important, however, not to overlook “old” papers. I (Chris) am a huge fan of papers written in the 1950s and 60s. In the fields I have come across, papers of that era are often much shorter, tend to have a very focused message, and are often more conversational. For example, I have seen papers published in top mathematics journals in the 1950s that offer conjectures and open questions to their readers, possibly with a little lemma or two they think

could be useful. The spirit feels to me like: “Here are some things I have been thinking about; I hope it proves useful to you.” I have not seen anything written in this style in more recent times.

When reading older papers, you can get to the “start” of ideas or research directions. This can sometimes be a double-edged sword. If you are lucky, the earliest presentation is simple and clear before other researchers have added complications and extensions over the years. But it can also happen that the earliest presentation is incomprehensible using today’s vocabulary, and it takes more effort than it’s worth to sort out.

Another benefit of reading “old” papers is that it can often take you back to when ideas were more controversial and thus discussed with greater depth or vigor. This is a point Latour makes eloquently in his classic book *Science in Action* [15]. In his view of scientific knowledge, something becomes knowledge as it becomes less controversial and more widely accepted. This is often achieved when papers cite earlier work without much comment. Something like:

According to Smith et al. (1972), the key decision variable in this problem is ...

Chances are, when you go back to 1972 and read papers in and around that era, the ideas of Smith et al. (1972) were far less accepted. Understanding the controversy of earlier ideas helps in learning what might be the controversy associated with newer ideas. This can be useful in coming up with original thoughts.

We think it is wise to read a balance of papers, new ones, and old ones, ones with many citations and few citations, ones from recognizable journals, and ones from more obscure outlets. Where you concentrate your efforts, of course, depends on your purpose and disposition.

Finally, a word of warning. Sometimes you need to read a paper in depth ...to know if it’s worth reading in-depth! In research, there are few shortcuts, particularly when you are learning a new area. The good news is that reading the wrong paper in depth is often not as much of a waste of time as you might think. There are many feasible ways forward in research. Even if the paper you read in-depth is difficult and confusing, knowing what is difficult and where to be confused is helpful in sorting out what paper you should read next. All is not lost.

Taking notes

Another skill when reading is documenting what you are learning by “taking notes” of some form. There are, of course, numerous ways to do this. Some people like to print out the paper and write on it by hand; others prefer to make electronic notes on a PDF; still others like to make notes on a separate, clean piece of paper or in a notebook. We (Chris and Runshan) find ourselves doing every combination of the above, depending on the situation. When looking to solidify a tool, we are more likely to “write along” while reading a paper, but sometimes when looking at literature to get a sense of an area, we may only make terse notes while reading through an abstract or introduction.

Another thing we have found is that while taking notes seems important, we very infrequently look at the notes we took at a later time. It is reminiscent of the famous saying often attributed to Dwight D. Eisenhower, which he credits as standard wisdom in the U.S. Army:

Plans are useless, but planning is indispensable.

Note-taking helps you process the paper you are reading, if only to slow you down and force you to think. Taking this time is often more valuable than the chicken-scratch notes that result. I (Chris) have printed the same paper numerous times and started taking notes from scratch each time. This is partly due to poor organization skills (I lose printouts of papers daily) but also because my need to look at a given paper changes over time. It sometimes makes sense to look at a familiar paper with fresh eyes.

Related to the previous point, note-taking keeps the reading process more active. It is easy to get lost while reading. A professor once advised me (Runshan) to always read papers with questions. He gave a list of general questions, including “What are the research questions?”, “What are the main challenges?”, “What are the key assumptions?”, “What are the main findings?”, and “What are the implications for future research?”

The answers to these questions may be scattered throughout a paper. I usually write down the questions before I start reading a paper and make notes when I find related points. Very often, I generate new questions while reading. If I do not write the questions down, they quickly disappear from my mind as I get absorbed into the paper. But this leaves me with the feeling that I am missing something. If these questions are captured in notes, they serve as a road map. The seemingly daunting and tedious task of reading a 50-page

academic paper is turned into an adventure of finding answers. I am always delighted when I put additional notes under my questions. This helps me maintain active attention during reading.

Making connections between papers

So far, we have deliberated on the skills useful in understanding one paper sitting on your desk. Of course, research is not limited to understanding one paper; you have to read many. Even more, you cannot read these many papers in isolation. What is essential is the connection between the papers that you read. Understanding the connections gives you a better sense of the gaps and missing storylines.

Reading a series of papers published over time that build, respond to, or extend one another can be particularly useful. Each paper is like a chapter in an evolving novel. As the story evolves, certain characters drop away, others come to the fore, and there are plot twists and emphasis changes.

Each paper also tells the parts of the story that came before it when reviewing academic literature. Sometimes papers even have a special section called “Literature review” for this task. Reading a sequence of literature reviews of papers that build on each other is an enlightening exercise in viewing the same concept from many perspectives. However, it is important to recognize that a paper’s literature review is not a perfectly unbiased telling of the history of an idea or a topic. The job of a paper’s literature review is to draw interest in the work done in that paper, not to give a balanced or complete overview. Reading multiple literature reviews provides a more complete picture.

The topic of literature reviews comes with a word of warning. Because of academia’s focus on novelty, there is a tendency for papers to overstate their novelty and downplay their similarity to past contributions. We don’t think that authors set out to be misleading, but they may unconsciously avoid looking into directions in the literature that may diminish their claim of novelty.

A thorough review of the literature is often required to get a sense of the true novelty of any one paper. But this is easier said than done. A surface-level understanding of the overall topic can lead to an impression that papers are unrelated when in fact, they are intimately connected. Finding these connections requires some effort to penetrate beyond the surface. But penetration itself can be insufficient.

See the extension of [Figure 6.1](#) in [Figure 6.2](#). You read Paper A in a lot

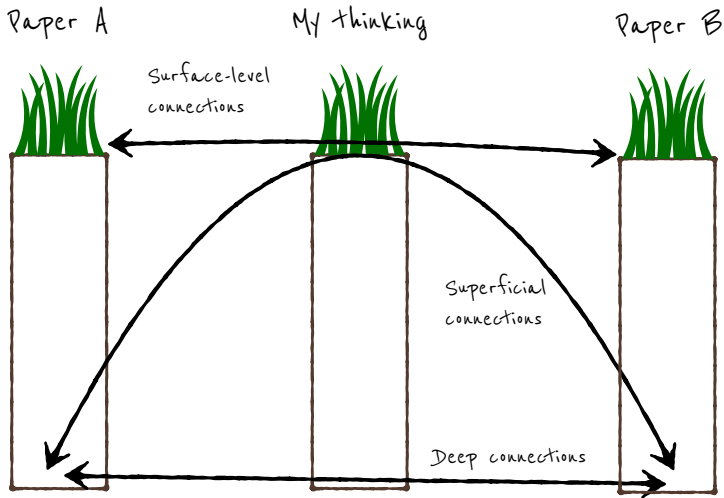


Figure 6.2: Making connections

of detail. You understand exactly how it built its arguments and derived its results. Yet, you never took the time to let that paper change the way *you* think about the topic. You understand Paper A in isolation, but you need to allow Paper A to impact your thinking so that you think about *new* ideas from its perspective. When you do this, you may look at Paper B and realize that Paper A and Paper B are deeply related, but this relationship would not be unclear to the casual reader.

Let us give a specific example of this phenomenon. I (Chris) was doing some research on the duals of vector spaces for an optimization project. There was a special type of element of the dual that I knew as a “singular functional” from the text I learned from. Yet, when I searched for “singular functional” in Google Scholar, I was not finding many hits. It was only when I decided to read a related paper carefully that I realized another name for a “singular functional” was a “charge”. As soon I searched for “charge” in Google Scholar, I found a whole world of related references. That taught me that a surface-level search of many papers—and not taking the time to let the papers change at least your vocabulary—was a draining and often fruitless exercise.

The search for connections, then, has a two-dimensional character. It involves both depth (penetrating a paper) and breadth (reading many papers). You often need to explore these two dimensions simultaneously to make progress.

It should be noted that finding deep connections between two seemingly unrelated papers is a tried and true style of research. It is sometimes described as “research arbitrage”—taking an idea from one field and applying it in a refreshing way to another. A recent example is excitement in many fields over machine learning tools. Finding a deep connection between machine learning and accounting could be a compelling piece of novel research. The number of people with a deep understanding of both topics may be relatively small, and the impact could be significant if the research creates a useful dictionary between concepts in the two fields.

6.3 Qualities

Solid skills are often not enough to persist in the challenging task of reading. Even today, I (Chris) find it immensely difficult to get myself to sit down to read a paper, despite the fact I have read hundreds by now. I believe deficiencies in my qualities are the main reason for my difficulty.

The six prevalent qualities in reading

In [Chapter 5](#), we detailed six prevalent qualities in the research process: patience, curiosity, humility, courage, hope, and empathy. In that chapter, we described these qualities in detail and made the point that they apply broadly across all research capabilities. That is certainly the case for the capability of reading papers. We deliberate on these qualities more here to further emphasize their importance. In later chapters, we will explore the six core qualities in increasingly lighter depth, as many of the most salient points will have been made by then.

Patience

Getting to the bottom of what a paper contributes is often far from obvious. It may introduce you to new terminology, frustrating notation, baffling figures, and confusing claims. It is a lot to swallow, and it takes a lot of energy to swallow it.

It is tempting to quit reading a paper before you come to the point of some clarity about what you are learning from it. There might always be a voice in

your head asking, “Is it worth the effort?” Sometimes it is, and sometimes it isn’t. Without patience, you will never find out.

Curiosity

Reading a paper without any curiosity about what it says or why the authors wrote it is a surefire way to take a nap. To avoid slumber, it helps to have questions in mind and to be curious about what the paper says about these questions (as we touched on earlier in our discussion of notetaking).

I (Chris) find myself immensely curious about what motivated the authors of papers to write them the way they did. I find myself pondering the most minor details: “Why did they choose this notation and not a more standard one? Why does the paper flow in this way? I would have thought these two sections should be swapped?”

Humility

It is hard for someone to read a paper who fears being confused or exposed to their ignorance. The best way to feel smart and superior is never to read at all.

I (Chris) have never read a paper that did not confuse me to some greater or lesser degree (often greater). For this reason alone, I read fewer papers than I probably should. I like the feeling of being on top of things, and so avoid confusion. Even when I get humble enough to read a paper, I find myself trying to dismiss it before I even understand it: “Well, this certainly can’t be useful to me; it doesn’t even make sense.”

Humility allows you to look at every paper as an opportunity to learn something new. You neither feel unequal to the task of understanding a paper nor dismiss it without giving it a chance to change the way you think. Humility equips you with the mindset to grow with every paper you read.

Courage

Reading a paper is a courageous act. Interrogating a paper requires you to slow down and say to yourself, “I don’t understand this.” This requires bravery because you start to add up all the things you do not know, and it starts to feel overwhelming. All the more so when you realize that you need to read ten confusing papers to get to the bottom of what is known.

It also takes courage to question a paper, especially when published in a top journal or written by famous people. When I (Runshan) first started reading papers, I assumed every paper was perfect. If something seemed ambiguous or suspicious, it must be that I was too stupid to understand it. One time I attended a student-led reading group session. In the session, one student presented a paper published in a top journal, and others openly discussed the strengths and weaknesses of the paper. I found that some points brought up by others were exactly my confusion, but I did not have enough courage to raise the question to myself, let alone bring them up in the discussion. After a few more sessions, I gradually developed the courage to acknowledge my own thoughts while reading.

Hope

Reading can be overwhelming; you need to have faith that it goes somewhere. If you have hope, you eagerly read the eleventh paper after reading ten papers that did not tell you what you wanted to know. Hope tells you there is a great paper to read out there; you just need to find it. If you lose hope, even if you find the energy to read papers, you will not be able to recognize their greatness.

Empathy

Despite everything we have said about interrogating a paper, you must also take it on its own terms. An overly dismissive reading of a paper clouds its potential depths. Best to read from the author's perspective. People who write papers are people just like you. They found it hard; they needed to read many papers to develop their own. Nobody sets out to write a meaningless paper. Empathy for the author helps you connect to the original passion or meaning behind the research. Each paper had meaning to someone at some time. Empathy will bring you to that meaning.

In addition to our six prevalent qualities, a few others stand out from [Table 5.1](#) as important to the capability of reading. Let's examine some of them in detail.

Assertiveness

As we have discussed, you need to insert your thinking to get more out of reading a paper. This takes assertiveness. This isn't easy because assertiveness

requires you to honor your voice and point of view. This is hard in the world of talented arguers that populate academia.

One interesting example is the frequent usage of the words “trivial”, “obvious”, “clear”, or “straightforward” in research papers. These words are tempting for writers because their use can avoid the necessity of explaining some steps in a logical argument. In some cases, these steps are indeed “obvious”, and using such words is appropriate. However, in my (Chris’s) experience, these words can also hide sloppy or incomplete reasoning that authors do not want to think about and do not want the reader to think about. In a shallow reading, the reader is more likely to accept that something is “obvious” on its face, even if their gut feeling is that it is not “obvious”. A more assertive reading asks why (or even whether) the statement is obvious. In my (Chris’s) experience, it is not too uncommon that something the authors claimed as “obvious” is hardly so, and sometimes even wrong.

Another example is when authors “hide” their strongest assumptions by downplaying their significance by writing them in an obscure way. Sometimes the role of an assumption is very clear in driving the result of a mathematical proof or logical argument, but that role is not made clear in the writing of the paper. It takes tremendous assertiveness to insist on understanding exactly *why* an assumption is made.

Flexibility

Assertiveness, of course, has its limits. When you assert yourself, you must beware of creating a weak “straw man” out of the paper’s arguments to serve only your interests. When you are flexible, you can see things from multiple points of view. You appreciate both the strengths and the weaknesses of a piece of research.

One thing I (Chris) often tell young researchers is that it is important to understand why their research results are *both* obvious *and* hard to understand. How is that possible? Well, it requires flexibility in thinking. It often requires you to look at your work from two different, even competing, perspectives.

Take, for example, the concept of opportunity cost in economics. This idea holds that taking action has an associated cost of *not* doing something else you *could have* done. Sounds like a reasonable enough and straightforward idea. If you decide to study for a Ph.D., you should consider what op-

tions you are foregoing. But from a different perspective, the notion of opportunity cost can be confusing and off-putting. Contrast it with the notion of mindfulness in Buddhist thought. Mindfulness is a state of being entirely present, accepting thoughts, feelings, and sensations without judgment. A goal of life, in some Buddhist traditions, is to live a life in a perpetual state of mindfulness. Thinking about what you are *not* doing to calculate your opportunity cost for what you *are* doing (or will do) is anathema to this ideal. For someone striving to live in a state of mindfulness, the value (and maybe even the meaning) of opportunity cost is hard to comprehend.

Trust

Earlier in this chapter, we described the skill of “digging” into papers. A quality related to this heavy task is trust. You need to trust the authors of the paper that there is something worth digging for. Alternatively, you need to trust your instinct that the effort will be worthwhile.

Trust is especially important when you read a paper that challenges your current ideas. Faced with some research contrary to what you currently think, it can be easy to dismiss it and move on to something more palatable. Getting to the bottom of a challenging research idea can require the Aristotelian notion of “suspending disbelief”. Your current thought process wants to reject what you read from the outset, but you suspend this reaction to give it a chance to change your perceptions. You are very unlikely to do this if you don’t trust the integrity and diligence of the author. Why would you suspend your disbelief to learn from a source you do not trust?

When you reach a confusing or off-putting part of a paper, you can say to yourself: “I don’t understand (or don’t like) this part, but I trust it will make sense later.” Overuse of this technique may give you permission to skip along the surface of a paper and never dive in. But if you never permit yourself to move forward in faith, you may never see the argument of a paper fully develop.

Gratitude

Even more than trust, it is powerful to feel gratitude toward the authors of the papers you read. Ultimately, you must be grateful for the thoughts of those who came before you. If you are truly “standing on the shoulders of giants,”

then you should be grateful for the shoulders you are standing on.

After a few failed attempts at finding a novel research direction or having a few papers rejected, it is quite tempting to start to read papers almost cynically. You might start to think, “Here is another thing I can’t do, and by the way, I don’t see how it’s any better than what I did.” These feelings are natural but ultimately unproductive. Gratitude is an antidote to cynicism.

It isn’t easy to evoke gratitude when you find someone in the research community who has already written about a cherished research idea. I (Chris) recall a time when I was working with another researcher. He had an idea that excited him and asked me to work on some technical details. Being who I am, I immediately started to look for answers in the literature before puzzling about it myself. After a day or two of going down a Google Scholar rabbit hole, I discovered the problem had already been solved in the 1950s (a relatively common occurrence for me). What happened next confused me for many years. I thought my collaborator would be excited to learn about the problem’s solution. Instead, his enthusiasm for the question evaporated. That was one of my first lessons about the importance of novelty in research. What excited my collaborator was not the question itself so much as the “newness” of the question. Learning that the question was old took all of his excitement away. At that moment, I could see how hard it was for him to be grateful to learn of the accomplishments of the past. Over the years, I have often found it difficult too.

6.4 Attitudes

Let’s now understand how attitudes are important to your capability of reading papers. Earlier in this chapter, we could not avoid discussing attitudes, so we have already started this investigation, if only implicitly. This is a general rule. It is hard to separate skills, qualities, and attitudes, as they are often mixed together. Let’s explore how taking the lens of attitude can provide even more insight.



“If you search hard enough, you will realize everything is flawed”

Most (all?) papers leave something to be desired at the end. You can take this as a sign that there are no “perfect” or even complete ideas. Everything is open to criticism. This can be both scary and disappointing. It is scary to realize everything *you* produce is potentially the subject of criticism by others. It isn’t very pleasant when your mind cannot be filled with the satisfaction of a complete and convincing answer.

To reduce some of this fear and disappointment, consider the following revised attitude: **“work goes from perfection to perfection.”** Everything is perfect in its time and for its specific purpose. When I (Chris) talk to young researchers, I ask them to keep careful notes of every stage of their project. Earlier confusions they had when starting out have their own “perfection”. This flawed idea is maybe a perfect mirror of how someone just coming to the problem might be confused. Keeping this in mind, you can write clearer papers and give more exciting talks (more on this later).

But you can view the papers you read in this same light. Just like everything has flaws, everything has perfection. When you acknowledge that you are going from “perfection to perfection,” you avoid both fear and disappointment. You become curious about how this next paper, or your next research idea, evolves the perfection around you.

“Old things cannot be relevant to new situations”

As discussed above, when we deliberated on finding the right paper to read, we raised a warning not to overlook “old” papers. One reason it might be tempt-



ing to do so is the attitude that “old” things are irrelevant to “new” situations. As we have detailed, this is not always true. There is that famous saying about history repeating itself. The same issues re-occur but in different guises. Many results have been rediscovered by different researchers in different eras.

This attitude is a general outgrowth of a societal trend toward a lack of appreciation for history. We think this perspective leads to missed opportunities. A deep understanding of history can suggest many things for the present. A popular saying among historians is that “the past is prologue”. When you deeply understand history, you can add depth to seemingly minor occurrences of the present day. In our opinion, a healthier attitude towards “old” things is the following: **“The history of our field has much to teach me.”**

“Reading too many papers robs you of original ideas”

Finding the right number of papers to read (and which ones) is a topic we have touched on earlier in this chapter. It is not difficult to see the flaw in reading *too few* papers, but what about *too many*?

We touched on this in the “Putting it all together” story in [Chapter 5](#), where Julian searched so much for answers in papers he did not allow himself space to think his own thoughts. A related but slightly different warning for over-reading is that it may also rob you of your original ideas, even if you take the time to develop them!

We have heard this sentiment many times over the years. The idea is that seeing how others have approached a problem or question can trap you in their way of thinking, and you will be unable to find your own path. The trodden path leads you astray. Analogous to this situation is when a very accomplished player of other people’s music cannot produce an original piece



of music themselves. They have played too many masterpieces to have any confidence in their own ideas.

We certainly find value in the advice of not reading too many papers. Students can and should think for themselves and on their own terms. The downside is that it can lead to stagnancy. Someone can get truly stuck and not allow themselves to find a way out. We (Chris and Runshan) sometimes think of research as more like a conversation than something “pulled out” of your mind. I (Chris), for example, have my best ideas when conversing with others. Often this is talking to someone in person or over a video call, but sometimes the conversation can be with papers themselves. It may sound something like this:

- So why did you do that?
- (reads more) Oh, I see! But that assumption seems a little strong. How do you account for that?
- (reads even more) Aha, that assumption is not weakened. Can we do something about that?
- (reads until the end) Well, I see where your ideas take you. But there is still a problem with that assumption. Where did you use it exactly? Why do you need it?
- (returns to earlier parts of the paper and continues the conversation)

In other words, I use the paper as a tool to stimulate my thought in the context of an asynchronous interaction with the authors. For social thinkers

like me, it is often far more effective than just sitting there trying to bring things out of my own mind. A more helpful attitude may be: **“Reading papers can help me develop my own ideas. Original ideas can arise in reacting to others.”**

6.5 Putting it all together

As a form of summary, we provide an example of young researchers exploring the ideas in this chapter. The story is fictional but inspired by real-life interactions with many young researchers over the years. The main purpose is to provide you with an example of how natural it can be to discuss your approach to research with those you trust.

In Dawn’s first summer after taking classes for her Ph.D., she was tasked with writing a summer paper. She had no idea how to do that. She started by looking at an issue of the top journal in her field that she took off the bookshelf of her advisor’s office.

The first paper she looked at (Paper A), which made no sense to her at all. It even made her wonder if she was reading a paper from the right field. The second paper (Paper B) was a little different. She could relate to the topic and liked what she could understand in the introduction and abstract. But when she looked at the rest of the paper, a similar feeling she felt looking at Paper A washed over her. She could not face the confusion again and did not look more into Paper B for the rest of the week. She spent her days trying to avoid thinking about research until she needed to meet with her advisor again.

Dawn told her advisor that she found a paper that sounded interesting, but she did not follow it. Thankfully, her advisor said: “Yes, that is perfectly normal. Papers are very hard to understand. But I think it is worth the energy to understand one really well; it can teach you many things about how papers are constructed, etc. Why don’t you take a couple of weeks trying to get a deep grasp of this paper?”

Dawn thought to herself: “A couple of weeks!” That seemed like a long time in the wilderness. But she was determined to give her best effort. She spent the next three weeks studying the paper.

In the process, she learned several important things. First, she needed to take another course to deeply understand what was going on in the paper. At the beginning of summer, she had hoped her course-taking days were over,

but it was clear to her that another methodology course would help her understand this paper and papers like it.

Second, although she liked the paper and had to admit to some joy in reading it, she did not ultimately feel satisfied with the paper's conclusions. The promise of the abstract and introduction did not seem borne out in the rest of the paper. The paper was made to sound more convincing than it actually was. Dawn still believed that the question in the abstract was a great one that was still largely left unanswered.

Third, despite feeling that she now roughly understood the paper (at least at a high level, if not every detail), she could not think of any way of overcoming her dissatisfaction. It was mystifying to her how she could ever improve on what was done. Thinking about how to fix or extend the paper filled her with anxiety.

Dawn felt a letdown after spending three weeks on the paper. She had no courage to start looking at any Paper C (even less than before) and tried to avoid meeting her advisor again. She felt she had nothing original to say.

After another week (the summer was now half over), Dawn's advisor finally tracked her down. To Dawn's surprise, her advisor was delighted with her assessment of the paper and wished only that she had come in sooner to discuss. Dawn's advisor agreed that the paper showed promise but was ultimately incomplete by its own standards. Her advisor suggested she extensively search the papers that cited this one to see where things lead in the literature.

Leaving the meeting, Dawn felt a little more energized. She did an online search to explore the papers that had cited Paper B. Of those, ten looked interesting and related. Dawn knew the summer would be over before she could tackle these ten papers in the same depth as she dove into Paper B. But now her focus was more pointed. She wanted to see how these papers took up the challenge left by Paper B. She read abstracts, introductions, and (when interested) a few of the results in these papers.

In the process, she realized only a part of Paper B had lived on in the literature. The other papers seemed not to refer to the parts of Paper B that confused Dawn the most. In any case, she had already registered to take the advanced methodology class in the fall and still felt it would be worth it. Dawn learned that other researchers also found Paper B's question interesting, but the results unsatisfying, just as she and her advisor felt. Each paper tried to attack similar questions in different ways. Some tried to tackle a simpler version of the question in a special setting; some studied a related question, etc.

After reviewing these ten papers, Dawn developed a deeper appreciation of Paper B and felt grateful that she had studied it so carefully. It was her “home base” in this new area of knowledge. Also, after reading all ten follow-up papers, she started developing ideas for her line of attack. She prepared a small document with her revision of Paper B’s research question and a plan of things she could try to answer it, describing how these differed from what she had read about.

When she met her advisor again, he was delighted by her progress. When he asked Dawn what she wanted to do next, she did not hesitate to share her plan. Dawn would need to search for a few more papers to read. Paper B was not satisfying because the method of analysis was flawed. While reading the ten papers, she saw a reference to a methodology paper that looked promising to her. She felt it would be worth spending a week or two reading that methodology paper to get a sense of how it might work. She felt somewhat confident it would, given how the other papers had referenced it in their work, but she needed to look deeper into it to know for sure. She also wanted to read *another* ten papers, while not being direct follow-ups to Paper B, to fill out the landscape of the research question. She felt it would give her better context for her adjusted approach.

Dawn’s advisor was delighted with the plan and booked weekly meetings for the remainder of the summer. She had a lot of work ahead of her, but at least now she had a plan.

6.6 Reflection questions

The questions below are provided for you to reflect on what you have read in this chapter. If possible, it is beneficial to discuss these questions in a group of your peers.

1. Describe an experience you had reading a paper. How did it make you feel? Did you feel confused? Did you feel empowered? What did you learn?
2. Describe some of the different ways you approach reading a paper. Do you read them all the same? Do you take notes?
3. How do you decide what papers to read?

4. What, in your experience, makes a paper worth reading in detail?
5. Have you ever doubted a result you read in a paper? If so, how, if at all, did you resolve this doubt?
6. How do you decide when to stop reading a paper and move on to something else?
7. Is it possible to read too many papers?
8. Is there a ranking of journals in your field? If so, how can you (or did you) learn about this ranking? How is this ranking determined?
9. What is the value of reading a paper together with other people?
10. In what way, if at all, do you relate to Dawn's story? What do you take away from the story?

Chapter 7

Listening

Listening is essential to all relationship-driven human activities, of which research is a prime example. There are many types of listening in research: listening to your advisors, your collaborators, etc. As the adage goes, “You have two ears and one mouth,” so you should listen twice as much as you speak. Listening is a life capability, not just a research capability.

The main focus of this chapter is exploring the capability of **listening to research talks**. Along with papers, research talks are a primary source of learning about the craft of doing research. We will focus on interactive talks, where the listener can interject with questions. Of course, other types of listening play important roles in research, and we comment on them in this and later chapters. For example, [Chapter 10](#) has an extended discussion of listening to people’s questions while you are giving a talk. [Chapter 11](#) talks about listening in the context of research collaboration.

There is, of course, a lot of overlap between the capability of reading and the capability of listening. Much of the previous chapter applies here. But, there are important differences. Papers are tightly wound and often defensive in tone (especially well-published papers). They often have deep moats and are hard to penetrate. Talks are often looser and more vulnerable. A speaker is more of an “open book” than an author.

Talks can also reveal more heuristic reasoning than papers. You typically get more images and figures, and you see how people “visualize” their work. You get sketches of arguments that do not appear in the “final record” of the paper. Talks are often fresher, touching on more recent or nascent ideas, while papers (by the time they appear in print) are more of a rear-view mirror.

Talks happen in real-time. It won't necessarily slow down for you. It is very easy to get confused while listening to a talk. If you were reading a paper, you would slow down or move on, but a talk is a social situation, so it is hard to slow things down or leave. You are forced to sit with your confusion.

But most significantly, talks are two-way communication. There are ideas presented, questions asked, and ideas defended or adjusted. This communication teaches you about power dynamics, tone, energy, conflict, bravado, contrition, and excitement. All of these give you information about the nature of the research community. This information can be invaluable on your path toward acceptance.

7.1 Purpose

What is the purpose of listening to an academic talk? Many of the purposes for reading a paper apply here—learn new tools, understand what has been done, understand what's next, learn how talks are given, and try to understand the research “taste” of others. We comment on just a couple of these in more depth.

While you can see a lot of variety in how papers are written, there are nonetheless specific well-established standards for how published papers look. As we discussed in [Chapter 6](#), there are different conventions that research communities follow that lend a recognizable style. For talks, there is a lot more variety. In several ways that we detail throughout this chapter, talks are much more personal than papers. Whereas published papers have uniform formatting, presentation slides take on more of the character and aesthetic choices of the speaker. This is not a superficial consideration. The variety in formatting can give many clues to how different researchers approach the research task.

This is beneficial to the listener who is attempting to learn the craft of giving research talks themselves. With so much variety, there are many “models” to consider for presenting your research. You can “pick and choose” strategies from different presenters. Attending an academic conference provides a crash course in different approaches, some of them conflicting or contradictory.

The variety arises because talks are often a clearer lens into the research “taste” of others than papers. Published papers can lose personality as they are filtered through the review process, while talks on unpublished work can capture more idiosyncratic enthusiasm and rawer ideas. Moreover, people se-

lect the work they present and typically only present a subset of all the papers they have published. You can learn things about the tastes of the presenter, even in how they select which paper to present.

We have heard the advice that to look productive as a researcher, you need many papers, but you only ever want to present a few of your best papers to signal quality. This gives the impression of both high quantity *and* high quality that may be needed for acceptance at certain research institutions. As listeners to talks, you can gain a sense of what others believe “quality” is.

However, there are other purposes for listening to talks that are different than reading papers. Let’s explore some of them.

Rapid exposure

As discussed in the previous chapter, reading papers is hard, especially if you want a clear picture of a paper’s core ideas and takeaways. Reading ten papers in one day would be a marathon of massive proportions. Taking in ten talks in one day at a conference is entirely commonplace. Talks often give you the high-level “gist” of a paper, and in good talks, a sense of the deep logic behind its arguments. If your goal is to get a comprehensive and quick overview of what interests researchers in a particular area, attending a session or two (or five, depending on the size of the conference) at a conference is probably the best you can do.

Ask questions

Beyond exposure to new ideas, it can even be worth attending a talk on a paper you have already studied carefully. Why is this? To ask questions of the presenter. We (Chris and Runshan) are always left with questions after reading a paper. “Why did they choose to do it like this?” or “Isn’t it easier to see it like this?” You can email one of the authors with your questions, but it is easier (and more natural) to ask them in person.

And when you haven’t read the paper before (or no paper yet exists), having the researcher there in real-time can be very useful in getting to the “informal logic” that a lot of research is built on but does not appear in papers. When you ask someone a question in real-time, they cannot always give you a thorough and tightly-reasoned answer. But that may not be what you want anyway. You are more likely to get “heuristic” answers that hint at how the

speaker thinks in broad strokes. This information can be highly insightful and inaccessible by other means.

Public display of your talent

Another reason for asking questions is to show something about your quality of thought in a public forum. If your question is particularly penetrating or insightful, other people take note. Asking questions is especially adept at showing how quickly you “think on your feet”.

We (Chris and Runshan) have had some people explain that they attend talks primarily to signal their intellectual prowess by asking incisive questions. We think this is a perfectly reasonable motivation. We agree that witnessing someone ask an insightful question is very impressive. It can also benefit everyone involved: the speaker may get a new research idea from the question, and the audience can benefit from how it is asked and answered.

I (Chris) hardly ever ask questions during a talk. I think it’s partly because I fear it backfires and makes me look silly in a public way (more on this below). But I think it also has simply to do with shyness. Both feelings (fear of looking silly and shyness) are hard to overcome.

I (Runshan) also rarely ask questions during a talk, but the reasons may differ slightly from Chris’s. First, I am usually slow in thinking and need to pay full attention to the talk just to follow. This does not give me much mental space to formulate questions. Second, if I have the conditions to generate questions, I tend to have a series of questions that cannot be quickly answered without interrupting the flow of a talk. I would feel uncomfortable taking the flow away from the speaker to satisfy my confusion. From a young age, I learned it was rude to challenge and disrupt a speaker at the front of a room. This history and background are important to me and inform my actions now. I tend to give the benefit of the doubt to the speaker that some questions may be answered by a later part of the talk. It feels more respectful to me to wait until I hear everything the speaker intends to share. For this reason, if I do ask questions, it is typically at the end of the talk.

I feel more comfortable explaining my confusion and sharing my thoughts “offline”. Intense discussion with the speaker after the talk is over, and we are standing at the front of the room as people file out; this is where I get a better understanding of the paper. This does not allow me to publicly display my “talent”, but at least most of the time, the speaker seems to appreciate me

expressing my thoughts and ideas.

Show respect

It may come off a little selfish if your primary purpose for attending talks is to look smart by asking good questions. But we (Chris and Runshan) can say, as speakers, we really do appreciate anyone asking us questions about our research. We think it is easy to underestimate how encouraging it can be for someone to show interest in the work of others. The first signal of this interest is simply showing up to the talk.

This suggests another purpose of attending a talk, which is to show respect to the speaker. Doing research and coming up with talks about that research is not easy. Doing all this work and having no one show up can be discouraging. This can easily happen at large conferences with many parallel sessions. Everyone is chatting in the hallways or attending the session of some prominent researchers, while other speakers are stuck in small rooms with only a few people attending.

Acceptance is reciprocal. By simply showing up to someone's talk, you show them some degree of your acceptance. Even if all you do is show up, this can sometimes be enough to make someone's day.

Give feedback or advice

Showing up is good on its own, but it can even be for a more respectful purpose: to help the speaker with their research. We know people that go to talks for this very reason. And we admire them. It takes a lot of energy to understand what is going on in a talk, and to do this to benefit another person is something worthy of praise. We talk more about talks as a way to get and give feedback in [Chapter 10](#) on speaking.

Generate a sense of belonging

There is also a benefit to attending talks that have very little to do with the research itself: social belonging. Academic work can be very isolating. You teach alone, and you do research alone or in small groups. Research talks are one of the few occasions that bring researchers together. This is especially

true at conferences. Having many talks happening at the same place at the same time gives us all an excuse to be together.

One purpose for going to talks is to show that you are a part of the group and to help bolster that group. The more people who show up, the better we feel as members of the group. If many people show up, we can see the vibrancy of our group. If only a few show up, we start to question the value of being a part of the group. Showing up, then, may not just be about showing respect to the speaker but respect for your field as a whole.

This purpose aligns with the “empowerment” direction of acceptance in [Figure 2.1](#). One way to become accepted is to build a culture of acceptance in general around you. Sure, you may not have the final decision about whether others accept you, but you can contribute to an atmosphere of acceptance that might tip this decision in your favor.

7.2 Skills

Some of the purposes for listening to a talk we listed above (like social belonging) don’t require a lot of technical skills. Belonging involves the skill of showing up and being happy to be there. On the other end of the spectrum, showing up to a talk for a similar purpose to that of reading a paper takes us back to many of the skills we already described in the context of reading papers in the last chapter. Here, we focus on skills that are particular to the purposes of listening to a talk we described in the previous section.

Getting valuable thoughts out of fast-moving talks

Don’t bring a gun to a knife fight. This is an English idiom about preparing yourself correctly to engage in a task, and specifically not to “escalate” the situation by being more serious (bringing a gun) than everyone else involved (who only have knives). When it comes to talks, you should not expect the speaker to give you a precise understanding of what they are doing. If you come expecting bullets, you will be disappointed by only getting a few paper cuts. However, if you are prepared to get a sense, a feeling, or a “bird’s eye view”, you will likely avoid disappointment.

When I (Chris) first started attending talks, I would get immensely frustrated. Why? Because I couldn’t figure out what was going on. As soon as

I lost track of the specifics, I would grumble to myself about how “bad” the speaker was. But I was asking too much. I was losing the forest for trees, and as soon as a tree was out of place, I would stop “looking” entirely. My old me would hate the way I present papers now.

I *still* can't figure out what is going on in most of the talks I listen to, but I now avoid frustration (most of the time). I have lowered my expectations for how much I can figure out to begin with. I focus more on the big ideas and the broad strokes of the argument. I learn about what is important in the work by noting what the speaker emphasizes and de-emphasizes. I allow myself to keep listening long after I lose track of the details. It's a bit like running around an oval with your eyes closed. You have enough sense of where you are that you keep running. Being comfortable with ambiguity and lack of precision helps you get more out of talks.

Crafting good questions

There are many types of questions you can ask when attending a talk. Here is an incomplete list:

- *Clarification questions*: “What did you mean by this? Can you restate that differently? Is this another way to understand your claim?” This is probably the most common type of question you will ask and hear. It helps the speaker to slow down and shows that you are paying attention enough to know even what to be confused about.
- *Boundary questions*: “Does your idea apply here? What if we relaxed one of your assumptions? Would the argument still hold up? What about a different cultural setting?” These questions explore the limits of what is being presented. You may hear more questions of this type near the end of the talk or after the main results are presented.
- *Connecting questions*: “What does your work have to do with Smith et al.? Isn't this just a special case of Zhang et al.? Doesn't this contradict the findings of Ahuja et. al.?” These questions help the listener to connect with things they already know. It also helps flesh out the novelty of the work.
- *Implication questions*: “What does this mean in practice? What are the policy implications?” These are more polite ways to ask “So what?”

Formulating a question can be challenging to do. The talk happens in real time, and you can be caught between formulating your question and keeping up with the flow of the talk. But even if you have a question, there is skill in asking it in a timely manner. Etiquette for when to ask a question depends greatly on the field and culture of your group. Some speakers set the expectation that they can ask a question anytime and “interrupt” them,¹ but getting their attention can be difficult at times. Sometimes you can wait until the speaker says “any questions?” It might be helpful to bring a piece of paper so you can write down your questions so you don’t forget them.

Connecting with the speaker

It’s not a machine up there giving the talk; it’s a human being. Connecting with the speaker is another set of skills that goes towards showing respect to the speaker and developing a sense of social belonging.

Nodding and actively thinking lets the speaker know you are tuned in. Trust us, the speaker always notices these things, and having someone nod along with you is very encouraging. On the other hand, falling asleep is something speakers will also see and gives a different kind of message.

Time after the talk is another opportunity to engage. It is quite daunting to approach an established researcher as a student and start talking to them. But in our experience, most speakers would be quite happy that people cared enough about their work to stay until the end of the talk and come up to speak with them. It makes the speaker feel important, which is good for everyone. A simple thing to say is:

Thanks for the talk. My name is _____. I am a student at _____.
I am doing some work related to what you talked about. Your
work is inspiring to me, so thanks.

How to take the conversation from here is quite personal. It depends on you and the speaker. But your skill in conversing with a speaker might benefit from a few insights that have dawned on us over the years.

First, asking research-related is often a good starting point for interacting with a speaker after they give a talk. You know that they are least somewhat

¹I (Chris) am not a huge fan of this language. It suggests that a question is an interruption, which I don’t think it is. Some strategies for encouraging questions are discussed later in [Chapter 10](#).

interested in the work they presented (otherwise, they would not present it), so this is a good starting point for engagement. It could mean asking some specific questions you have about the paper (e.g., “How do you identify this parameter in your model?”). Or you could share a personal experience that relates in some way to the theme of the paper (e.g. “I can totally relate to the findings in the paper, as this is exactly how I made my decision last time I was trying to take an Uber”). You can also go with general questions or thoughts related to the larger context of work presented (e.g., “I am concerned about the large percentage of commission fee charged by these platforms. What are your views about the future business models of these platforms?”) Speakers are usually happy to see that you are interested in their papers, so it is hard to go wrong with asking a question that reveals this sincere interest. These are opportunities to gain insights and knowledge from speakers that are not directly related to a particular paper.

But it is also important to note that just because we are all engaged in some form of research does not mean that all we care about is research. Don't assume that the speaker's “on” persona (“this research is the best thing ever!”) with their “off” persona. People also care about the weather, what restaurant is good, which sports teams are good, and what television shows are good. They also care about less pedestrian things like the state of the economy, the state of higher education, or the state of their academic field. There are so many interesting things to discuss that it is impossible to list them all. After you run out of research-related questions, it is natural to transition into these waters and continue the conversation. As we discussed in [Chapter 3](#), successful academics train themselves in making things interesting. Surely, two academics can find something interesting to discuss outside of research.

It is advisable to engage people as people, not just as repositories of their academic achievements. It can be off-putting to approach someone with the attitude:

Hello there, Prof. Published Many Top Papers. Can you talk to me so that I can improve my chances of publishing top papers?

Prof. Published Many Top Papers has a name. The fact she has published many papers may be the least interesting thing about her! You can only know if you engage her as a person.

Skills of belonging

If some of what we have discussed so far in this chapter strikes you as being more like general advice on how to be likable, that's because it is. As we have often stressed in this book, career success depends on acceptance, and likeability is a huge part of acceptance.

But there are also direct ways you can contribute to creating a sense of belonging and acceptance around us. As discussed above, one purpose for going to a talk is to generate a sense of belonging. How can you enhance this sense of belonging? Observational skills, like noticing connections between people, are valuable. Being able to say things like (when speaking to two other researchers in the audience before or after a talk):

I noticed that both of you are from the same undergrad institution. I wonder how your experiences were different?

This question is not about you and your direct connections with others, but it is about the general connectivity of the community.

Beyond connecting with the speaker, you can work to give a sense to other young researchers that they belong. After a talk is over, you might approach a Ph.D. student who bravely asked a question and say:

That was a great question; I wish I had thought of it and asked myself.

One of the things we (Chris and Runshan) like about academia is that there is always a stream of new people coming in. There is always someone to welcome and make feel more comfortable. If you are a second-year Ph.D. student, you can welcome the first-year students. If you are a first-year student, you can help undergrad students apply for grad schools and prepare for the challenges of research. Creating a community of support around you makes the work of doing research feel more real and consequential.

7.3 Qualities

So far, our description of the capability of listening to talks provides a much broader context than what is typically discussed. There is the act of learning from the talk itself, but also the social components of the talk environment.

The former has qualities similar to what we covered in the previous chapter on reading. We will not retread that ground here. In this section, we will focus more on qualities related to the social aspects of the research talk environment.

Let's briefly examine the six prevalent qualities discussed in [Chapter 5](#). Patience in reading papers and listening to talks manifest themselves in different ways. Papers often come with abstracts and introductions that quickly summarize the paper's findings. Talks often make you wait. In this way, talks are more like a dramatic performance—the speaker does not give away the twist or punchline at the beginning. They know the audience is committed to sitting there for the duration of the talk, so they can allow things to build.

If you go to a conference, or a speaker comes to give a seminar in your department, you may find yourself listening to talks that are not really in your area. Filling your day at a conference with talks relevant to your interests can be hard. Whereas you seek out papers to read, talks are much more likely to come to you. Thus, listening to talks exercises your quality of curiosity. I (Chris) view it as a challenge. Can I be focused and open-minded enough to be curious about *any* talk I attend? I consider this an admirable state of mind and often fail miserably at achieving it. I know of other researchers who can maintain their curiosity for a whole day of talks at a conference. It is a marvelous feat.

As we discussed above, asking questions during a talk is one of the more “public” activities of your career. It is typically different from *giving* a talk (because you can prepare in advance for this) and fielding questions you receive when giving a talk (we can partially prepare for this; see a discussion of this in [Chapter 10](#)). You often have to generate questions “on the fly” without preparation. Putting yourself out there by asking a question during a talk takes a lot of courage. The fear of asking “silly questions” can be debilitating. One of the true tests of academic courage is to ask questions during a talk. We recommend that you try it.

Empathy plays an interesting role in the context of listening to talks. Here, when you can feel the speaker's emotions, you can become excited and optimistic along with them about their research, and research in general. You can also share their feelings of accomplishment and pride in their hard work. Viewed in this light, going to a talk can sometimes be an uplifting and encouraging experience. Empathy tunes you into the fear and discomfort of the speaker if things are not going for them as they planned. This also allows

you to “save” the speaker by possibly asking them a question that brings them back to safer ground. Consider interjecting with something like this when a speaker is fumbling with a difficult question asked by an audience member:

Do you mind going back a couple of slides? Yes, here. I really like this. It took me a couple of minutes to grasp its significance, but I think you are saying this: _____. Am I seeing this correctly? I would benefit from hearing your explanation again.

This brings the speaker back to a place of confidence, you have shown that you are interested in it, and you emphasize that you care about what they have to say. This can help the speaker reset things and gain confidence to finish their talk. This goes a long way to showing respect to the speaker and building an environment of acceptance and belonging to the group assembled at the talk.

Let’s now explore some additional qualities that take on extra significance in the capability of listening to talks.

Joy

One thing that is often overlooked is that a talk can be seen as an occasion to celebrate the research that the presenter has done. As you are likely well aware, coming up with any original research is difficult. If you are presenting something, it means you have done something. This alone is worthy of celebration. Viewed in this light, every talk can be an occasion for sharing joy. It is a joyful experience to see someone skilled talk about something they have invested so much time and energy into. It is joyful to see others take the time to appreciate that display of skill.

Wonder

Although we (Chris and Runshan) strive to experience joy in every talk we attend, we must admit it is not easy. This is partly due to our normal state of being confused. But even in these states, we find it possible to conjure feelings of wonder. Listening to a talk is an opportunity to show our wonder at the talent and progress of our fields. We find ourselves thinking like:

Wow, that literature review shows that this problem has roots back to the 1950s but has exploded recently. Isn’t it amazing how knowledge progresses?

or

I would have never thought to go down this path. And yet these researchers seem to have dedicated a big chunk of their lives to it in recent months or years. That is truly stunning.

By maintaining wonder, we can be awake to possibilities, recognize greatness, and see opportunity.

Respect

We described above how showing respect to the speaker is one of the purposes of listening to a talk. Respect is itself, a quality of vital significance for research. Having respect in your heart is one thing, but expressing that respect is another. Consider the skill of asking questions during the talk. Respect plays a pivotal role in how questions are received, and different ways of approaching a question reveal respect to different degrees. Consider the following question:

Yes, but does your work apply to this important setting?

This implicitly presumes that the speaker did not think about the implications of their ideas in another important setting. The question is somewhat accusatory of the work being incomplete. A more respectful approach might be:

What are some of your thoughts on applying this work to other settings?

It leaves open the possibility that the presenter might have already thought about the other setting. It is more respectful and less accusatory.

The purpose here is not to split hairs with the English language but to get to the underlying sentiment of words. If you have respect in your heart, consider how your words reflect that sentiment.

Unity

Unity is an interesting quality. It speaks to a kind of acceptance that brings people together. Someone strong in the quality of unity is not only accepting of others but allows people around them to find it easier to accept each other.

The purpose of going to a talk to create an environment of social belonging would be significantly enhanced by the quality of unity. This quality makes it easier for people's hearts to navigate towards common ground rather than distinction. The following statement from the Universal House of Justice expresses this sentiment better than we ever could:²

...the merit of your every contribution lies, first, in your resolute commitment to discover that precious point of unity where contrasting perspectives overlap and around which contending peoples can coalesce.

One of the great “divides” in academia is the tiered systems of the universities themselves. I (Chris) remember hearing about a conference organized for Ph.D. students from universities in a small region in a particular academic field. They listed the universities involved, and I noticed that more than half of the Ph.D.-granting institutions in this region were not invited. Evidently, it was a conference organized for a particular “tier” of university and not for *every* Ph.D. student engaged in research in that field. Someone strong in unity would see beyond these distinctions and look to what unites. Isn't it enough that we all study the same (often quite niche) research area? Why can't we acknowledge our natural attraction to others who spend their days doing roughly the same things as us?

Sincerity

But arguably, the quality that stands out as most relevant to the social side of the capability of listening to talks is sincerity. Feigning joy, wonder, and respect is easy to spot as insincere. Luckily, there is no need to practice insincere qualities. As we saw in [Chapter 5](#), qualities can be expressed in degrees. There is always something to base genuine joy, wonder, and respect on. As you cultivate these qualities, finding them within a place of sincerity in your heart becomes easier and easier.

I (Chris) remember with great warmth a friend I knew when I was around 16 years old. He was a bit older than me (maybe 20) and showed much affection for me. I was astounded by how much joy he could conjure when he

²Taken from a letter written 25 November 2020 available online here: https://www.bahai.org/library/authoritative-texts/the-universal-house-of-justice/messages/20201125_001/1#300076430

saw me. Everything I said seemed to fill him with wonder. It felt entirely genuine; I never once doubted his sincerity. I have to admit, though, I found it a little confusing. At first, I thought I had this unique and special bond with him, like I was his prized protégé on whom he showered his most positive energy. Later I realized that he shared a similar bond with many other people: men, women, and children of all ages and backgrounds. When I first found out about this, I think I felt some jealousy, like I wanted to be the only special one. I thought that this realization would color how I felt about him. But it never did. His joy and wonder about me always remained sincere and grounded. Looking back now, I realize it is not confusing at all how he interacted with me and the others he knew. He was simply someone deep in qualities of character with skill in expressing these qualities in his interactions with others.

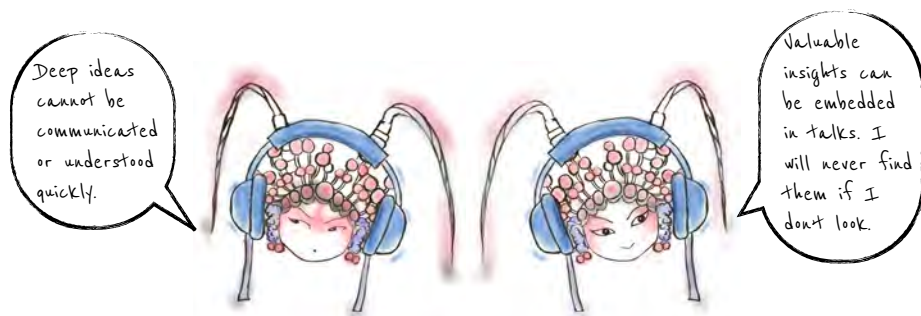
7.4 Attitudes

It is fitting to emphasize sincerity (as we have in the last section) because your attitudes can greatly color how strongly you can hold onto and express your qualities. I (Chris) can already anticipate some readers seeing my story of a friend who impressed me with his sincerity and thinking to themselves:

This friend of yours wanted something from you. Maybe he had a strong desire to be admired.

This is certainly consistent with the worldview that everyone seeks to maximize their desires, even if those desires are not as “admirable” as my friend in the story. Under this worldview, sincere interactions with me could have been sincere in some short-term sense, but the ultimate goal was for him to be recognized and admired by people like me some twenty years after the fact.

This reading is hard to dispute, but my heart persuades me to believe something different. This is exactly the point. Attitudes are about what you believe, and this belief changes your perception of reality. Belief is, in many instances, beyond proof. Let’s examine a few instances in the context of speaking.



“Deep ideas cannot be communicated or understood quickly”

We (Chris and Runshan) have often asked ourselves: “Why go to this talk? I won’t understand and will have to read the paper anyway to get the idea.” This misses the point that talks can provide heuristics of deep ideas, which are sometimes precisely the insight you need to make sense of things. We prefer the attitude: **“Valuable insights can be embedded in talks. I will never find them if I don’t look.”**

The best example is how few figures and images you find in papers compared to how many pictures are found in good talks. The fact that a talk constrains how much attention the speaker can imagine getting from the audience (i.e., the duration of the talk or less) makes figures and pictures more appealing. We will discuss more the power of figures in [Chapters 9 and 10](#).

I (Chris) remember attending a talk related to a paper I had been struggling with reading for some time. In the first few minutes, the speaker put up a nice geometric representation of the phenomenon I had been puzzling over. It clarified things to such a degree that I always start my reasoning about that subject by drawing out the picture from those slides. That mental image was well worth the time I spent in the talk—it has repaid me many times over.

“Asking a silly question makes me look stupid.”

As discussed above, asking questions takes courage because there is a downside risk of looking silly if you ask a silly question. We don’t deny that such a risk exists. Despite the truism that “there are no stupid questions,” the reality can sometimes be a little different. Depending on the culture of the academic group you are in, this can be more or less true.

One salient feature of that culture is its stance toward fixed mindset and



growth mindset thinking. This distinction was made popular by Dweck in her book [5]. A fixed mindset is one where people’s abilities are difficult (if not impossible) to change. In this mindset, asking something stupid is a signal of the underlying stupidity of the asker. This can be a dangerous prospect if the social environment highly values “intelligence” as some innate character (academia is a pretty good example of such an environment). By contrast, a growth mindset acknowledges that people can grow and develop their talents over time. In this mindset, asking something stupid is not so damaging, as it shows that the asker is on a courageous path of improving themselves.

While there is a risk of asking a stupid question, there is also a risk of *never* asking a question. Never asking anything can communicate that you are disinterested in the academic field in general, and the given speaker, in particular. In the journey towards acceptance (as detailed in [Chapter 2](#)), not finding anyone interesting enough to ask them about their work may indicate that you want to belong to the wrong group. Of course, there is another explanation for not asking questions: you are shy. I (Chris) ask questions infrequently for this reason. If are not able to conjure a question, try nodding and making eye contact with the speaker—some way to communicate to the speaker that you care about them and what they are saying. A healthier attitude might be: **“Asking questions (or otherwise acknowledging the speaker) is a way for me to show that I belong to this community.”**

“Everyone but me follows this talk. There is something wrong with me”

One of the criticisms of social media is that it makes it appear that everyone other than you is having a great time. Few people make posts about how they



feel aimless or lost. A similar situation happens in academic talks. Few listeners talk openly about how they don't understand what is going on in the talk. But let us say this—if you are confused, there is a near certainty that someone else is.

As we have discussed earlier in this chapter, understanding occurs at many levels. People nodding does not mean they understand everything being said, but maybe just enough for it to be meaningful to them at some level. You can nod through relative confusion. You can strive to be confident in what you know and openly admit what you don't.

The reality is that every academic field is very broad. New research, by definition, needs to be new. New things are often hard to understand. It can take years for a new idea to reach a point of clarity. In this sense, it is okay to be confused in a talk. It is potentially a sign that the work is new and exciting. We find this revised attitude helpful: **“It is okay to be confused. There is no end to what I can learn about my field.”**

7.5 Putting it all together

As a form of summary, we provide an example of young researchers exploring the ideas in this chapter. The story is fictional but inspired by real-life interactions with many young researchers over the years. The main purpose is to provide you with an example of how natural it can be to discuss your approach to research with those you trust.

When Milton first started his Ph.D. program, he never attended talks. He would tell his peers: “those presenters are all so bad, it's not worth my time, and besides, I am not interested in any of those topics.” But deep down, Mil-

ton knew he was avoiding the talks. Milton was a methodical thinker and always felt overwhelmed when he was sitting in the talk and did not know what was going on. He felt it was better to avoid the talks than have the anxiety of being lost; he did not want it to erode his confidence.

One day Milton was sitting down to eat lunch in the Ph.D. lounge with Lucia, a fourth-year Ph.D. student who got to know Milton during his first year of the Ph.D. program.

Lucia: You should really come to the talks. I promise you it will be worth it. We have had some great speakers recently.

Milton: No, I'm okay. I use the seminar time to work on my research. I feel it's a better use of my time.

L: There is always more time for research. When these speakers visit our department, it is a real opportunity for us.

M: Maybe an opportunity for you, but I can't make sense of anything they say. I don't want to sit there looking confused.

L: I find that hard to believe that you can't make sense of *anything*. At least a few ideas must come across, no?

M: Are you trying to tell me you understand everything they say? I find that hard to believe.

L: Just wait a minute, I never said I understood *everything*, just that I understood *something*. I am sure you would as well. All that matters is that you catch a few ideas from the speaker. Sometimes these thoughts can be pretty insightful.

M: But I can just read the paper to figure out what they are saying. What's the point of taking all that time just to learn *some* disjointed ideas from the speaker?

L: You must be more skilled than me at reading papers; I find catching the main ideas of a paper to be one of the hardest parts. I see a lot of details there but not so many ideas. I would like to have someone excited about the work explain their sense of the main idea to me.

M: I also get confused about why they are so excited. Much of this research is incremental, no? We are not even sure if it will ever get published.

L: I don't think we can belittle someone's excitement. You must know by now, as much as anyone, that maintaining enthusiasm for research is one of the hardest parts of doing research. I find their enthusiasm infectious. I mean, we are all in this together, aren't we? Even if their work is incremental, it's progress.

M: I guess I don't feel that kind of connection to the work. I am not sure how excited I am about my research these days.

L: Maybe you should come to the seminars to feel more connected. I find it energizing to be around people trying to do some similar things to me. Even if you don't understand or find the talk interesting, you can chat with the faculty and other students. Maybe you will be inspired.

M: Maybe you are right.

In the weeks after this conversation, Lucia started going by Milton's cubicle before every research seminar. Reluctantly at first, Milton began to join at Lucia's insistence. She could be very persuasive. Lucia and Milton went together to the remaining seminars before Lucia's graduation. Milton never raised his hand to ask a question. Lucia did on occasion, for what Milton thought were obvious questions, but no one seemed to mind. Milton just sat and listened but started to find that he was more and more engaged.

Over time, Milton became fascinated by the question of why the speakers seemed to be so excited by their work. Some of the excitement did not feel genuine, but he could tell that some of it truly was genuine. He noticed that some speakers got excited when diving into the details. Others focused their enthusiasm on interpreting their results. Others seemed more excited by the research question than even their results. Milton started to understand better what motivated people to do their work. He also began to appreciate the effort speakers put into their talks.

One time a speaker came to talk about a paper that Milton was reading for his research. He was amazed at how clearly the speaker could explain the paper's main ideas. While Milton did not learn anything really new about the paper's contents, he certainly learned a thing or two about how to describe what he had learned.

Milton kept going to the seminars every week, even after Lucia graduated. Milton even became the student who would round up students before the seminar and invite them to join.

By Milton's fourth year, he came to look forward to the talks. He started to ask questions, and everything made more sense to him. To his surprise, he even started to make some sense out of papers very different from his research area.

In Milton's final year, Lucia came back to give a talk in their seminar series. She had worked for a couple of years as an assistant professor at a university on the other side of the country. Milton had not seen her since she left and was eager to reconnect.

During Lucia's talk, Milton raised his hands for questions several times, patiently listening to Lucia's responses. After the seminar, Milton and Lucia had the opportunity to have lunch together and catch up.

Lucia: Milton, seeing you in the audience was so nice. I must say, you seem to have learned a lot since I last saw you. You had really great questions.

Milton: That's nice of you to say. I am not sure I know much more than I did before, but now I am less worried if I look stupid, I guess. I find if I am in the mode of thinking of questions, I find the seminars more fun, and I get more out of them.

L: That's great. Are you this way for every speaker? We do similar research, so maybe you had a lot to share. But what about those with very different topics?

M: After a while, I stopped worrying about whether the talk was relevant to my research and whether the speaker would be any good. It became part of what I do as a Ph.D. student. I attend talks to be a part of things. Asking questions has just become a natural part of "showing up" for me.

L: You know, it's greatly appreciated from the speaker's perspective. I have given talks at a few places where no one seems interested, even falling asleep. It isn't very encouraging.

M: Yeah, I can see that. Research can be pretty disheartening as it is. I just got a paper rejected—the first paper I ever completed. I would hope

that at least someone, somewhere, would be excited to know about what I spent all this time doing.

L: Certainly. I was pretty impressed with your questions. How did you come up with those?

M: Yeah, I am not sure myself. I guess I was curious and wanted to know. I still feel overwhelmed by the talks and understand only bits and pieces. I guess what's changed is that I believe it's worthwhile to understand something even incompletely. Who knows later if it may inspire me.

L: What a wonderful idea my work may inspire someone!

Milton and Lucia finished their lunch by catching up on the whereabouts of their peers who graduated after Lucia left. Lucia needed to leave for some one-on-one meetings with the faculty. Milton and Lucia left with mutual gratitude for what they had done for each other.

7.6 Reflection questions

The questions below are provided for you to reflect on what you have read in this chapter. If possible, it is beneficial to discuss these questions in a group of your peers.

1. Describe an experience you had listening to a research talk. How did it make you feel? Did you feel confused? Did you feel empowered? What did you learn?
2. How easily do you get lost in a talk? Once you are lost, what do you do?
3. At a conference, how do you decide which talks to attend?
4. Do you attend all of the research seminars in your department? Why or why not?
5. Have you ever asked a question in a research talk? If so, describe what happened.

6. To what extent did you feel united with the audience of the talks you attended? To what extent did you feel united with the speaker?
7. What unites you and your close friends?
8. What actions lead to unity? What actions lead to disunity?
9. Do you ever talk to your peers about a talk you just attended together? If so, what do you talk about?
10. In what way, if at all, do you relate to the story of Milton and Lucia? What do you take away from the story?

Chapter 8

Creating

For our purposes, “creating” mean bringing something into the world that was not there before. Of course, we have already touched on this in earlier chapters. To “dig” into a paper, you need to read between the lines by creating your own thinking. When listening to talks, you must create questions, often on the fly. Nonetheless, this chapter represents a transition in our exploration of the research capabilities in **Part III**. While we have touched on creating in bits and pieces until now, reading and listening are primarily about taking things in. This chapter starts a more deliberate process of coaxing things out.

You might feel that creating is the hardest part of doing research, and yet we only dedicate one chapter out of twelve in this book to it. Why? There are a few reasons for this. First, it is in the act of coming up with the *content* of research that things become much more field-specific. Creating in some fields may require access to lab equipment. In other fields, creating might require interviewing a bunch of people. Still others, creating requires stating and proving new mathematical theorems. This book is aimed more at the *form* of the research process that applies broadly across fields. To maintain this generality, somewhat ironically, we will have relatively *less* to say about actually *doing* research (coming up with the stuff you put into papers).

Second, of all the capabilities, it is quite plausible that *creating* is the most mysterious. Indeed, bringing something out of nothing is wonderfully surprising. The opening episode of the *Daodejing* by Laozi (see, for instance, [1]) reads:

故常无欲，以观其妙；常有欲，以观其徼。此两者同

出而异名，同谓之玄，玄之又玄，众妙之门。

Translated (imperfectly) into English, we have:

For it is through the constant alteration between Nothing and Something that the wonder of the one and the limitation of the other will be seen. These two, having a common origin, are named with different terms. What they have in common is called the Mystery, the Mystery of Mysteries, the Gate of all Wonders.

Much cannot be said about the process of going from Nothing to Something. We air on the side of caution and say less than more.

Third, there is a case to be made that creating is the most individual and subjective of the research capabilities we discuss. This is because, in a certain sense, creating is the most *private*. The other capabilities result more in the context of interacting with others. Reading and listening ask us to conform ourselves to the thoughts of others. Writing and speaking require us to conform to the accepted rules of grammar and decorum. Collaboration is built entirely on accommodation and teamwork. On the other hand, creating at some point, and in some way, involves you in a room alone with your thoughts.

For the above reasons, we stick to the broad strokes of creating in a research context. We hope that the insights discussed here apply generally—across fields and individuals—but also leave enough unsaid to honor the mysterious nature of the creative process.

Finally, we want to say about the choice of the word “creating” for this capability and not something like “discovering”. Our experience of doing the content-generating part of research has always felt more like creating something rather than uncovering something about the world. This may partly be due to our fields (operations research, operations management, marketing, and information systems). But creativity nonetheless plays a pivotal role even in disciplines where “discovery” or “uncovering” feel more accurate to describe the process (such as, possibly, the physical sciences). In setting up an experiment to discover something about the world, you still need creativity to imagine what that experiment should be. Creativity is also significant for interpreting what you observe about the world. The result of an experiment is never wholly unambiguous. It always demands some creativity in interpretation.

8.1 Purpose

This section was maybe the most difficult for us to write in the whole book: what is the purpose of creating? In a way, [Chapters 2](#) and [4](#) have already answered this question. The whole book is centered around the idea that success in a research career is contingent on being accepted by the research community. Thus, in a very direct sense, a purpose for creating is to be accepted. [Chapter 4](#) discussed additional reasons for researching beyond acceptance. But again, these were more focused on the consequences of that research—what would be discovered, shared, or used to support a career.

What more, then, can be said about the purpose of creating? At a high level, the purpose of being a human being is connected to producing things. You are not designed to just consume. A Ph.D. is a point where a learner transitions from consuming knowledge to producing knowledge. One of the key ingredients of producing knowledge is bringing something out of yourself. If you don't add anything that comes from you, it is just rearranging what others have done. If it does not come from you, then the fact that *you* are doing it loses its significance. A purpose of creating is to find an expression of your authentic productivity.

But this exploration of creativity is not selfish or hedonistic. The hope is that the things you bring out of yourself fill a gap left in the hearts and minds of others. Maybe the purpose of creating is to complete each other. New empirical evidence, a new theorem, a novel methodology, or a theory to explain a phenomenon can be meaningful to both the creator and the recipient. At a minimum, your creativity may inspire others to bring something out of themselves. This sense of value your creativity has for others is another source of purpose.

8.2 Skills

As we discussed at the outset of this chapter, the skills of creating are the most discipline-specific of any we discuss in this book. Luckily many of these skills are covered in Ph.D. courses. But even with this training, many of us find it difficult to consistently engage in the act of creating. The skills we discuss here are those we have found helpful in maintaining consistent effort in creating.

Defining and documenting progress

One of the challenges of creative work is that it often feels amorphous. There are no clear steps to creating a masterpiece. You have some sense of the outcome (a published paper), but it is harder to understand what the intermediate steps entail. This makes it hard to define progress. Suppose you wake up every day with the same “to do” list:

- Come up with a research idea.
- Come up with interesting results.
- Write a paper.
- Publish a paper.

So, what are the daily tasks? You need to be thoughtful about how to break things up into component pieces that fit a daily (or even hourly) time frame. These steps need to be meaningful enough but not overwhelming to tackle. Here are some potential measures of progress that we have found helpful:

- *Document time spent puzzling.* “I spent one hour today thinking about how to solve the problem.” “I will spend 30 minutes this morning thinking about that thing I am stuck on. Whether I get anywhere or not, I will spend 45 minutes of uninterrupted time thinking about it.”
- *Articulating next steps.* “I spent 20 minutes making a plan of how I am going to spend my energy this week.” “I wrote myself a note that articulates why I am stuck.”
- *Conditional thinking:* “Today, I was able to articulate why my current step is so important to the progress of this project. If I solve this, then many other things will become clearer or easier.” “Today, I learned that the step I am currently stuck on is not needed if I slightly change something I did before.”
- *Turn vague thinking into words* “Today, I could articulate my research idea in words.” “Today, I identified a set of keywords to search for in the literature.” “Today, I could clearly state why my previous attempts failed.”

- *Gaining alternative perspectives:* “Although I did not solve my problem today, I rephrased it in a different language that could be useful.” “Today, I learned that the dead end I tried last week may still have some potential. Given what I have learned this week, I was thinking about that direction in too narrow a way.”
- *Learning to break things up into smaller steps.* “Today, I learned that the next stage of the work can be broken down into five smaller steps. These steps can be tackled independently.”
- *Communicating with others:* “Today, I prepared to meet my collaborator. I have some things to show.” “Today, I emailed my collaborator with a question.”

Regarding the last point, I (Chris) used to send many emails to my professors during undergrad ...or at least I *started* a lot of emails. Something kept happening. By the time I wrote an email explaining where I was confused or felt the assignment had a typo or was unfair, I very often resolved my confusion and found a solution in the process. I believe there were a couple of factors that explained this. First, when I imagined another person in my head (the professor), I started to feel less alone and isolated. This thought was often enough to cheer me up and get my mind thinking positively again. Second, writing the email got me in a mode of explaining rather than just dwelling on my own stupidity. It started me down a path of saying the things that I knew rather than focusing on what I didn't know. Building on this knowledge, I could slowly walk towards a solution.

Redefine setbacks as progress

It is not always possible to make forward progress toward your goals. The process of creating is a dance with forward steps and backward steps. For example, you might think you have a very nice result on which to center a paper, only later to realize that you made an error in the initial stages of the argument. An idea that once excited you weakens as you recognize its limitations. What you thought was a novel idea was done (or at least strongly anticipated) by earlier work.

These can be very discouraging, especially for a young researcher looking for their first few research victories. One valuable skill is being able to redefine

these setbacks are progress, but possibly from a different lens.

One common (and often necessary) approach to research is “truth via exhaustion”. If there are ten possible explanations for a phenomenon, one way to find the right explanation is to show that the other nine are wrong. Uncovering the *wrong* idea is progress—it brings you one step closer to the right idea. Understanding very clearly why one approach is wrong can also help eliminate other paths from consideration. For this reason, spending a lot of time carefully understanding a wrong path is not time wasted.

An effective strategy to stay optimistic is to build “win-win” scenarios. Let us give a few examples. Suppose you are working on a mathematical modeling paper, where you are using a math model to provide insight into some phenomenon. A given mathematical statement is either true or not. Suppose that the statement is something you think a reader will expect to be true. If you can prove the statement is true, then this is a nice result because the mathematical model confirms intuition. It makes the model feel more natural and believable and helps build a case for generality and usefulness (going back to the aesthetics discussed in [Chapter 3](#)). On the other hand, if the statement is *not* true, then you have something counter-intuitive. This counter-intuitive result could be an ingredient in establishing surprise and novelty (again going back to [Chapter 3](#)). Both outcomes are a “win”.

Here is another “win-win” construction. When presented with a problem, standard approaches to resolving it will work, or they won’t. If standard methods work, you have solved your problem. This can only be a good thing. At least you have solved one problem, which is a step toward building a paper. On the other hand, suppose standard approaches fail. This also adds to the richness of your story. You can write:

You might think that the standard approach works here. But this thought would be wrong, and here is why ____.

Differentiating your methods and ideas from standard ones builds a case for novelty and difficulty. Again, you have a “win-win” scenario.

We leave this subsection with an even more basic sense of progress in the face of challenges. Sometimes, creating is so overwhelming and exhausting that even trying should be counted as a victory. Suppose you can say something like:

Yesterday I got my paper rejected. Despite this, I found the energy to think about research for a few hours today

This is progress. We included a quote at the beginning of this book, taken from the pages of the *Romance of the Three Kingdoms*:¹ “谋事在人，成事在天。” The English translation is “Process belongs to humans, outcomes belong to the heavens.” The fact that you continue to try to create is, in the end, all that you can do. The success or failure of that effort is out of your control. In this view, effort and movement are the only kind of progress we can make.

When I (Runshan) first started doing research, I often got frustrated that I could not solve problems and make progress as planned. Sometimes the standard methods could not be applied to my problem because of seemingly minor differences in the models. Sometimes data showed contradicting patterns, and no conclusion could be easily drawn. Sometimes exciting new ideas were not supported by the data. There was a time when I was too frustrated by all the “failures” that I stopped doing anything. After all, what’s the difference between not trying and trying and failing? Over time I learned that those “failures” could also be meaningful. Sometimes they led to inspiring new ideas; sometimes, they presented intriguing puzzles that enriched the stories; sometimes, they guided me toward the correct path. In any case, they marked my discovery process and helped me better understand the problem at hand. This positive view has helped me go through many challenges.

Use different energy levels and moods to tackle different tasks

One of the modern-day complaints about the educational system in North America is what is called a “trophy culture”. The idea is that everyone who shows up and tries gets a trophy, not just the winners. For research, we are believers in the trophy culture. Research can be such a disheartening and frustrating endeavor that regularly showing up to do it is worthy of great respect. As we just described in the previous section, one of the most significant signs of progress in creative work is the fact that you are engaged in it.

We have learned over the years that to put regular effort into the creative work of research; you need to think about your moods and energy levels and how these match different research tasks. For instance, coding takes intense focus and attention to detail. Generating new ideas requires moving away

¹For a recent printing of this classic, see [8]

from detail toward more expansive vistas. When in the middle of a complex argument, you can't be distracted by "minor issues" that might take you out of your chain of thought and diminish your clarity of mind. But when recording your ideas in notes or manuscripts, you need to make it intelligible for when your mind is less engaged and where unaddressed "minor issues" can start to build up into confusion.

All of these tasks are quite different. Depending on your mood and energy levels, some are easier to tackle than others. You can't do everything at once, which is a good thing.

I (Chris) like to use mornings to write out my ideas. This is when my mind is most active in making connections. When writing things out, I like to think of all possible ways someone can get confused by what I am writing and address these confusions in some way. I find this work harder to do later in the day. As the day goes on, I become increasingly obsessed with certain things that bother me. This mood lends itself to more focused tasks. I might say: "Why don't I put down everything and just try to resolve this." If I do this too early in the day, I often feel I did not cover enough ground. I feel derailed if I get into something tricky and forget about lower-hanging fruit. Nearer the end of the day, when my mind is tired, I like to get up and go for a walk. These walks are an ideal time to let my mind wander to think about the next steps or fresh ideas. This pattern helps me not to feel like I am wasting the wrong energy doing the wrong task. There is always more time and different types of energy to come.

I (Runshan) usually have high motivation in the morning, so I like to use mornings to tackle challenges, such as analyzing a model, writing, or testing a complicated piece of code. This is the time when I stay curious and optimistic. Even when things do not go as expected, I do not feel too defeated. It is much harder to have the energy to stay upbeat and keep going later in the day. I become more easily distracted in the afternoon, and I usually use the time to communicate with others. This is the time when I like to meet and write emails. Engaging with people usually helps me maintain my focus. My energy level is low in the evenings, but my mind is clear. It works best if I work on things that require focused attention but do not involve a lot of uncertainty, such as writing, documenting the day's work, or learning new skills.

Of course, I cannot spend every day as desired. Sometimes I have to spend all day writing and debugging code. Sometimes I do not have the energy to do anything difficult at all. But knowing the patterns of my energy shifts in

a typical day and being aware of my mood has helped me make better use of my time.

Allow inspiration to sprout

One of the truisms of creative work is that ideas and flashes of insight come when you least expect them. Almost everyone we have worked with has a story of getting a resolution to a research challenge while doing something entirely unrelated. Hadamard (a well-known mathematician) interviewed a group of other prominent mathematicians to write a book on mathematical creativity [9]. One of the common themes was that to invent a mathematical idea, you need to work very hard and then let that thinking go entirely. Then you are taking a shower, and *pow* your problem is solved.

In this view, creativity is like planting a tree. First, you need to dig the soil and place the sapling. This creates the conditions for growth. But then you need to wait. It might seem like nothing is happening for a very long time, but new leaves eventually sprout.

One of the difficult parts of this skill is acknowledging that creativity in research is a mysterious process. I (Chris) have worked with co-authors who are masters at embracing this philosophy of planting seeds and waiting. In a recent project, we got a very tough referee report that I thought would be impossible to address. But my co-author was not deterred. As she always does, she thought long and hard about the problem. However, she does not burn herself out by pushing into hopeless dead ends. She plants many seeds and comes back to water them occasionally. She works deliberately but not obsessively. Finally, eight months after getting this tough report, one of her ideas started to provide a great deal of clarity. In the end, we could address all of the referee's concerns. I would have given up months ago if it was not for her seed-planting efforts!

Another place where inspiration may unexpectedly come is in casual conversations. I (Runshan) like to share my ideas and problems with my friends (both academic and non-academic), who know little about my research. I find it easier to get inspiration during those informal chats than to think hard on my own. This is probably because, in those chats, I am usually more relaxed and allow myself to focus more on the key issues and be less bogged down in the details. Explaining to someone with little prior knowledge gets me to the core of what is interesting and important about my problem. My

audience connects my idea to other ideas that they have. Given their varied backgrounds, these connections can be surprising.

Work backward and forward

One of the most challenging impediments to creativity is the “blank page”. You are sitting there, and you have an empty page in front of you, and you need to write down something clever. The “blank page” perspective looks at creating in the forward direction: you start from nothing to build something that was not there. There is a blank page, and you begin to fill it. Another perspective is to work backward. Instead of thinking about the start, think about the end. You might ask: “What filled page would I be happy with?”

But how can you start thinking about “the end” when you may not know where that is? One approach is to think about a simpler but related problem than the one you are working on and then solve that simpler problem “to the end”. This gives you a sense of a roadmap to follow for the problem you eventually want to solve. It may follow a similar path with a few more detours.

Another way is to assume you are part way to the end: “If somehow I got *there*, then it is much clearer how to get to the end. So why don’t I assume that I am there.” This may help you fill up the bottom half of a blank page, making it easier to fill out the top half later.

Of course, another problem is finding yourself partway to your goal and realizing where you are currently stuck is not a “stop” along the way to solving your problem. All the roads forward are treacherous. This is a time to think about going backward rather than forward. A useful question in the task of creating is: “How can I *not* keep getting back to where I am now?”

One thing I (Chris) have thought a lot about is the nature of the destination. How can I recognize when I have arrived? This may sound like a silly question, but in my experience collaborating with researchers over the years, a challenging topic is deciding when a project is “done”. If you are accustomed to looking forward, you can always see more things to do. The horizon gets wider and wider. The discipline of “looking backward” helps give us a sense of when you have traveled far enough. If the story, both backward and forward, feels clear and exciting, I see this as a good sign that I am done and can start writing things up.

8.3 Qualities

At the beginning of the chapter, we described how creating is both mysterious and also quite personal. When creating, you must inevitably sit with yourself and look inside. This process can be both uncertain and a little uncomfortable. Who knows what you will find inside? What if you don't find much?

The six prevalent qualities we featured in [Chapter 5](#) continue to play an important role in the self-revealing process of creation. Patience is needed to nurture an idea from inception to maturity. One of the easiest ways to become frustrated in the creative process is to expect things to come quickly and easily when, in fact, that path is long and treacherous. But for the skill of allowing inspiration to sprout, hope is equally as important as patience. It takes tremendous hope to wait for a whole winter for some green leaves to sprout. If you lose hope that any of these ideas will bear fruit, the task of creating is unbearable.

Up until a Ph.D., in many disciplines, creativity is not heavily emphasized. There are still so many courses to study and things to know. How could you possibly be ready to come up with your own stuff? And then, one day, you have to. This transition from absorbing knowledge to generating knowledge takes tremendous courage. It takes courage to bring something out of yourself, to stand behind it, and show the world. You cannot always wait for the perfect thing to come out that you know everyone will be impressed by. Sometimes you need the courage to stand behind the imperfect and contingent things that come out of you. This is a test of true courage. Let's examine a few other qualities.

Detachment

It takes courage to show something of yourself to the world. But it is important to remember that you are not what you create. I (Runshan) used to put a lot of my identity into my work. When I had setbacks, I would begin a seemingly endless cycle of self-doubt. This prevented me from doing work and making progress. One day I read somewhere that "you are not your work". It struck me, and I came to realize that my work does not wholly define me. Indeed, I would not judge the other people around me by their work. Why would I judge myself? Realizing the difference between the output of my work and me as a person not only helped me focus better on learning and

improving but also reduced my anxiety level.

My (Chris's) experience is a little different. I always found it surprising that others would think that the fullness of my interests and thoughts are contained in my research. I recall talking to a young researcher who said he felt it was always best to prepare when meeting an experienced researcher by reading their research and having comments or questions about it. I told him (to his surprise) that this was something I would decidedly *not* like. My research ideas are primarily in my paper and my talks. Those are, in a sense, already public. When I meet someone in person, I would rather talk about my other interests: sports, music, history, philosophy, etc.

Detachment allows you to pour energy and hopes into your creations and then let them go. If others criticize what you have created, that is fine. They are not criticizing us, they are criticizing our work, and the work is not us. Of course, it is not always so simple. Sometimes people go beyond the work to criticize us as researchers. They might say, "This work is poor, and that is evidence of a poor mind." But I (Chris) like to think that even these critics are at most pointing out flaws in that narrow part of me dedicated to my research career. There are dimensions of the real Chris that they can never reach in their critiques. They are noticing, at most, flaws in "research Chris". I am much more than "research Chris".

Orderliness

The concept of detachment goes even further. If, like the quote says, "outcomes belong to the heavens," all you are responsible for as a researcher is the research process. Accordingly, you should take the research process (particularly the creative process) very seriously. As we have emphasized above, one of the challenges here is giving structure to this process. Creating involves many challenging and amorphous tasks. With so many possibilities for diversions and distractions along the path of creativity, having an orderly mind is essential. A chaotic mind will find it difficult to piece together the long chains of logic, effort, and adjustments needed to finish a research project.

One strategy I (Chris) use in maintaining "orderliness" in my work is what I call my "big board" spreadsheet. In this spreadsheet, I keep track of the projects I am working on, what stages they are in (developing ideas, writing up results, preparing for submission, revising for resubmission), and what steps may be needed to move projects forward. I like to include even com-

pleted projects here to remind me that I have done all of this before, and it worked out. It makes it feel that it is just a matter of time to complete the other projects on the board. Another feature of my “big board” is that it prevents me from forgetting about one project while I work on another. Having everything laid out this way helps me stay organized in targeting my effort throughout the day.

What I have learned over time is that one of the biggest challenges for me in doing research is being able to clear my mind of worries and doubts so that I can feel good about “wasting time” just thinking and trying to create. Doubts and worries continually evolve in my thinking. For example, suppose I am trying to prove Conjecture A. In that case, I need to fight back doubts about whether Conjecture A is worth proving and why I am not spending time on Conjectures B, C, and D. If I am using Approach 1 to tackle Conjecture A, my mind somehow cannot let go the tantalizing possibility of trying Approaches 2 and 3 even before I have given an honest effort at Approach 1. The first sign of a setback in applying Approach 1 diverts even more of my attention from persisting in Approach 1 to dreaming about Approaches 2 and 3. If left unmanaged, a significant amount of my brainpower gets dedicated to *not* thinking about what I am *not* currently doing. This is undoubtedly an example of a chaotic, not orderly, mind.

I try to combat this by keeping a small stack of “flash card”-sized pieces of paper on my desk. I use this paper to write down my doubts or thoughts about next steps. This gets these thoughts *out* of my mind and onto the page, where hopefully they will stay and not creep back into my mind to distract me. For example, I could write something like:

Return to Conjecture B after attempting Conjecture A. Decide between Conjectures C and D after tackling Conjectures A and B.

or

If Approach 1 does not work, try Approach 3 next and then Approach 2.

For me, practicing the quality of orderliness manifests itself in stacks of small pieces of paper everywhere. Better there in stacks than clogging up my mind!

I (Runshan) find orderliness even more important when doing empirical research. I learned this the hard way. When dealing with data, there are so

many details you need to keep track of. There were times when I could not remember how I had processed the data last time or how I had obtained the previous results. I often spent long hours reproducing things I did already but lost.

This happened partially because of laziness. I thought I was doing a quick check and did not bother to document all the steps. Partially it is due to, ironically, my low tolerance for messiness—I did not want to sully my folders with seemingly useless scripts and nonsensical notes. What would be the use of this piece of code when it did not lead to satisfactory results? The reality is you never know what will be helpful in the future.

The solution to this problem is not that hard. I now try my best to document every step, no matter how small. I regularly organize my files and notes. This involves developing consistent naming rules, archiving (but not deleting) old files, and merging related notes and code. It feels like extra work sometimes, but the feeling is much better than having to recreate things I lost.

Confidence

We spoke already about patience and hope in the context of letting your inklings “sprout” into research ideas. But we have also emphasized that part of this process is mysterious and beyond your control. What remains is setting up the conditions for the possibility of success. As discussed in the last subsection, one part of this is being organized. Being organized is also unlikely to be sufficient. You need to be confident that you have set up the conditions correctly for research inspiration to sprout. Just like when caring for a tree, you can be confident if you have given it water, nutrients, and sunlight. Once you have done all you can, you should be confident that leaves will sprout.

In the creative process, it is hard to find the confidence that you are doing everything you can to be prepared for inspiration to strike. What exactly are the analogies to water, nutrients, and sunlight in this case? All we know is this: If you never feel confident that you have done enough, it is like a self-fulfilling prophecy. You will never find yourself ready for inspiration.

We (Chris and Runshan) are firm believers that if you keep digging and pushing, you will create something interesting eventually. It may not be what you initially expected or set out to create, but it will be something interesting nonetheless.

This is partly because an arduous journey is interesting in and of itself.

There is some universality in the experience of struggle. We believe that other researchers, in their hearts, want to validate your struggle on the path to their own struggles being validated. Such open-minded researchers can appreciate work that results from thoroughness and rigor. The harder you work, the stronger your case for thoroughness.

I (Runshan) struggled with confidence for years. I constantly doubted if I could ever make anything happen. Even when I received praise from others, I would think that they did not know the “real Runshan”. The fear of them finding out how useless I actually was overwhelmed and paralyzed me.

Luckily, I have received a lot of help in building my confidence over time from my undergraduate and Ph.D. advisors. The help guided me through many challenges. The first lesson I learned is that true confidence does not come from comparison with others. There will always be someone better than you at something. If your confidence is built on being “better” than others, it is easily shaken. A more helpful approach is to always ask, “What can I learn from this person so that I can improve my skills?”

I also learned that it’s easier to focus on the practical problems at hand than to waste energy nursing my ego. If I asked myself, “Am I good enough to solve this problem?” I would get stuck for a while and conclude, “Probably no”. But eventually, I realized that I had no idea what “good enough” meant. Questions of this type just fueled my self-doubt. Instead, when I asked myself, “Is there anything I could do to help solve this problem?” the answer was almost always “Yes”. Questions phrased this way guided me to focus on all the possible next actions. The voice in my head that always questioned my ability was silenced. If the next actions did not solve the problem, I could apply the same mindset and try more actions. If the next actions solved the problem, my confidence would be boosted. To me, confidence is fostered by the challenges I overcome instead of the endless debate with myself about whether I am good enough.

Naivete

We are big fans of this quote by Edgar Schein in [19]:

“It is when we do not understand something that we need to pursue vigorously why we do not, and the best way to search is to use our own ignorance and naivete.”

We find this quote refreshing because it gives us another avenue in which to explore. If we are struggling to understand a problem in our research, don't try to come up with an answer. Instead, we think about why we can't solve the problem. There are too many things that we can never and will never understand. It is more fruitful to gain insight into "why" we do not understand things. This deeper exploration of why we struggle points us toward more fundamental ideas and directions.

One simple reason we do not understand something is that we did not have enough time to consider it or were even aware of it. That is the easy case. It does not take too much vigor to appreciate this lack of understanding. We can resolve this with focus and effort. More challenging are situations where we do not understand something despite being exposed to it and having had the opportunity to think about it. This is typically how we get stuck in research. The frustration comes because we feel like we *should* be able to come up with something or resolve some issue. Why can't we?

In this case, what stops our understanding is some lack of insight, possibly caused by a lack of patience or empathy. Maybe it is the bias of our worldview that stops us. Such misunderstandings are much more challenging to overcome because they require us to understand ourselves better. Overcoming this type of misunderstanding requires far more vigor.

But the quote continues by offering us advice on the "best way" to search for this understanding—to use "our own ignorance and naivete." When we first read this quote, we found it surprising to think of "using" ignorance to understand something. It seems like ignorance is the opposite of understanding. We view it now more like embracing the fact that we do not know things, ironically, helps us understand more. If we cannot accept that there are things that we do not know, we cannot pursue why we do not know them. The fact we are ignorant allows us to understand the nature of ignorance. An analogy here is that you cannot truly understand the value of food unless you have felt hunger.

When we go out in the world, embracing our ignorance puts us in a stronger position to overcome our ignorance. It also allows us to learn more from others. If we do not embrace our ignorance, it can be hard for us to be surprised by the insights of others. Instead, we will act as if nothing surprises us and keep yourself "safe" from things that may reveal our lack of understanding.

But it is hard to embrace our own ignorance. The quote offers the concept of naivete to assist in this direction. We like the idea of naivete because



it suggests approaching the world with purity and unsophistication. It allows us to ask basic questions and start from the beginning.

When understanding why we do not understand something, a common reason may be that we assumed you already understood it. We may be afraid to acknowledge that we actually don't. Only someone unsophisticated questions received wisdom. Most of us accept received wisdom as our own beliefs to fit in and look like we belong. The draw toward sophistication is powerful. We believe this makes naivete an important quality in the task of creating. Many of the most creative directions come from re-asking "Why?" to established directions.

8.4 Attitudes

"You must have solid training in what is known before you can create something new"

One of the received views of creativity is that you can only be creative after having a very long and solid preparation in what is known. This view tells children they need to finish level 10 in piano before they can write their own piano composition or that they need to memorize 100 classic poems before they can write one for themselves.

Growing up, my (Chris's) family took the opposite point of view. If you wanted to play guitar, you picked a guitar and started playing. If you wanted to play tennis, you grabbed a racquet and played. The idea of taking a lesson for things like sports or music was anathema to me as a child. Why would you ask someone else about how *you* should be creative and have fun? Playing



tennis was not about being good at tennis; playing tennis was about enjoying the game of tennis.

I was also really into skateboarding. Skateboarding was unlike any other sport; there were no coaches, teams, or rules. You learned to skateboard through experimentation and trying to understand in your feet how the board moved and reacted. I was never very good at skateboarding. I could not do many of the tricks the other kids could do. I couldn't go fast without getting scared. But I developed my own set of tricks that I did not see other kids do. I could be creative despite being far from the most talented. And in the skateboard world of that time, this was embraced and celebrated.

There is even some credence to the idea that being good at what is "right" and what is "standard" can inhibit creativity. There are numerous stories of students with very high grades in undergraduate studies who struggle during their Ph.D. They were maybe so well-trained at conquering what was expected of them in homework that they did not have their bearings for the task of coming up with something unexpected.

Of course, creativity doesn't come out of nowhere, and having good preparation can undoubtedly be a boon to the task of creating. The mistake is thinking that good preparation is sufficient, or even necessary, for creativity to flourish. We prefer the attitude that **"creativity can be developed and practiced at every stage"**. Developing your creativity can accompany your increasing knowledge of standard techniques and best practices.

"Only geniuses can be creative."

A related attitude is that true creativity belongs only to the few geniuses who naturally come by creativity. This is the view that notices that Mozart was



writing concertos at age five and you weren't, so you shouldn't even try. The best you can hope for is to learn how to play Mozart's concertos and not be so arrogant as to believe you can come up with your own. This attitude often makes the subtle claim that something is not creative unless it is as "good" as the "best" things we have now.

This attitude draws almost the opposite corollary of the previous attitude: there is essentially no amount of work you can do to overcome a deficit in genius-level talent. Reality tells a very different story. The vast majority of physics was not done by Einstein. Sure, he was a genius and highly creative, but many others have done many things that got them careers and moved their field forward. The public just happens not to know their names. And that is perfectly fine, even preferable. I (Chris) am comforted by the fact that (hopefully) very few people will ever know my name. I am happy to create things in relative obscurity. Accordingly, we prefer the attitude **"everyone is creative, we just might not always realize it"**.

"My training is inadequate for this task"

Of course, many of us know that genius is not required for creativity. We learned this long ago when we realized that we were not geniuses. Yet, we still want to pursue a career in academia that requires no small amount of creativity to survive. We also know that we can't have the perfect training we start being creative. That is just simply unrealistic. The question then becomes, how much do you need to know to start creating things in your research?

One debilitating attitude is the belief that what you need to know is somehow *always* just a little bit more than what you know now. At some point, we

all need to say: I am going to do what I can with what I currently know. This is the essence of confidence that we discussed in the last section. Otherwise, you will never start.

When I (Chris) first started as an assistant professor after my Ph.D., I was essentially clueless about what I would do next in my research. I had a few papers from my Ph.D. days but did not know how to build on those moving forward. Instead of thinking about what questions interested me or what was possible, I got convinced that my problem was the many gaps in my knowledge. When I sat down to write everything I did *not* know, I was utterly overwhelmed. I convinced myself that I needed to study a stack of twenty math textbooks before I could even *begin* to think about starting a research project. Every day I woke up and convinced myself that a *different* book in the stack of twenty was the most important. I would work on that for a few hours before I changed my mind that another of the books was *even more* important. This lasted for months.

I was saved from this vicious cycle not by any insight or milestone of progress but by a person. He came to me and said, “Chris, let’s figure out one of these things together.” What amazed me was that he did not seem to care what we worked on, just that we would do it together. After a year of spinning my wheels, convinced I needed more, one day, I just started to think about what I could do with the help of someone who seemed to care. This turn of events put me on the path to where I am now.

I now believe that I was hiding behind the attitude that “my training is inadequate” to avoid the task of being creative. I needed the help of someone else to get me out of this hiding. We think a healthier attitude is: **“I may never feel fully adequate, but this feeling of inadequacy will not stop me from creating.”**

“Creating should be fun, it should come naturally”

Why would someone avoid creativity, isn’t creativity supposed to be fun? Indeed, creativity is often associated with playfulness. Get children to do something creative—like drawing or painting—and they will be happy. One reason for this is that it can sometimes feel like creative acts should flow naturally and easily. When you see someone skilled in painting—like Bob Ross in his



famous painting show on television² —it seems effortless and joyful. Bob is always smiling and relaxed. This hides the immense number of hours Bob put into his craft to make it look easy.

We (Chris and Runshan) agree that creating in a research context is not always fun. Early in a career, research may hardly ever be fun. Scrambling for meaningful questions to work on is frustrating. Making mistakes and starting again from scratch is frustrating. Shooting in the dark at new ideas can be a frustrating goose chase. But it gets easier as you get more experienced. Over time you might even make it look easy (even though, in our experience, it always remains challenging). And the payoff of creating something “cool” can make it all worth it.

I (Runshan) am lucky to have a natural interest in the research questions of my field. I still remember my excitement when I first joined the Ph.D. program and was presented with all kinds of exciting research projects. This excitement never wore off for me. But much as I enjoy exploring, discovering, and sharing my findings, the creating process is still sometimes painful and no fun. When creating becomes an essential part of your career, it can feel stressful and drain part of the fun. I usually try to enjoy the process as much as I can. When I have to face something unpleasant or deal with my frustration, I tell myself that this is the tax I need to pay for working on the exciting projects I have chosen. Looking back, I find that some of those challenges made the projects more meaningful to me. I feel proud that I poured significant effort into crafting something that now looks simple and smooth.

I (Chris) took many years to come around to find much fun in the research process. Early in my career, I was perpetually crippled by my lack of interest

²For those unaware of Bob Ross, a search on YouTube for “Bob Ross” will quickly make you aware.

in research topics that seemed “popular” in my field at the time. I also had a lethargy in undertaking the “trial and error” it takes to find good research topics and come up with meaningful results about them. I knew I had to do research to thrive, so I did it, but I must admit that it was not my idea of fun. Somehow, my aversion to the process weakened with time. When I started to teach classes to undergraduate and graduate students, I developed a genuine interest in more research topics in my field. As for “trial and error”, the errors had piled up so high that a few things started to work out. But the real turning point was when I found other people willing to go through the challenge of research with me (more on this in [Chapter 11](#)). Only recently, I have become more like Bob Ross. On a good day, I can find joy in my research. Much of that joy comes from interacting with my collaborators as we tackle difficult things together. We (Runshan and I) came to the same attitude but in different ways: **“Creating is difficult, but if we persist in it, it can bring us joy.”**

8.5 Putting it all together

As a form of summary, we provide an example of young researchers exploring the ideas in this chapter. The story is fictional but inspired by real-life interactions with many young researchers over the years. The main purpose is to provide you with an example of how natural it can be to discuss your approach to research with those you trust.

Jonas was immensely frustrated. Whereas other Ph.D. students in his cohort were already writing papers for submission, Jonas was still searching for a good idea. Jonas had done better than most in his coursework and already had a paper published during his Master’s degree. It did not make sense to him why no good ideas were coming his way. His advisor was supportive, but she was adamant that Jonas identify a research question himself. This was new territory for Jonas.

At the beginning of the academic year, Jonas was very active. He read what felt like hundreds of papers, meeting his advisor every week to discuss what we had learned. And yet no good ideas were forthcoming. He had many ideas, but he felt none were any good. But reflecting on it later, Jonas realized it is more likely the ideas were not good because he never gave any of them a real chance. He had gotten into the groove of just reading and reading. Nothing

he could come up with ever seemed as compelling or complete as the next paper on his list to read. Every “lead” or “direction” that came to his mind, he would chase down related papers and convince himself that these papers had answered everything already.

In the latter months of that period, he kept gravitating to papers written in the 1950s – the classics. He made a higher and higher pile of papers (burning through all his printing credits at the office) he felt he had to read. By that time, any idea, even what he considered bad ideas, stopped coming to him. But it got even worse. In the last two months, Jonas even stopped reading papers for the most part. He had what he felt like two years of reading in a pile in front of him but no energy to tackle it. Jonas even stopped coming to the office.

Then came a turning point. Magda, a fellow Ph.D. student in his office, became worried about Jonas and knocked on his apartment door. Jonas answered, woken up only a few minutes earlier by an alarm around 11 am. When Jonas opened the door, Magda said, “Jonas, get your jacket. You’re coming with me for coffee.”

What else could Jonas say but, “Umm ...sure.”

Magda and Jonas went to a local, bustling coffee house. It was mainly frequented by graduate students, some of them debating loudly with each other on the meaning of this and that.

Magda: Jonas, I am worried about you. Why are you not coming to the office?

Jonas: I don’t see the point. I am not sure I want to continue with this program. [expecting shock]

M: Don’t worry about that; everyone I know has felt like leaving the program at one point or another. It’s a rite of passage.

J: It’s just I thought it would be more fun. The idea of having all this time to think seemed so romantic. But this freedom ...well; it feels more like a cage than anything else.

M: But Jonas, this is training for a job. It is not freedom; at least, it never felt like that to me. I show up every day not because I enjoy it every day but because that is what I am supposed to be doing.

J: You mean you don’t always enjoy it? You always seem so happy.

M: Happy to see you and the others! I guess I don't expect the work to always give me joy. Work is work; it is there to test you and help you grow.

J: (long pause) I am not sure I can agree with that.

M: Let me tell you how I do it. I go every day to the office just like my mother did. Rain or shine, she went, even when she was sick. That was her honor, her integrity. I try to do the same. When at the office, I set myself tasks, little tasks. My calendar is filled with half-hour chunks. Each half-hour, I try to push something along. If it's painful, I know it's ending soon. But I won't quit. I remember my mother, and I won't quit.

J: How do you feel when you don't get anywhere in that half-hour?

M: I always get somewhere.

J: What!? How?

M: Well, I know by the end of that 30-minute chunk, at least I tried something. That is getting somewhere to me. I make a note of what I tried and go on to the next thing. Sometimes I string together a bunch of 30-minute chunks to chase something down. I did whole days on one idea before submitting my last paper. Other times I fill up that 30 minutes with getting some admin tasks done. Sometimes I take a friend for coffee and reflect on the research process. [smiling]

J: These are all just incremental steps; they don't always add up to anything. Doesn't investing so much time into something that might not go anywhere give you anxiety?

M: At times, sure. But we do creative work, Jonas. I give my day as much structure as possible, but I don't expect it to be a formula. There are many dark alleys in the process of creating new things. It seems a more efficient way does not exist.

J: It all sounds so frightening.

M: More frightening than six months from now, you realize you slept away half of your waking hours?

J: Well ...no.

M: Jonas, you are coming with me to the office.

Over the next several weeks, Magda dropped by to pick up Jonas on her way to the office. At first, she had to wake him up and wait for him to get ready (sometimes, she did one of her half-hour chunks on the doorstep while Jonas got ready). But after a while, she found him ready to leave when she arrived.

And one day, she arrived to find a note on his door:

Dearest Magda, I've left for the office, see you there. Affectionately, Jonas.

When Madga got to the office, Jonas was working away furiously at the whiteboard.

Jonas: Magda, glad you are here. That coffee and bagel are for you. [pointing to the table]

Magda: Thanks. What's gotten into you?

J: All of this [pointing to the board] just came to me. After all those months of reading those papers, it all started to click these last two weeks. It began when I forced myself to sit in front of a blank piece of paper for one of my 30-minute chunks. At around the 25-minute mark, I wrote down one sentence. That sentence led me down this path. I woke up this morning with an immense sense of clarity. I came in right away to write it all out.

M: Wow, congratulations. That's amazing.

J: Would you like me to explain it? You can enjoy your coffee and bagels while you listen.

M: Sure ...as long as it takes less than 30 minutes!

8.6 Reflection questions

The questions below are provided for you to reflect on what you have read in this chapter. If possible, it is beneficial to discuss these questions in a group of your peers.

1. Describe something that you were involved in creating. What steps were involved? Did you work with others? Was the work frustrating at times?
2. How do you organize your time when doing research? Do you make lists? Do you divide your time into chunks?
3. How do you decide when it is a good time to take a break from doing research?
4. How do you define progress in your research? How can you tell if you are making good progress?
5. Describe a time when you were stuck while doing research. How did you become unstuck?
6. Has a research idea ever come to you seemingly out of nowhere? If so, what were the circumstances?
7. Is there something you thought at one time was obviously true that later you came to be less sure of? If so, how did you come to this realization?
8. Do you feel you have enough knowledge to do creative research in your field? If not, what more do you need to learn? How will you know when you have learned enough?
9. In what way, if at all, do you relate to the story of Jonas and Magda? What do you take away from the story?

Chapter notes

Please consult fellow students, advisors, and colleagues on field-specific guidance on conducting research in your area. Our favorite pieces on creativity in mathematics are [9, 10].

Chapter 9

Writing

Academics write many things: emails, reviews, textbooks, cases, proposals, reports, tutorials, etc. There is also writing for yourself—notes, plans, ideas, etc. We have known academics who inquire about our research and ask “What are you writing about these days?” instead of “What are you researching?” Without a doubt, the act of writing out your ideas is one of the most labor-intensive tasks of being an academic.

This chapter is not about writing in general but focuses on the specific task of writing academic papers. Much of the ideas will also carry over to writing other documents you must submit for review, like funding proposals. But our primary focus here is on writing up work that you have done for publication at peer-reviewed outlets. These papers represent a culmination of many hours of thoughts, discussions, notes, emails, and false starts. Papers are what you have to show in the end for much of our effort.

Sadly, writing a paper is not easy. It is an iterative process that can take years to complete. As you grow in your career, you will have a chance to work with many groups of co-authors who approach the task of writing papers differently. We (Chris and Runshan) have learned something from all of these different approaches. In this chapter, we will attempt to share some of our hard-won insights.

9.1 Purpose

A central message of this book is how writing papers to get them published is arguably the main task on the path to academic success (as detailed in [Chapter 2](#)). This is a very practical purpose for writing papers. But, of course, there are multiple other purposes for writing papers beyond this relatively short-term and career-focused one.

To record

Suppose you have created some research, possibly through a process similar to what we described in [Chapter 8](#). Maybe you have solved an open problem, devised an algorithm to make a decision, found an interesting regularity in a dataset, etc. Is it enough to have produced these results, with maybe no one else ever knowing? Possibly. But, in many cases, there are funding agencies and university administrators who are not satisfied with you simply creating things for yourself. They want your creations to be shared with the world (and count towards their objectives).

One purpose of writing papers is to record what you have done and share this record with the world. The desire to record our accomplishments is quite natural. Who among us would reach the top of a treacherously high mountain and not take a picture? And wouldn't it also be natural to send that picture to your friends and family? It says to them: "This is who I am, and this is what I do."

Similarly, it is natural to write and send out a paper that describes your accomplishments (if only it were as easy as taking a picture) just to let people know who you are and what you do. Maybe you can hope that others will build on it, respond to it, or criticize it, but at a minimum, there is something you were involved with that was not there before.

But even if you can't be fussed about whether others know what you have done, there is another compelling reason to record things—so you don't forget yourself. No creation is so beautiful that even its creator would forget about it some years later. I (Chris) have research results I uncovered so long ago that I can hardly remember any of the details. If I didn't have papers describing what I did, they would be lost to me, just like everyone else. In a sense, your papers are like historical fragments of your own mind. They capture the way you thought—or at least how you tried to explain your thoughts

to others—at a particular time and place.

One thing I (Chris) enjoy is when I look back at some of the papers I wrote, and I find I have no memory of writing them. At this point, I can enjoy (or dislike) my words and ideas just like anyone else could. They are not coming from me anymore; they are coming from an older version of me, who I might not even recognize.

To educate

Again, suppose you have just reached the top of a treacherous mountain. As we said, it is natural to take a picture. But what does the picture serve? It records that you were at the top of the mountain, but what else? We'll discuss two other reasons: (i) to educate and inspire and (ii) to impress. Of course, you might do both simultaneously (or neither), but let's discuss them separately for contrast.

Writing a paper can help inform those who read it, putting them on the path to making their own creations. Maybe the paper answers an important open question or opens up new possibilities for exploration. The paper is a tool of education for students and other researchers. In some cases, the paper can enlighten decision- and policy-makers. Your paper is an “offering” to the world. You put it out there and see where others run with it.

But there is maybe even a more fundamental reason to write. There is a feeling you have when you create something in this world. You know that there was luck and inspiration involved. The work is not entirely your own—you may feel that the “heavens” have assisted you. A mysterious force graced your life with ideas and experiences. In this view, your ideas and experiences do not entirely belong to you. By writing, you return your knowledge to its natural owners—everyone who can benefit from your experiences. What came to you could have come to them. In this view, inspiration is gifted to a people; the individual is only the messenger.

In writing this book, we (Chris and Runshan) have felt a great sense of relief in getting our thinking onto the page. We have had the experiences of struggling with our academic research careers, and we hope that others can learn from our struggles. Keeping these experiences to ourselves did not seem right. Although it may be impossible for you to verify this assertion, we believe our primary motivation for writing the book is to help you navigate deep waters better than we did. Our driving purpose was to educate.

To impress

But we know what some (possibly many) of you are thinking: “Hey Chris and Runshan, didn’t you *actually* write this book to impress your colleagues and gain adherents to your own personal brands among the younger generations?” We would be lying if impressing others never crossed our minds (although we do stick to our statement from the previous paragraph about our primary motivation). Indeed, it is quite natural. Return to the mountaintop, where you took a picture of your victory and sent it to your friends. There is something very innocent in proclaiming: “Look at what I have been able to achieve!” We hope others can appreciate the hard work and determination it takes to write a paper. We want them to value and honor the time we put into conquering a research question and bringing something into the world. We hope that others can catch a glimpse of our ingenuity. We hope that others are impressed. And we must all admit that climbing Mount Everest is undeniably impressive, just like publishing a paper is undeniably impressive.

In these pages, we have discussed at length that success in a research career depends on acceptance. Part of the acceptance process was the process of persuasion. When it comes to research, there is nothing more persuasive in making a case for acceptance than publishing impressive papers in reputed outlets.

But the purpose of impressing others can go beyond the process of acceptance in the context of a career. There can be other motivations when you send that mountaintop picture. You might send it to someone who doubted you to show them they were too quick to judge. You might send it to an early ally or mentor as a testament to the efficacy of their help. You might send it to your parents, who will pass it on to others, creating a ripple of “impressiveness”. The fact of victory itself shines a light on you and those around you. A great paper not only lifts the author’s career, it can lift many others in the process.

To belong

Let’s turn to a purpose that is much more simplistic, maybe even tautological. The purpose of writing papers is to be someone who writes papers. Success in a research career requires acceptance from the research community, but for some, there is an even simpler reason than career success for wanting to

be accepted—they highly value belonging to the community of academics. There can be little doubt that, collectively, the academic community is an impressive and productive segment of society. Many societies entrust their brightest young people to the care of academics at their universities and turn to academics when faced with the need to answer questions ranging from the practical to the profound. Unsurprisingly, you could find purpose in writing papers by the simple fact of belonging.

I (Chris) recall an opportunity I had to meet a group of talented undergraduate students to discuss with them the prospect of being an academic. These students could (and largely did) follow lucrative paths into industry right out of their undergrads. My task was to share what might be attractive about pursuing a Ph.D. I could not think of too many reasons initially, so I probed my own motivations. Only after some thought did I come up with the following prompt:

Suppose you just learned something new and unexpected in school. Raise your hand if the first thing you would think to do is to explain what you learned to someone else.

(Pause to allow people to raise their hands)

Those with their hands up—you have the spirit of an academic. It is not just the fact of creation or discovery; it is the desire and will to share your discoveries broadly with others that distinguishes this profession.

In many ways, the writing of papers is what distinguishes the work of research academics. The fact that you write papers for public consumption is one of your strongest signals of belonging.

9.2 Skills

The subject of useful skills and tips for writing a research paper could fill a whole book (and there are many examples of where it does, see a selection of our favorites in the chapter notes). Below is a collection of skills that have meant the most to us; it is far from exhaustive.

Providing logical flow

Writing has as much logic as coding programs and proving theorems. This statement may sound blasphemous to those from more mathematical or engineering backgrounds, but we stand behind it. Good writing is as elegant and logical as any mathematical proof or computer code. It is just a different genre of ingenuity.

So what do we mean by logical flow in writing? You have undoubtedly experienced bad examples. Consider a television show that jumps back and forth in time without many clues about what (and when?) is happening. For a television show to make sense, viewers should always have a pretty good picture of where they are and what is happening. They can be surprised on occasion, but it is dangerous if they are confused. The same is true in research papers.

To give just one example, one of the great points of confusion for me (Chris) is shifting notation. For a while, a variable is denoted x , only later to be denoted t . I generally have one of two reactions to this: rage or slumber. Both are not good if you are trying to persuade me to like your paper.

One of the principles of writing logically is that the story of your work needs to make sense in real-time to a somewhat uninitiated reader. It is often the case that the logic you currently use to think about your work will be different than the logic that an uninitiated reader would most easily understand. Why? Because you have been thinking about things for a long time. Lots of small leaps in logic are done automatically in your brain, even subconsciously. Of course, when developing the research content of the paper, these leaps in logic are entirely necessary. If you had to construct the entire logic tree every time you needed to make progress, your mind would be exhausted before covering any new ground. But for your reader, even small leaps can leave them stranded.

What can be done about this? For one, you can try to empty yourself of your own logic and imagine you are a reader and not yourself. What you produce will likely feel slow and pedantic to your “informed” self, but it may be exactly the right speed for someone just arriving. The “direction” of your thought might change completely in this process. When producing research, your mind always wants to make forward progress in getting to some destination where the story will be concluded. In the writing process, there is a strong feeling of “back-tracking” and uncovering lots of small details that

were maybe not on your path of discovery but are important for fending off confusion in your reader.

I (Chris) have been involved in projects where the order we discovered our results ended up being in the complete opposite order of how we presented them. The first thing we realized in the research process was the last thing we mentioned in the paper. This is, of course, completely natural. Maybe you are looking at some data and making a very revealing observation in the data that tells the whole story you want to tell. But then, you need to set up an empirical model and inference scheme to show that this observation is statistically or theoretically significant. The thing you observed first, possibly by chance, is presented in the paper as the outcome of a laborious and principled scientific process.

I (Chris) am often known among my groups of collaborators as the “slowest” person in the group. It takes me the longest to take leaps of logic and go with people on the adventure of creating. Of course, I wish I was a little faster and could take leaps with them, but over the years, I have come to realize that I am slow for a reason. During my high school days, I always finished my work quickly but then found other people in the class who were confused and could benefit from interaction with me. I had a powerful impression through that experience that my level of understanding was continually strengthened by explaining things to others. This became my intellectual standard. I felt I did not know something *until* I could explain it to someone who was confused.

One reason I am slow is that I am carrying, now imagined in my mind, a whole other person who is very confused. I am reluctant to move forward until that imagined and confused entity in my mind is comfortable. This is not the best mental construct for someone looking to make leaps of logic and push forward creative research ideas. But it is certainly helpful in writing.

One of my collaborators once said about me: “By the time Chris understands, everyone can understand.” Of course, this message has two sides. One is that Chris is not so sharp sometimes (which I readily admit). But the other side is that by the time Chris understands, he can explain it to everyone else, and they will get it. I am not sure sometimes which of these two messages is more accurate, but I have come to accept some combination of both is true.

One important part of providing logical flow is being able to break down big concepts into smaller and smaller steps. If an idea is complicated, take the time to break it down so that it can be explained layer by layer. In this process, you often discover there are more layers of logic than you initially thought.

If an argument is convoluted, establish it with multiple steps. If a point is important, it is okay to repeat the idea in a different way for emphasis. It is better to explain ideas clearly with more logical steps than cramming many into very dense sentences. I (Runshan) get more frustrated facing convoluted arguments than bored by having logic laid out slowly.

Providing context

Imagine the following scenario. An alien has arrived on Earth and is trying to learn about humanity. The alien sees a clock on the wall of the room and asks, “What is this?” How would you go about explaining the clock? There are roughly two directions you can go into in your explanation. First, you can explain that a clock is a mechanical device made up of sets of gears that run two arms. You can explain how the numbers on the face of the clock are related to the rotation of the Earth and how the duration of that rotation is broken up into smaller pieces. You may even want to explain why hours are measured by groups of 60 minutes while days are measured by groups of 24 hours. You can talk about how the clock runs on electricity or battery power.

We can call this first direction *analysis*. It attempts to understand the clock by examining the nature of its component parts and how these parts interact. But how else can you explain the clock?

The other direction goes into the history and significance of the clock to human society. In other words, you are putting the clock into greater and greater *context*. In this direction, you can talk about how the method of time depicted in the clock evolved to be universally embraced by humanity, based on some ideas from the Greeks and Babylonians about time and how it’s measured, finally becoming encoded in the mechanical clock in the 13th Century in Europe in roughly the form you see it today.

But, of course, there are even deeper contexts for humanity’s relationship with time. Most people have a clock on them at all times (on their wrist or on a phone) because we have structured our activities like school and work around measured time instead of natural time (whether it is dark or light outside, for example). And, of course, there is the even deeper connection of humanity’s fascination with time due to its connection to aging and death.

The alien likely comes from a world with different lengths of days, broken into different notions of hours and minutes. Maybe they have “clocks” but may not be mechanical; perhaps they are based on a different technology. It is

in the deeper contexts of the meaning of the clock where more commonalities with aliens are likely to be found. They, like us, are likely concerned with aging and death and their relationship with time.

Like in our explanation of the clock, papers have components of both *analysis* and *context*. The analysis part comes naturally to most of us because undertaking research involves breaking the problem into smaller and smaller pieces. Indeed, in **Chapter 8**, we strongly advocated breaking down research into smaller, manageable tasks. What may be less regularly practiced is the task of putting our research into more and more meaningful contexts. This direction repeatedly asks, "Why am I doing this research? Why would it be meaningful to someone not immersed in this like I am?"

Abstracts, introductions, and literature review sections of a paper are typically places where the context for the work is provided. This is where you take all the pieces of the work and put them back together again. But more than that, this is where you take that assembled work and put it in a context that others can relate to. Providing context is a skill of understanding why others would care about this very focused and specific "new" piece of knowledge that you came up with by connecting it to something bigger and more profound. If this context is authentic and far-reaching enough, even a little idea can take on great significance.

Outlining

In our discussion of the last two skills, we focused on how to think about what a good paper should do for the reader. Let's turn our attention now to some skills needed to do the physical and mental task of writing.

The first lesson when sitting down to write is quite simple: if you don't have a plan for what you are going to write, you are, in all likelihood, doomed. You will most likely go in circles, write the same thing repeatedly in different words, or end up staring at a blank screen. The only reliable way we know how to make forward progress is to have an outline.

Most likely, all of you have taken writing classes in your education. They probably told you about the steps of brainstorming, outlining, writing, and revising. And presumably, you did those things during your courses on writing because they were required. But probably a few of you downplayed this guidance for writing research papers. It is not like an opinion piece in a literature class, where you must organize some naive thoughts about a poem

and then write them out. You are doing research here! It takes you months or years to come up with the content you will write about. Can what you learned in those introductory writing classes really help you with such sophisticated work? The answer is: yes.

We (Chris and Runshan) wrote this book precisely following the pattern taught to us in our writing classes: brainstorming, outlining, writing, and revising. The brainstorming took about a year of weekly meetings. In the process, we generated many pages of handwritten notes and drawings. Next, we structured the chapters and sections within the chapters. Next, we made detailed outlines for each chapter. Finally, we wrote the ideas in the outlines into sentences and revised them several times. We know for a fact this book would have been impossible to finish if we did not put time into the outlining stage.

We have noticed that many young researchers overlook the outlining stage. They go from brainstorming (in this case, collecting results, models, and other ideas) and try to go right into writing the final version of the paper. This is extremely challenging. You are not only trying to write nice and grammatical sentences (a hard task even for native speakers of English) but also keep the logical flow of the story “up in the air” at the same time.

We believe one reason young researchers don’t do extensive outlining because there is often a sense of urgency when it comes to writing in a research context. Once you have the results you want for your research project, there is an urge to write it up quickly and get it out there. We have heard researchers say many times: “All we need to do now is just write it up.” Maybe we have even said it a few times ourselves. But “just writing things up” has its own process.

I use outlines to make clear what I need to say and when. I find it better to decide on a structure and flow I like before over-crafting the sentences. I save worrying about *how* I am going to say things (sentence polishing) until after I am organizing *what* I want to say and *when* I want to say them (logic-generation). Everything has a time and place. Outlining is the puzzle of finding the right time and place for everything you want to say. Ironically, outlining is one of the ways to make your writing go faster, even though it seems like more work.

We are probably all familiar with the very broad outline of a paper with sections like “Introduction”, “Literature review”, “Conclusion”, etc. Such broad outlines would help you break the task of writing a paper into smaller

parts, but our experience is that they may not be very helpful in separating the logic-generation process from the sentence-crafting process described above. Detailed outlining is more helpful in establishing this separation.

When writing this book, we (Chris and Runshan) usually wrote outlines at least as detailed as the final output. The final outline was essentially as long (page length-wise) as the book's final draft. For example, if a paragraph is five sentences long in the finished product, we started with at least three or four bullet points that detailed what the paragraph was about. The complete logic of the book was essentially set before we started writing complete sentences.

Building on this experience, I (Runshan) have adapted this approach to my writing for research projects. It was eye-opening how helpful a detailed outline could be in the writing process. Before, I would typically make much broader and vaguer outlines. The writing process can still be quite stressful, but less stressful than before. Another benefit (besides reducing stress) of separating the two different processes (the logic-generation process and the sentence-crafting process) is that outlines give more clarity to the overall logical flow of the paper.

I like to think of writing a paper like building a house. An outline is the structure of the house. If you do not pay enough attention to the outline and rush into writing sentences, you are painting the walls of a poorly constructed house. If you are short on time, laying the foundation right is even more important. I often rewrite an entire polished paragraph or section because I later learned its logic was flawed or did not flow well. Revising and rewriting are sometimes inevitable, but a well-thought outline can save a lot of wasted effort.

I also want to add more on the importance of outlines, especially for non-native speakers. Many non-native speakers believe they could never be good writers and dislike the whole writing process (more on this later in this chapter). Being a non-native speaker definitely brings you more challenges in writing. But I believe the most important element of a good research paper is a coherent, logical flow, which does not require great mastery of the English language. A good outline helps me avoid worrying about the rules of English and allows me to focus on what I want to say. It reminds me of my main goals for writing a paper—clearly document and communicate my thoughts, ideas, and findings.

Making the paper navigable

Another reason young researchers may “skip” the outlining stage is that the outline is a document that never appears anywhere. Published papers do not have an outline for the reader to navigate its content beyond a few section and subsection headers. Often the reader needs to recover for themselves the lists of bullets that give an idea of how the story of the paper is structured.

One skill in writing is to make the reader’s job easier in navigating what is in the paper. Luckily, software for writing papers (like L^AT_EX) has a lot of tools to make this easier. In my (Chris’s) papers, I like to give numbers to equations and refer to those numbers in the paper. Instead of saying vague things like “what we argued above” in the paper, we can rather say “following the reasoning after equation (2)”. I would like to go even further than this to number each line of the paper and refer to those. For some reason, this is not standard in academic writing in the fields I have encountered. Maybe it makes it look less beautiful? I am not sure.

A telltale of a poorly written paper is repetition: explaining essentially the same thing at multiple locations in the paper. No one wants to get the feeling that the paper was written by a distracted writer who forgot what they just wrote. To combat this, writers try to avoid repeating themselves in any way. When taken too far, it leads to a paper that is hard to follow. This can involve removing signpost sentences that tell the reader what will happen next or summarize what just happened. One principle of good writing that you often hear is roughly this:

Tell them what you are going to say. Say it. Then tell them you said it.

From a purely logical perspective, this is redundant. Just say something once and get on with it! But from the standpoint of helping the reader navigate your paper, including sentences that explain where you are heading and where you have been is highly appreciated.

Grammar and composition

Once you have an outline of what you want to say and where you want to say it, you need to decide *how* you want to get your ideas across. Now you need to craft sentences in English. And now a whole other world of troubles begin!

Many excellent books focus on the fine details of writing elegant English. We will not try to improve on them because we really can't. We recommend several of our favorites in the chapter notes, including books on writing prose and writing mathematics.

The key idea behind tight grammar and strong composition is to provide *clarity*. You are not writing obscure poems. You are writing papers to convey your research ideas and findings clearly and unambiguously. Here are a few examples that we come back to over and over again when discussing writing with students and collaborators:

- *Use unambiguous names for concepts.* Don't use multiple words that refer to the same thing for "variety". For example, using different names—customer, consumer, agent, player, decision-maker, etc.—to refer to the same entity in a model. Multiple labels for the same object do not make the work more interesting. It makes it more confusing. Just like in mathematics, you do not use multiple different labels—for example, x_i , x^i , $x(i)$ —for the same object, you should not use multiple words for the same concept.

Conversely, don't use a single word for multiple concepts. Suppose you set up an economic model with two agents. Later in the paper you write: "The agent decides" Sadly, since there are two agents, the reader may be unable to tell what's going on: which agent decides? For clarity, it might be an idea to give them separate names. An inelegant (but efficient) solution is "agent 1" and "agent 2". Then you can say: "agent 1 decides ..." to avoid confusion.

- *Write short sentences.* We cannot stress this enough: write short sentences. Do it. If you are having a hard time deciding where to place a comma in a sentence, here is a solution. Don't use a comma. Write two sentences instead. Confused about when to use colons versus dashes? Here's a solution. Use neither. Write two sentences instead.

For some reason, some researchers have gotten the impression that writing long sentences makes them look smarter. It doesn't. It only makes you harder to follow. There is a difference. Don't get it twisted. Strunk and White's *Elements of Style* is a master course in writing shorter and clearer sentences. Read it today.

- *Use present tense.* The English language has many tenses for many purposes. When it comes to writing research papers, you essentially need only one: simple present. Scrap “we did” for “we do”. Scrap “we will show” for “we show”. Scrap “Zhang et al. showed” for “Zheng et al. show”. Don’t get us started on the problems with “we will have shown”. The simple present is more active and engaging than other tenses. As a bonus, its grammar is simpler, so you are less likely to make mistakes using it.
- *Use familiar words.* Avoid using “sophisticated” words that most people will need to look up in a dictionary to understand. Impress people with your ideas, not your vocabulary.
- *Avoid identifying mistakes.* Nowadays, the standard language for writing papers in most fields is English. For those non-native English speakers out there, sorry for this unfortunate historical circumstance. There are common grammar mistakes among native and non-native English speakers alike (like placing pesky commas). But some mistakes are more likely to be made by non-native speakers. This is what we mean by “identifying mistakes”. If this concerns you as a writer, you may learn about common grammar mistakes among non-native speakers and try to avoid them (a Google search will reveal some resources). Some examples we have noticed include pluralizing noncount nouns (literatures instead of literature, notations instead of notation, works instead of work) and incorrectly formed prepositional phrases (for instance, using “in” when “on” should be used, or “to” when “with” should be used).
- *Use conjunctions to guide the logical flow.* Words like “because”, “although”, “therefore”, and “meanwhile” can help the readers understand the main points you are trying to convey. This also includes structures like: “There are two reasons behind this phenomenon. First, ... Second...” We (Chris and Runshan) usually find it easier to navigate a paper with the help of such “structure” words. The downside of conjunctions is that using these structures to make sentences longer is tempting. Also, improper use of conjunctions makes the texts confusing and can be an identifying mistake (picking up on the last point). Repeating the same conjunction frequently (Therefore Therefore

.... Therefore) diminishes their value in providing structure and logic and can even become grating.

Luckily, there are great grammar tools available for free online these days. I (Chris) sometimes use multiple grammar software tools on the same piece of writing; each gives different suggestions and finds errors the others do not. I make a lot of grammar errors when I write. My text messages to friends are riddled with missing words and typos. I rely on grammar tools for these fixes so that I don't waste mental energy worrying about these things as I get my ideas down.

Rewriting

Zinsser said it best in his classic [27]: “Rewriting is the essence of writing well.” No one can write 1000 lines of computer code in one go without introducing at least a few bugs. No one can write a 20-page paper in one go without at least a few issues. If you write a first draft of a paper that has no issues, you are doing it wrong! The goal is not perfection; the goal is evolution.

When I (Chris) write, I heavily use outlines (as discussed above). This is how I organize my thoughts. Then I convert bullets into grammatical sentences. This is when I try to turn thoughts into something others could potentially understand. Then I revise those sentences and try to get them to flow better into each other. This is when I try to turn something potentially understandable into something potentially enjoyable. Then I read the whole thing from start to finish and cut away as much as possible but still retain the main message. This is when I try to turn something potentially enjoyable into something that ideally feels elegant or vital. I often have to do these steps multiple times and often fail to achieve the elegance or vitality I dream of.

Indeed, I am never fully satisfied with what I write, only resigned that revising more would waste my time and energy. That is when I know that I am done. Either that or a deadline removes the privilege of more time. I do not attain absolute perfection. I reach a practical perfection that achieves what I need to achieve. When it comes to writing, I succumb; I do not conquer.

Coherence

In the previous subsection, we talked about elegance and vitality. These aspects of good writing can only be achieved through thinking carefully about

why everything is precisely where it is. This involves considering how each part of your paper relates to the others. This is what we call *coherence*: everything hangs together.

Creating coherence is not easy. You have to read the paper from start to finish many times. If you write the paper in parts and read those parts in isolation, you will struggle to attain coherence. If you write the paper at different times, you may have used different ways of explaining the same thing in different parts of the paper. This is not coherent.

I (Chris) try to apply the following high standard in my writing. I am constantly asking myself: could an engaged reader, without too much effort, anticipate what I will write next? If the answer is “no,” does this achieve some purpose of surprise, or is it just that I did not prepare the reader enough? A paper full of surprises is another name for a confusing paper. A paper where the reader can often anticipate what will happen next feels comfortable. This makes the true surprises “pop.”

Another thing to avoid is a paper that feels like it was “written by committee” where each section has a different tone, uses different terminology, and potentially even different notation. We are huge advocates of collaboration in research (see our [Chapter 11](#)), but when it comes to writing a paper, it’s best to write it so that it feels as if it was spoken from one voice, not many voices. This coherence in “voice” also does not come for free. It requires collaboration among the writers or sometimes even a single writer taking a final pass that establishes a consistent “voice” throughout.

Use of pictures

A picture is worth a thousand words. There, we said it. Now underline it a thousand times, and you start to understand how vital figures are in academic writing. We have a simple exercise for you. Ask someone to explain their paper in front of a whiteboard or pad of paper. They will inevitably draw a picture. Often, it will be the first thing they do. Why? Because pictures are good at capturing a lot of ideas in a short amount of time. They can quickly get across the essence of your thoughts.

What I (Chris) find surprising is that this explanatory picture—offered when you ask someone to explain their paper—rarely appears in print. What happens to it? Why would it disappear? There are a few reasons. One is that the pictures may illustrate a simpler case than what is eventually treated in the

text. The thinking here is that it is better to explain something more general than something specific, and the picture may oversimplify things. Maybe it makes the idea look “too obvious” and undercuts a message of difficulty. Perhaps it gives away too much, diminishing the potential for surprise.

Another reason for not including illuminating figures is due to a saying that is popular in math circles (and maybe in other forms in other communities): “A picture is not a proof”. A picture is worth a thousand words, but which thousand words is a little subjective. A picture is great at giving the idea of an argument, but it rarely makes the idea unambiguous. A paper built on pointing at pictures is quite literally “hand-wavy”.

My reply to the saying that “a picture is not a proof” is equally as pithy: “Why does everything need to be a proof?” During my undergrad, I had a math professor who would write figures and pictures on a sideboard, reserving the main board for epsilons and deltas. One time after class, I asked him, “Why do you draw the pictures separately?” His reply was, “Strictly speaking, the picture is not needed. If you understand the math, the picture is implied.” I guess I didn’t understand the math. In most cases, the picture was essential for my understanding. Maybe this means I am bad at mathematics, but perhaps it also means that pictures can help people make sense of rigor. Pictures often give much-needed context.

A third reason that people do not include figures in their paper sounds a little silly when you first hear it: Drawing beautiful pictures is difficult. Whereas words, numbers, and mathematical symbols have standard beautiful fonts (Times New Roman, Garamond, Computer Modern), pictures do not. Each picture is a delicate flower. However, the excuse for not including figures because they look ugly is increasingly indefensible. Tools like IPE can easily make elegant figures to embed in LaTeX.¹ Tablets make handwritten figures feasible in some cases.

We strongly advocate using figures, but we admit that coming up with the “right” figure is not straightforward. It’s hard work to make figures that are not confusing. Just like a clear figure is often more clarifying than a clear paragraph of text, a confusing figure is likely more misleading than a confusing paragraph. One way to ensure that your figures are not confusing is to explain them to some of your friends. It is less of an investment than asking

¹See <https://ipe.otfried.org/>. It seems everyone has their favorite way of drawing in LaTeX; as far as we can tell, there is no standard.

them to read your whole paper, and it is a good test of whether you can get across your ideas efficiently.

Drawing out the research aesthetics

A good way to summarize the skills involved in writing is that these are tools to draw out the six research aesthetics we described in [Chapter 3](#). At some point, your results are what they are. Writing is the place you can think carefully about how others will react to what you have. In writing, you can accentuate the beauty inherent in your work.

We wanted to give a few examples of “aesthetic” writing we found in published research papers. Read them and think about which of the six research aesthetics in [Figure 3.1](#): novelty, usefulness, difficulty, generality, surprise, and simplicity.

(E1) *“...unlike the aforementioned works, we assume ...”*

Surely this is a statement of novelty. You do something others do not. Of course, you can use your writing to explain what makes it novel. Much of this requires explaining what others *do* and *do not* do in relation to your work.

(E2) *“...we cannot solve (a generalization of our model), but we are confident it would still be subject to the issues we identify...”*

This is one of our favorites. It includes an admission that the analysis is a special case but makes overtures to generality without necessarily going through the (potentially dull) rigor of establishing generality.

(E3) *“...we derive a surprising characterization...”*

This one is instructive because sometimes, just naming an aesthetic in your writing is helpful. No one wants to read about a characterization that isn’t surprising, do they?

(E4) *“...this decision is more difficult than it appears at first blush...”*

This is another one of our favorite conceits in writing. To set a tone of difficulty in your work, you need to actually talk about why it isn’t easy. Don’t leave it implied; make it explicit.

(E5) “...we use [obscure sounding theorem] to ...

Nothing says “difficult” like a mathematical theorem you have never heard of.

(E6) “...new algorithm outperforms state-of-the-art methods...”

This one touches on a couple of different aesthetics. If your algorithm is better than the “state-of-the-art” it is, by definition, new. The hope is that it is also useful. What can be more useful than an algorithm that is better than what is out there?

(E7) “...we believe this approach provides a stepping stone ...”

This statement speaks to another notion of usefulness. Maybe this paper can be a jumping-off point for other papers. Researchers are always looking for more papers to write, so a hint like this could be much appreciated.

(E8) “...although we present our contributions in the context of (some area), our findings have several other applications ...”

Ah yes, the old “this applies to many other things” stab at generality. Sometimes even hinting at other applications, without investing the labor needed to actually elucidate how these applications work, gives enough of a whiff of generality to satisfy the reader.

(E9) “...we abstract away from modeling...”

This is an interesting admission that the paper does not get into some important detail, but this abstraction serves some purpose. This is a nice trope in setting up a simplicity aesthetic.

(E10) “...our work relates to n distinct streams of literature ...”

A mainstay of literature reviews. Nothing says generality like n large in this statement. But beware of stretching the connections too far. Spurious streams of literature can make the writing feel a little desperate.

(E11) “...despite the consensus of the academic literature ...”

This one speaks again to “novelty” but lays the seeds of surprise. It is not just that your work adds to a growing literature in some meaningful

way. It's that your work breaks from consensus, suggesting it represents a surprising paradigm shift.

(E12) “...we propose a unified framework ...”

Classic strategy to emphasize generality. Others have played with pieces and special cases. We put them together.

(E13) “...on the other hand [do some innocent change] and all of these results change ...”

You have a collection of results that seems very fragile and sensitive. Why now package this as a surprising feature instead of considering it a potential drawback?

9.3 Qualities

As always, the six prevalent qualities discussed in [Chapter 5](#) apply powerfully to writing. Many points have been emphasized earlier, so we only elaborate on a few that may take on more prominent roles in the writing process.

The first is implicit in some of the skills. If the essence of writing well is rewriting, then surely good writing requires patience. We (Chris and Runshan) have *never* had the experience of writing taking less time than we planned. It always takes more time ...and then some. Good writing cannot be rushed. And one thing a reader can quickly identify is when writing feels rushed.

But the quality we have thought most about when it comes to writing is empathy. Can we anticipate what will be confusing to the reader? We have known many researchers who are puzzled when others do not understand them. When it comes to teaching students in classes, it is easy to dismiss confusion in your audience as the ignorance of the hearer. But in research, dismissing your reviewers as ignorant is entirely unhelpful. If they don't understand your work, they reject it—end of story.

Empathy for other people's sense of confusion is especially important when writing. What makes it difficult is that you do not have the reader in front of you to see what confuses them. Maybe you can get some students or friends to read your work and tell you where they are confused, but even this is a scarce resource. Ultimately, the more you can develop a feeling for the types of confusion others have, the more you allow yourself to feel your own confusion.

A good way to practice is by using every opportunity you have to explain things to others (say when teaching) and trying to understand *why* your audience is getting confused. Is it just because you are going too fast? Or are certain concepts inherently more challenging than others? Am I saying things in the wrong order? Why do some orders work better than others? And so and on and so on.

I (Chris) will speak very honestly about how I review papers. If I have the feeling that the author is indifferent to my confusion, I am much more likely to reject their paper. My attitude can sometimes evolve into something like: “If you show me you don’t care whether I can follow you, I can show you I don’t care what you have to say.” Not the noblest sentiment, but it is what it is. Insinuating the things I don’t understand are “trivial”, making four steps in one line of a proof with no explanation as a kind of “dare” to see if I can figure it out, referencing details of a paper I have never heard of without explanation and suggesting any civilized person ought to know those details—these are all surefire strategies for me to reject your paper. They clearly tell me I am not the right person to read your paper. These are the results of writing *without* empathy.

Gentleness

The topic of confusion is so important; let’s examine another quality that can give rise to papers that are not so confusing to read. Your path to understanding the material you are writing about was long and arduous. It may have looked like (a) in [Figure 9.1](#). Should the paper be a record of your journey? Not really. There may have been many setbacks, red herrings, and dark alleys in your process of coming to your results. Not all of these are needed or helpful to your reader. So what should you cut out?

One common direction is to remove all the “fluff” and leave only what is *necessary* to understand your ideas. This results in the most logically direct journey through the content. The appeals of this direct presentation are many. First, it maximizes how clever the authors look. It is an unbroken chain of propulsive insights that are essentially impossible to generate in real time. Second, direct treatment has a natural elegance. People enjoy sausages but don’t want to see how the sausages are made. Writing in a direct mode shows the sausage with a minimum of the “mess”. Third, the direct treatment yields shorter papers. This can undoubtedly be a feature in outlets that emphasize

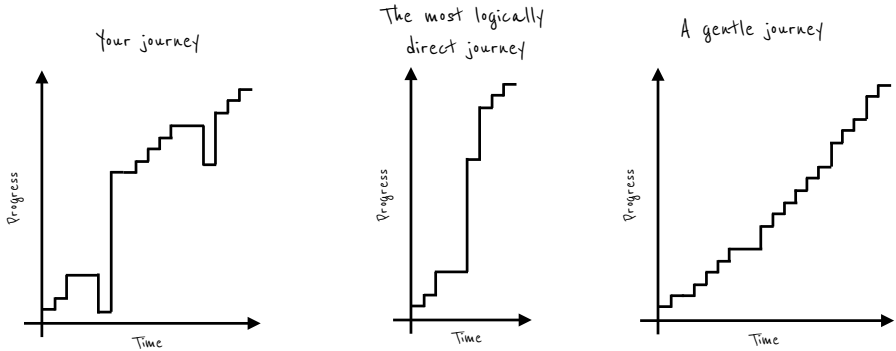


Figure 9.1: Three different journeys

tight page limits.

However, there are numerous problems with the direct approach. What is “necessary” is in the eye of the beholder. When I (Chris) read a paper, it seems most people have a different idea of what I need to follow them than I do. I get lost quickly and find another paper to read. The problem with “necessity” is that it only welcomes those who need exactly what you decide they need.

Second, the direct approach is like asking someone to hike a mountain going straight up the gradient. This is efficient for getting up a mountain but exhausting for an inexperienced hiker. The direct approach makes it harder for others to follow in your footsteps. When fewer people can follow your path, fewer people can extend your journey. It is less likely to become a “worn” path in the future.

You might think at first that having few people able to follow you is a feature. Indeed, if few can follow your path, you can keep adding to your path and getting more and more publications before anyone else can figure out what you are doing. However, this strategy often backfires. An area of research with very few people working on it is not very interesting to most people. They want to feel a part of a movement rather than way off in the woods. Writing in a way that encourages others to follow in your tracks and discover new things along your path is a more powerful path toward acceptance.

Finally, a third drawback of the direct method is that it can easily find as “unnecessary” the imprecise notions you used to make sense of the paper

to begin with. We mentioned this earlier in the context of figures. People often use pictures to explain the ideas of the paper to themselves but then do not include these pictures in the paper. The direct method of writing can justify removing not only figures but, for example, vague partial ideas that communicate the “high level” ideas clearly but are not optimized for rigorous analysis.

I (Chris) recall working on a paper where we had a very formal description of an algorithm that even I barely understood. It was much easier to understand what was happening by handling a small numerical example. We included both the numerical example and the careful description of the algorithm, both being important to grasp what was going on. Of course, it was tempting to cut the small example. We would say, “Hey, a smart reader can devise their own example”. But this presumed they could make sense of our very formal algorithm description. I am glad we included the example. When I looked back at the paper a few years later, I could not make heads or tails of the formal description. The example saved the day.

This underscores a certain kind of irony. Just because something is direct and only says what is necessary does not make it clearer. It may make it clearer *for someone who already understands*, but this is not the intended audience of most papers.

One of my (Chris’s) pet peeves in writing is “guess and verify” proofs in papers that use mathematical reasoning. Such proofs are mainstays of the direct method. For those unfamiliar, this is a genre of proof that comes up with a “guess” for the solution to some problem and then verifies that this guess meets the required criteria. Typically, no effort is made to explain where the “guess” comes from. It’s just a “guess”. What bothers me is that this way of thinking is especially unhelpful for those trying to understand the paper. Sure, you can appreciate the ingenuity of the “guess,” but you can never replicate similar reasoning for yourself. The “guess” comes out of nowhere. The “guess and verify” structure seems, if anything, like a good way to *hide* your thinking from others.

We advocate gentleness. Take time to invite people to journey with you. Explain important ideas in more than one way. For pivotal arguments, add sufficient detail so that the ideas can be recovered without requiring the reader to fill in so many gaps for themselves. The goal is to get across your way of thinking. Show them the completeness of that thinking and its robustness to different starting points or perspectives.

A gentle approach would think about potentially confusing things and warn the reader to avoid going down dark paths. A full exploration is unnecessary, but saying a word or two about why another path is abortive is always welcome. A gentle approach takes the time to clearly make a case for why you think your results are significant. It connects these ideas to simpler, intuitive ones and points out salient differences. It builds bridges of clarity that are not strictly necessary.

We leave this discussion with an analogy we find fitting. We like to think of writing a paper as being analogous to inviting people to join us for tea in our home. If you were to have a guest over, would you make them go into your kitchen, find where you keep your kettle, fill it with water, and open all of your cupboards looking for tea? No! This would not be gentle; this is not how you treat a guest.

Contrast this with a gentle, welcoming invitation to tea. You would welcome them in and help them relax. Instead of having your guest have to search themselves for refreshments, *you* would bring the tea to *them*.

Reflect for a while on what this analogy means for welcoming someone into your “intellectual home”—i.e., your paper—for tea. Are you making it hard to be comfortable and refreshed when reading your paper? Are you making your readers do a lot of the work? Would you want to be invited to your intellectual home, given how you have treated others who have arrived in the past?

Joy

What I (Chris) may emphasize most when talking to young researchers about writing papers is that they need to rediscover and channel joy for their projects. Many young researchers fall into the trap of learning to hate their papers. They get exhausted by thinking about the same ideas over and over again. They know their work is likely not a masterpiece or worthy of some prize. It is just some work that they need to put out there one way or the other. They are also acutely aware of the flaws and limitations of what they did.

The problem is, if you write your paper from this place, that mood can easily be communicated in the writing. No one wants to read a paper written by someone who does not like what they are writing about. They want to feel excitement and joy coming off the page.

This is why I (Chris) emphasize to young researchers that it is essential

for them to return to a place of joy while writing. Luckily, this should not be too difficult to find. You can return to a time early in the project when things were new and uncertain. You can return to the feeling of establishing the first few results and the sense of joy and accomplishment you had. Maybe there is a simple case of an idea that brought you clarity that became more elaborate with time. Perhaps you can rediscover your initial spark.

Writing brings me (Runshan) a lot of joy. I like the feeling of “bringing everything together”. In the process of creating, you explore many different things and have disorganized findings all over the place. Writing allows you to organize your thoughts and findings into something coherent. It feels like guiding your friends through a cool new place you have just discovered. It’s like crafting a sculpture in your mind and presenting it to others. Thinking about how it might delight others brings me joy during the writing process.

Indeed, explaining things well can bring out a natural joy. You feel a sense of satisfaction when a “light bulb” goes off for the reader, and they understand something they did not previously know. There is a natural joy in shared understanding.

Detachment

We featured the quality of detachment in the last chapter on creating, but the concept is worth re-emphasizing in the context of writing. You are not what you write. Writing is something you have to do on the path toward success in an academic career. You have to write no matter if you are inspired, happy, or even connected to your work. If you find joy or a sense of purpose in your work, that is great. But if you produce things you do not find too meaningful, that is also okay. You can have a purpose even when you feel your work does not.

One emanation of detachment in writing is the possibility that you have to restart writing a paper from scratch. This has happened to both of us (Chris and Runshan), and it will likely continue to happen to us. At first, it isn’t very pleasant, but it is always better to let go and accept when things are not working. Retooling old thoughts and old paper drafts can be much more work sometimes than just starting with a fresh page.

Young researchers have sometimes asked me (Chris) whether they should write up the main body of their paper first and then the introduction at the end. In this view, the introduction should be a summarizing exercise only

added at the end. Adding it sooner, you need to change it as the main body of the paper changes. That sounds like a waste of effort. While this perspective makes a lot of sense, there are alternative perspectives.

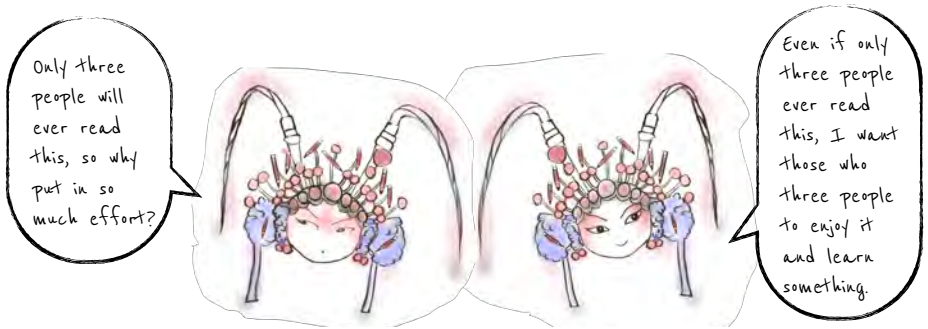
For example, I have *started* many projects by writing down what I imagine *could* be an introduction to the final version of the paper (often in bullet form, but occasionally in complete sentences). Setting things up this way gives me a sense of the direction I want the project to take. The introduction serves as a kind of North Star. I have *always* had to rewrite the introduction, often from scratch, as the reality of the project unfolds. That is fine. I am not attached to the form of the introduction; it serves its purpose of inspiring the work and is always treated as a work in progress. I believe every part of the paper will likely need to be rewritten, including the introduction, many times.

Honesty

One of the things that can turn off readers of your paper is when you over-promise what the paper does in the introduction and under-deliver in the main body. This is not an uncommon occurrence. It can be especially disillusioning for young researchers reading their first few papers to discover this kind of “bait and switch” modality.

Over-promising is dishonest. It is best to be honest (including with yourself) about what you have and try to find the beauty in it. I (Chris) have often found that people don’t spend enough time thinking about why what they have is great. It can take time and energy to conjure grounded positivity and excitement about your own work.

There is a culture of academia in focusing on the negative aspects of everything. We wrote about this in [Chapter 3](#) on research aesthetics. One reason it is hard to learn what research people like is that most comments and questions you hear about research point out things people don’t like. You need to create space to like your own work. There is beauty in everything, including weaknesses. You need the energy to find that beauty. Once you find this beauty, you can say honestly that you love your work.



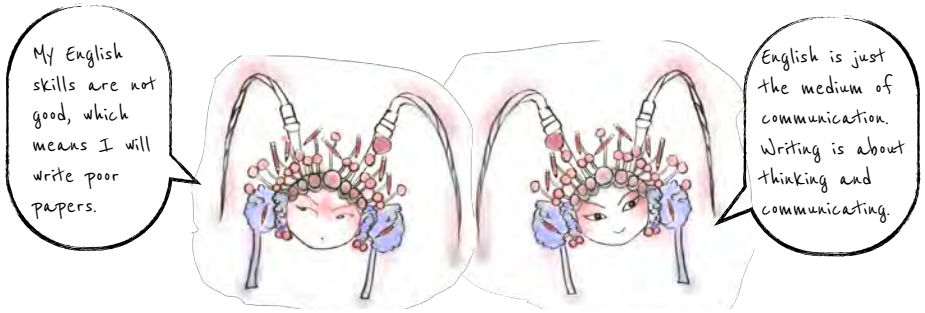
9.4 Attitudes

“Only three people will ever read this, so why put in so much effort?”

How many papers have you read all the way through? Maybe not so many. So is it too much to expect that the papers you write will be read by many? You should not be discouraged by this. The job of academic research is to write papers. Whether they are ever read is somewhat beyond your control.

We make the papers beautiful because that is the job. If you don’t expect anyone to read it, and write it poorly, then this is a self-fulfilling prophecy. Your paper is a show of respect. If you were to read a paper that, by chance, was useful, you would appreciate it if it was written well. We prefer the attitude: **“Even if only three people ever read this, I want those who three people to enjoy it and learn something.”**

At the time of writing this book, I (Chris) have a couple of published papers that do not have a single citation (outside of self-citations)! I am not discouraged. I am more surprised if people actually do read my papers. I think, “Who has the time to read this when they have their own research to do?” The worst feeling for me would be someone getting disheartened by reading one of my papers. I would rather no one reads it at all than someone reads it and feels discouraged.



“My English skills are not good, which means I will write poor papers.”

It is tempting to reduce writing to the final layer of polish and overemphasize its value. One aspect of this is an over-emphasis on the English language itself. Sadly, we see too many students whose first language is not English feel defeated in the writing process because of their lack of confidence in English. While we would never suggest that English is unimportant, we believe its importance should be put into some context.

Much needs to go into writing to get to the place where details of the language start to matter. For example, you can write the outline in whatever language you feel comfortable and think best in. The ideas, not the specifics of the language, are what are important at this stage. Of course, different languages give rise to differences in the structure of human thought, but in many ways, the purpose of the outline is to collect and organize *your own* thoughts. After you have collected them in a way that makes sense to you, you can start the exercise of having them make sense to others.

I (Chris) have worked with newcomers to Canada and the United States for many years to help them gain confidence in their English. I remember one wonderful friend who was accompanying her daughter during her Ph.D. at the university where I worked. This friend was an older woman who knew maybe 200 English words and very little grammar when I first met her. By the time she moved back to her home country two years later, she was a confident speaker of English. She was confident, despite still knowing less than 1000 words and just the basics of grammar. Her confidence was not based on precision in English. She gained confidence by bravely walking up to people and talking to them in English. She is among the most fearless people I have

ever known. She knew what she wanted to say in her heart and used English however she could to get her ideas across. English was a tool for her expression, not something she let herself be judged by.

I (Runshan) used to believe that, as a non-native speaker, I could never write as well as any native speaker. Certainly, being a non-native speaker poses certain challenges in writing, but over time, I have noted two things. First, writing can be and has to be learned and practiced. Native speakers are not automatically good writers, and non-native speakers can keep improving their writing. Second, most academic writing does not require advanced language techniques. Surely it is important to get the grammar correct and avoid confusing expressions. But it is unlike writing poems or novels, which may require more elegant and beautiful-sounding language.

When I read well-written papers, I am usually amazed by how smoothly the text flows, how tightly the logic links, and how clearly the ideas are expressed. But the sentence structures are usually simple and straightforward. I rarely find a case when I read a great sentence in a research paper and know that I could not write something like that because of my lack of English skills (but it did happen a lot in other forms of writing, like when writing this book with Chris). Even when it does happen, I can remember the expression and try something like it in my next writing attempt. This goes back to the first point—writing something that can be learned.

For non-native speakers of English, you may try to think of English more as a tool to express your ideas rather than a source of your ideas or some important barometer of the quality of your thinking. Logic knows no language. This is not to suggest that time does not need to be put into polishing your ability to write in English. This is very important. But it does give rise to our preferred attitude that **“English is the medium of communication I need to use in my writing, but writing is about more than just language, it is about thinking and communicating.”**

“People only care about the results; the introduction and prose are all fluff.”

This attitude reminds me (Chris) of my children (two boys) who watch anime cartoons and fast-forward through all the “boring parts” to get to the battles and action scenes. I tried to explain to them that the “boring parts” make the battles meaningful, but I am not sure they heard me. They were acting out



the battle that they just saw.

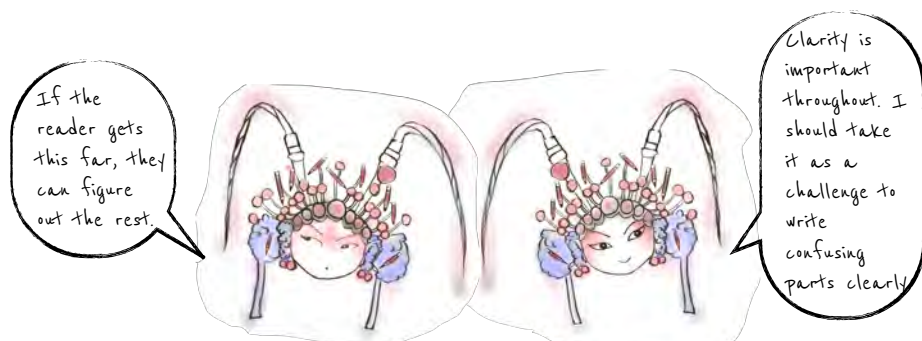
In research, raw findings are like action scenes. This is what everything leads up to. But it is typically wrong to think that the song and dance about why the findings are important or interesting is just filler. Just because you have been thinking all day about why your findings are important doesn't mean others have, even those quite familiar with the terrain. "Fluff" matters. To quote my (Chris's) favorite line from the TV series *The Wire*: "All the pieces matter."

Of course, there exists the opposite attitude of sorts, that *only* the introduction matters since no one hardly reads the rest of the paper. We have known people who mostly read introductions and others who essentially never read introductions. As discussed in [Chapter 6](#), people read papers for different reasons. Some may only be interested in your introduction. Some may only be interested in the details. This gives rise to what we think of a more constructive attitude: **"Some readers will only read my intro, others only care about the details. To be as helpful to my reader as possible, I can make both great."**

"If the reader gets this far, they can figure out the rest."

Many writers start their papers with good intentions. They set out to be clear and helpful to their readers. And then they get tired. As the paper goes on, especially when entering more challenging terrain, there is a natural drift towards less clarity because clarity can take a lot of effort to produce. This drift is perfectly understandable and maybe even inevitable.

However, when this happens, it is very tempting to adopt the attitude that you need to explain *less* as you go along. We have heard justification along



these lines many times. The thinking goes that only the die-hards will dive into your details, and these brave adventurers can do well with only a few signposts on their journey. But this can cover an even subtler rationalization. It is tempting to say “less” about things that are potentially more confusing. Confusing things hold a greater risk that you explain them incorrectly and get exposed as not being as clever as you want to represent. It can feel safer to say *less* and let mystery be your ally. But we prefer an attitude that **“Clarity is important throughout the paper, I should take it as a challenge to write the most confusing parts of my argument clearly.”**

I (Chris) was trying to understand a paper that looked very helpful for a project I was working on. In the beginning, the paper was quite enjoyable to read, but the clarity of the writing dropped precipitously when I reached the middle of a hard proof. At some point, the result seemed just asserted, and many things that confused me were brushed under the carpet. There were a lot of “clearlies” and “obviouslies”. I never doubted that the result was true, but I certainly doubted my ability to understand why it was true. Needless to say, I ended up not using the ideas in that paper in my later research.

9.5 Writing for reviewers

So far, most of our deliberations have been on writing research into papers. This is only one step on the journey of a paper getting published. In peer-reviewed publication outlets, there is a process where reviewers give feedback on your work, and you need to revise it. The final published paper is a combination of the authors’ input and the reviewers’ input. Depending on the reviewers and the field, they can request substantial changes to the original

submission. In some cases, the published paper may look almost unrecognizable when compared to the first submission.

It is also not uncommon for revising a paper to take longer than writing the first version. It's like retrofitting a house. Sometimes it is cheaper to scrap it and rebuild. This can be time-consuming and frustrating, especially if you liked how you wrote the original submission.

I (Chris) have papers that I like the published version *less* than the original version. It was painful to take steps to change the paper in a direction that I thought made it worse. But my job is to write papers that appear in journals, so this is just a part of the job. And I have to admit the vast majority of the papers I have been involved in were improved during the revision process.

One reaction you might have about the revision process is something like: "If I make the paper perfect, the reviewers will be happy, and I will not have to revise it." This would be great if true, but in our experience, this never happens. Reviewers in the peer review process are responsible for holding up the rigor of the journal. For many of us who review, this gives us a sense of being responsible to point out something ...anything ...that we think the paper should do better. In other words, no matter how good you think your paper is and how hard you work on it, the reviewers may inevitably find something about it that can be improved. This is one of the strengths of the peer review process but also one of its major points of frustration for authors.

While striving for perfection may not be attainable, we do not suggest that you write a bad paper so that the reviewers find it easy to find flaws. The strategy is something closer to this—write the paper well enough not to get rejected for being obviously flawed, but allow space for the reviewers to contribute. This balance is not easy to get on the first few papers you write. This comes with experience and is quite field dependent. But we firmly believe that there is no way to avoid criticism. It is better to embrace it and plan for it. We take the view that the paper is a collaboration with the reviewers of the outlet. We cannot finish the paper until we hear from them during the review process.

9.6 Putting it all together

As a form of summary, we provide an example of young researchers exploring the ideas in this chapter. The story is fictional but inspired by real-life interactions

with many young researchers over the years. The main purpose is to provide you with an example of how natural it can be to discuss your approach to research with those you trust.

Soham had a good summer doing research after his first teaching semester as a new assistant professor. He could answer the research question he thought of during the one-day-a-week research allotment he had given himself while teaching. Despite feeling good about where he got to, it was time to write his results in a paper. This made him nervous. He long had an issue with writing.

During his Ph.D. years, he had to meet his advisor repeatedly before she allowed him to move on from a paper. This was immensely frustrating to Soham because most of the edits seemed arbitrary or unnecessary. When he explained his ideas to his fellow Ph.D. student at the time, they all seemed to understand (or at least they pretended to). But his advisor seemed not to understand. She would sometimes have hundreds of comments on his drafts. Thinking back, he found the process so mysterious. At the time, his question was always “Why is she so picky?” Now his question was more “*How* was she so picky?”

After feeling overwhelmed for a week or two, he decided it was not helpful to anticipate the problems with his writing; he should go ahead and write things up. Soham started to write. He wrote up his analysis part first. That is the part he liked, so he found this relatively easy. When he started on the introduction, he got frustrated. He asked himself: “Why do I need to add all this fluff onto this beautiful argument?” Soham loved the papers from the 1950s that were short and to the point. He wondered why he couldn’t write one like that.

Soham took a few weeks off of writing to start on a new project—the stage of research he greatly preferred. When he dragged himself back into writing the introduction for the old project again, he remembered that he needed to give a talk next month at a nearby university. He decided to present his old paper and went about making slides. He figured making the slides first may help him to write the rest of the paper.

And he was right. To make the presentation, he needed to think about what was important. Talking it out and organizing it into discrete chunks (slides) gave him a plan for the introduction. He printed out his slides when he was done and wrote sentences based on them to make his introduction.

Soham thought it would be a good idea to post the draft of his paper on-

line before the talk, but he wanted additional feedback on what he had written before doing so. He was pretty happy with the paper, but he recalled all of those weeks of revising that his advisor put him through. Can he get away without doing that now?

Soham sent his paper to some members of the faculty in his department. Most of them said they were too busy and would read later. Several others gave very short feedback, like “Congrats!” or “Nice paper.” He was hoping for something deeper.

He decided to approach one of the graduating Ph.D. students, Elis, and ask her for help. Elis had just submitted her dissertation and had the time. Soham gave her the paper, and about a week later, they met to discuss it. Soham was quite nervous about what her reaction would be. He knew he was sensitive to negative feedback but understood that it could not be avoided in a career as a researcher.

Soham: Elis, thanks for taking the time to read the paper.

Elis: My pleasure, but you might not like my feedback.

S: Why is that? (nervously)

E: Well, I didn’t understand what was going on in the introduction. If I wasn’t trying to help you, I might have quit reading the paper right there. But I pressed on, but I never felt I knew what the paper was about.

S: Sorry to hear that.

E: I guess it felt like I needed to know much more to understand. For one, you assumed that the reader was very conversant with Reference 12. At first, I tried to get away with not reading it, but I realized it was impossible. When I searched for Reference 12 online, I found it wasn’t well cited. I could not even track down a PDF of the paper from our library. Is this an important paper that we should all know?

S: Hmmm, good question. I was maybe thinking too narrowly. Although I consider the paper to be really important, I think I overestimated how widely known it was to others. Let me explain the idea of Reference 12 and how it relates to my paper.

Soham takes 10 minutes or so to explain, writing on the whiteboard in his office.

- E: That picture right there. Why is that not in the paper? It makes everything so clear.
- S: Yeah, that picture is how I really think of it. I guess I didn't include it because I thought it might be a little misleading. The real story is more complicated than the figure suggests.
- E: But without the picture, no one will care about complications. You should include the figure and then explain the subtleties in the writing. If the picture is how you understand it, why not let people understand it like you do?
- S: Good point; I have never thought of it that way.

Soham and Ellis go over many small edits, grammar, and confusion in the analysis section. To Soham, it brought back many of the memories of those sessions with his advisor. Forty-five minutes into the session, Soham's frustration starts to boil over.

- S: You are even confused by that?! Wow. I thought I explained it so clearly. I spent most of my time coming up with this explanation.
- E: I just tell you from my perspective. Maybe it is a good explanation, and I am just slow. Does it matter if I get it? I am just a Ph.D. student, after all. Maybe your audience is more like an expert.
- S: No, you have invested all this time in understanding it. I would hope that someone who takes a week to read my paper—and can get a Ph.D. in our field—should be able to get it. And in its current state, I am pretty sure I don't want an expert to see it. I am too embarrassed!
- E: No need to be embarrassed; it just means if you want me to understand, you need to say it another way.
- S: Okay, let me give it a try.

There is another interlude of Soham writing things on the whiteboard, with Elis taking notes and asking questions.

- E: Ah, I see. That is very clear now ...and very clever too.
- S: Yeah, this is a much better way to say it. If I explain it this way, I don't need to lean on Reference 12 at all, and it makes so much more sense. Thanks for listening to me work through this.
- E: Happy to do it. I made so many people sit through me explaining my dissertation work. I am happy to pay some of that time back.
- S: The good news is that I think I can explain all of this much better now. The bad news is that I think I will have to rewrite all of the analysis. Adding that to the fact I want to rewrite the introduction to avoid dependence on Reference 12, it looks like I am starting from scratch.
- E: Not the first time or last time you'll have to do that.
- S: True, true. Thanks again.

After the meeting, Soham felt defeated. There was so much to do, and starting again from a blank page overwhelmed him. He realized that by now, he had spent more time writing the paper than actually coming up with results. This thought brought him back to those dark months during his Ph.D., when it seemed like he would never return to doing research. He dreaded those months of tweaking words and sentences in meaningless ways.

Soham spent the next three days avoiding thinking about starting the rewrite. But every time he saw Elis around the building, he felt guilty that he had not started implementing some of her suggestions, which were all so helpful. She was able to pick up many of the things his advisor would have seen. How could Elis see it and not him?

The second draft took Soham twice as long as the first to write. He sat on the draft for another week; he couldn't bear the thought of receiving negative feedback again. The thought of Elis pointing out all of these flaws again in his paper overwhelmed him. Soham was genuinely grateful for Elis's help, but he also felt embarrassed by how bad he was at making himself clear. So Soham decided to submit the paper without asking Elis's help again.

Six months later, the paper came back from the journal. Rejected! By then, Soham had gone on to other projects and even considered dropping this old paper entirely. Would it be worth spending even more time on a failure?

Soham forced himself to read the three reports of the reviewers, and his mood changed. All three reviewers seemed to like the idea of the paper but had various comments on how the paper was poorly executed. They were not rejecting his idea, only how it was expressed.

After working through the detailed comments of the reviewers, he realized he would again need to change the paper's emphasis. It was a third complete rewrite. But this time, it did not take nearly as long since he had a much clearer plan of what he wanted to do based on all the reviewer's comments.

When he finished the newest draft, he did not want to make the same mistake as before; he asked Elis for help again. She was now doing her postdoc at another university, but luckily she said she was still willing to help.

She again found many small mistakes, but fewer than before. Soham was becoming aware of common mistakes and learning how to avoid them. Soham thanked her by sending her a fancy notebook. He wrote on the first page: "To Elis, the one with an eye for detail. In admiration, Soham".

Soham sent the paper to another journal. Four months later, the reviews came back positive: a conditional acceptance after some revisions. Just before submitting the final version for publication some months later, Soham looked back at the original first draft of the paper he first discussed with Elis those many months ago. They were hardly anything alike. Soham had learned a tremendous amount about writing in the process, and now it felt like writing was as much a part of research as anything else.

9.7 Reflection questions

The questions below are provided for you to reflect on what you have read in this chapter. If possible, it is beneficial to discuss these questions in a group of your peers.

1. Describe an experience you had writing a paper (it could be a term paper for a course or a research paper). How did it make you feel? Did you feel confused? Did you feel empowered? What did you learn?
2. Of the many steps involved in writing, what steps do you find most difficult? Easiest?

3. Do you aspire that the papers you write are understandable to Ph.D. students in your field, or are you comfortable if your paper is only understandable to experts in your subfield?
4. Describe a story (it could be a story of your life or a fictional story) that you have told to your friends or family. What makes the story interesting? Why did you tell it?
5. In what ways are research papers like stories?
6. Do you use an outline when writing? Why or why not? If you do, how do you decide what details to put in your outline? How do you structure it?
7. How confident are you in your English grammar? What can you do to boost your confidence?
8. What are some common mistakes you make in your writing?
9. How do you know when a piece of writing is done and ready to be sent to the intended reader(s)?
10. In what way, if at all, do you relate to the story of Soham and Elis? What do you take away from the story?

Chapter notes

There are many good books on writing. For general guidance on effective writing, see [21, 27]. For thoughts on mathematical writing, see [11, 13, 14]. For writing in economics see [16, 22]. There are more informal sources including slides² and YouTube videos.³

²see, for instance, https://www.mit.edu/~dimitrib/Ten_Rules.html

³see, for instance, <https://www.youtube.com/watch?v=ND1By203Eu4>

Chapter 10

Speaking

A big part of research is communicating ideas. The chapters so far have highlighted receiving ideas (in **Chapters 6 and 7**) and writing ideas onto paper (in **Chapter 9**). The next two chapters focus on two-way communication about our research ideas.

Of course, speaking about your research comes in many forms. You can give an elevator pitch of your idea at a conference. You can explain things to your colleagues. You can discuss with your collaborators. The focus here is on speaking in the context of a research presentation, or simply, “giving a talk”.

It is hard to overemphasize how essential talks are in the journey toward acceptance in a research career. One of the key milestones in that journey is getting a job, and all formal job interviews for academic research jobs we have ever heard of involve giving a “job talk”. What we share here will not be specific to the “job talk” context but instead any research presentation, including talks at conferences, department seminars, and even talks in the context of a Ph.D. class. Mastering the art of presenting is important at every stage of your academic career. It is the most public exposure you have as a researcher, and how you handle yourself in presentations goes a long way to establishing your persona within the research community.

10.1 Purpose

There is a lot of overlap between the purposes of writing a paper and presenting a paper. So the four purposes we described for writing a paper—to

record, to educate, to impress, and to belong—apply quite analogously here. But there are also important differences.

One difference is obvious; a talk is much more of a social space than reading or writing a paper. It involves interactions between the speaker and the audience. Accordingly, some of the purposes we described for *listening* to a talk—public display of talent, showing respect, and generating a sense of belonging—apply equally well to *giving* a talk. The purpose of publicly displaying talent is more enhanced for the speaker than the audience members (more on this below).

This overlap with the purposes of capabilities we discussed in previous chapters does not exhaust our understanding of the reasons to give a talk. Let's discuss a few more.

Advertising the paper

One commonly expressed reason to give a talk is to “advertise the paper”. The wisdom here goes that the talk should really just highlight what the contributions of the research are, with the hope that people will read and take up the ideas of your paper.

Talks are a powerful way of communicating the main idea of a paper. In each of my (Runshan's) papers, I usually have a key message that I would like to send. This key message is illustrated by a lot of analysis in the paper, but reading through the analysis could be daunting and deter many people. I view presentations as an opportunity to communicate the key messages to a broader audience. If someone becomes interested and later reads my paper, that is great; if not, I am happy to share what I think is most important to more people.

I (Chris) have not subscribed too much to the idea that a talk is advertising for the paper because I am not always sure I *want* people to read my paper. I would sometimes rather they get the gist of the idea of my talk so that they never have to read it. Maybe that “gist” is enough to inspire them to write something of their own. My admiration and thanks go to those who dive into the gory details of my paper after the talk, but this is certainly not an outcome I am expecting or even hoping for.

This hints toward an even more pointed expression of the advertising purpose for giving a talk. Maybe you are gearing up to submit your paper (or have already submitted it) and want to get the word out that this would be a fun

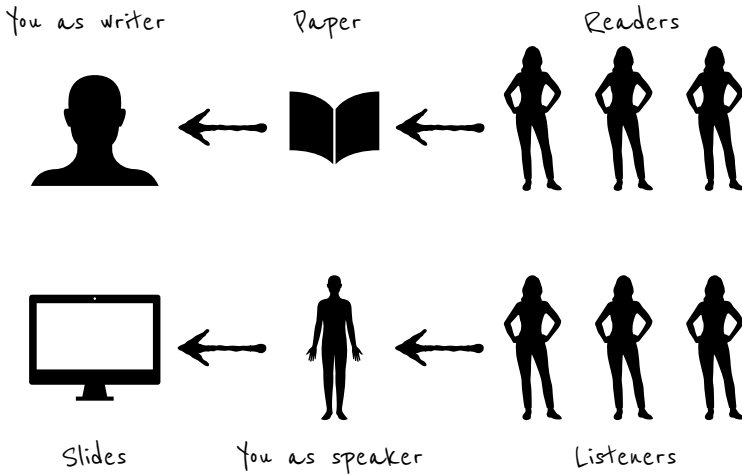


Figure 10.1: Talks present the person, papers present the work

paper to review—and ultimately accept—into a journal. Through the talk, you can highlight what is great about your paper that would take a lot of effort to glean by only reading it. Maybe by giving a talk, a reviewer who listens can form a good impression of the paper without investing so much energy in reading it.

Advertising you

The previous purpose, however, possibly overemphasizes the importance of the *paper* as the focal point of a talk. The other object under scrutiny is the *speaker*. That is, *you*. An important reason to give a talk is to advertise yourself. You need to be accepted into this community to succeed. Getting papers accepted is one thing. Getting *yourself* accepted is another.

You might think people can know you well enough through your papers, so why do they need to see you giving a talk? There are a few reasons. First, people learn about you indirectly by reading your papers. What they can observe is your paper, not you sitting across the table from them while they read. Talks are the opposite. When you give a talk, you literally stand in front of the material you present. See [Figure 10.1](#) for an illustration.¹

Second, most papers in most fields are co-authored papers. These papers

¹All images in this slide are from the stock clip art in Apple Keynote.

represent the collective work of more than one person. By contrast, virtually all talks are given by one person. There is nothing for you to hide behind, including your co-authors. Talks better communicate your independence and originality of thought.

Third, talks demonstrate your dynamic and spontaneous ideas about the topic. Papers can be prepared over months and years. They are not performed in real time. Talks can be prepared to some extent (see many details below), but preparation is always limited. The audience can interject new ideas or questions that can put you “off” script. When you are put on the spot, the audience learns more about your overall intuition and grasp of the paper’s content. The audience can quickly separate a knower from a pretender during a talk.

Fourth, you are exposed emotionally to the audience when you give a talk. The audience can learn more about your character and beliefs than through reading your paper. For example, they can see what excites and what bores you. They learn how patient you are. When you are standing in front of someone, you are no longer a disembodied mind. You have a heart and a body. You are a whole person.

Seek feedback

In some fields, it is common to present completed work. You may even summarize several of your recent papers to give an overview of what you have been thinking about in the last few years. In other fields, it is more common, even implicitly mandated, that speakers present working papers that are not yet published. The logic goes something like this: the role of the audience is to share their thoughts and concerns about the paper. If the paper is already published, then why is the audience there? They can’t make any comments that will change anything.

In such environments, one of the main reasons to give a presentation is to get feedback on your work. Presenting your work to others helps you clarify what you want to say. You can test out an explanation or emphasis. Since a talk is not permanent—it is performed in the moment—the good can be kept and not-so-good dropped. Iterating a few times can improve your presentation and paper concurrently.

You might think you can get feedback on your work in other ways. For example, you can send a paper to others for comments. Our experience (Chris

and Runshan) is that this hardly ever translates into much concrete feedback. You can get a lot of “Way to go!” and “I will read with interest” messages but relatively few thoughtful responses and concrete comments. This is perfectly understandable. People have their own research to do, and reading a paper to give concrete feedback takes a lot of energy.

How is a talk different? The audience is essentially stuck there listening to you. Sure, they can get up and walk away (or turn off their cameras and walk away in an online setting), but this exerts some social cost on them. The most collegial thing to do is stay and listen through the talk. The audience might not extend this courtesy to you, but they might show up and stay anyhow to be collegial with the other audience members. The speaker can use this to their advantage. You have a captive audience. Use this opportunity to interest them and seek their feedback.

Start a conversation

A final purpose for giving a talk can be something far more innocent. It is not about advertising your paper or yourself but simply posing questions that interest you and starting a conversation with others. I (Chris) remember a talk at a conference where the presenter did not present any results. Instead, she gave a list of problems that interested her and current conjectures about how to best approach these problems. The purpose of the talk was to interest the audience members in working with her on tackling these problems together. I found it very refreshing, but I have not had the opportunity yet to replicate this approach in any of my talks.

Sometimes an area of research has a narrow audience of interest. Maybe the paper will be read by a few experts at most and not appreciated by a general audience. The speaker may want to expose more people to an interesting area. I (Chris) recall one talk at a conference where the presenter presented six or so results, five of which were already known but to a very narrow community. The talk was as much about advertising the research area as advertising the researcher’s new results.

Giving talks to start a conversation is a relatively time-effective way to expand interest in your field. Review papers may serve a similar purpose but are a much more significant investment to write.

10.2 Skills

Giving a good academic talk is not an easy task. One reason for this has already been demonstrated in the previous section: there are many overlapping (and sometimes potentially competing) purposes for giving a talk. Coming to understand what your primary motivations are for giving a talk is already a lot of work. But, adding to this, many challenging skills are unique to giving talks in the research process. Some of these skills are performative and relational. These skills can feel in stark contrast with other parts of academic life, which require careful thought, deliberation, and, often, isolation from others. By contrast, giving a great talk can require emotion, spontaneity, wit, and social graces.

Understanding your audience

First and foremost, as a speaker, it is essential to understand your audience. Different audiences have different tastes and different unspoken conventions.

But how to learn about your audience? There are a few ways. Maybe you have some insight into how this audience reacted to other talks. During a conference, you may be able to observe the same audience listening to other talks during the day. You can see what excites them or what types of questions they ask.

But even if you have never observed your specific audience before, there are things you can learn. To the extent that there is a common culture across your academic discipline (which there typically is), learning about one audience gives you insight into others. Sitting and listening to talks in your own academic department can give you insights into audiences elsewhere. Even watching recordings of academic talks helps you learn about what tropes and approaches might work well.

Your work as a speaker does not start when you start making presentation slides. Some of the work can happen days, weeks, months, or years earlier as you learn about your audience. This means developing an appreciation of the “performing arts” in your academic field.

The audience of a talk is often more general than a paper. Attendees in the audience are more likely to have varied backgrounds. For example, giving a talk in an academic department often assembles researchers and students with various research interests. They are brought together through social connec-

tions, and not only intellectual connections, to sit through your talk. Readers of your paper, by contrast, will likely only do so if they have an intellectual interest in what you have done in your research.

On the other hand, in some cases, your audience can be much narrower than for a paper. A good example is when you go to give a talk at another department. You can prepare by studying who the researchers and students are in that department. You can read their websites and note their interests to tailor your talk accordingly. The talk only exists in that time and place. A common strategy (although I (Chris) tend not to follow it) is to tailor the literature review to include papers by authors in the audience. While this is an excellent example of what we mean by tailoring, you must be aware of the possible scent of pandering. We talk more about this below.

Storytelling

Another skill is finding a way to tell a gripping story in your presentation. A story is a retelling of a sequence of incidents or events. A story has character, theme, and structure (beginning, middle, and end). Storytelling shows up in writing papers, but the nature of storytelling in talks is different. When presenting, you are naturally a protagonist in the story. You are telling people about *your* research, and *you* are standing there right in front of them. The audience will naturally be interested in the role you played in the narrative you present.

I (Chris) have evolved my approach to presenting over many years. In the first few academic talks I gave, I saw myself as a dispassionate and objective reporter of “the facts”. I did not enjoy these talks, nor did my audience. Over time I developed a style where I view the talk as an opportunity to recount the adventures I went on with my co-authors to come up with the ideas. It is common for me to say things like:

- “When we first started this project, we thought about it in completely the wrong way ...”
- “Eventually we came to this, which really shocked us ...”
- “I don’t know about you, but we found this part quite confusing at first ...”

- “Let me take you on a brief detour into how we discovered this connection.”
- “This is the idea that turned everything around for us ...”

This helps the listeners understand how your ideas evolved, what excited you, what challenged you, how you overcame challenges, and what makes the story still ongoing. This builds in drama, suspense, and character development (the evolving understanding of the speaker/protagonist with time). A telling in this style also, I believe, more accurately represents the reality of doing research. Research is not entirely an objective or dispassionate exercise but often involves a haphazard journey of collective human will.² I also need to build a sense in the audience that they, too, are part of the story. Indeed, giving a talk is a culmination and validation of many hours of toil.

This way of approaching the talk also reveals something about my (Chris’s) view of the type of story the audience wants to hear. In an academic audience, the listeners are primarily academics. They are interested in doing research. They are not passive consumers of my results but active producers in their own right. So what is most interesting to them? An explanation of some results? Or, an explanation of how a group of people came to those results? I think the latter is more interesting and, ultimately, more helpful.

In summary, the last couple of paragraphs represent my attempt to answer the natural question that an audience might have: “Why are *you* standing in front of *us*?” My approach is to bring myself and my co-authors into the story by making the talk about our journey. The goal is to help the audience get some ideas about how they might follow a similar journey for themselves. Of course, this is just one answer to the question.

As for my (Runshan’s) perspective, I have found it essential to clearly understand the paper’s story before I can give a gripping telling of it. It is easy to be buried in the detailed results and complicated analyses that got you where you are. Storytelling requires you to step back and think carefully about what you want to tell. A simple test I usually use is to frame the story in just a few sentences and see if it is logical and interesting. Next, I try to explain what I

²In writing this, I somewhat align myself with the thoughts of Polanyi [18] and Latour [15] on the practical and social nature of conducting science. This can be contrasted with the positivist and falsificationist views of science more standard in my academic fields (operations research, operations management, economics, etc.). If you are interested to learn more, I suggest you read these references.

have done in a way that fills five minutes. I will only go with more details if I am satisfied with the first two steps. No matter how complicated the analysis is, a compelling story should not require too much effort from the audience. If the main story in a few sentences cannot convince me that it is worth sharing, then it is unlikely that others would find my talk interesting, no matter how many “bells and whistles” I add.

Providing clarity

One essential skill in giving successful talks is making your ideas and explanations clear. Let’s reflect for a moment on what we mean by clarity. We will take “clear” to mean that people can make sense of and relate to what you are saying to the extent that they can own a piece of it for themselves. Let’s take some time to deliberate on what we mean by this.

Clear is not the same as “smooth”. A smooth explanation is one where all of the handles have been shaved off. It says exactly what is necessary to understand the idea in a streamlined and logical way. Those who already know what you are talking about, they will find your explanation elegant and economical. Those who don’t already know will risk looking stupid to admit they do not. A smooth explanation may look something like this:

Now, let me explain our main idea. It’s just like the classical and beautiful thoughts of AAAA, except that we relax exactly one assumption. This relaxation is very straightforward to state but requires a completely different argument to re-establish the result. Others have tried relaxing this assumption and failed. Although our argument is completely new, it is also very easy to see why it works. You simply need to verify that a new construction, which is nothing other than combining the approaches of BBBB and CCCC (which are both classical, by the way), gives you precisely what you want. It’s a two-line proof once you have the right insight.

In this explanation, the speaker is skilled at making her results seem both completely natural and impossibly clever all at once. Everything is built on ideas everyone ought to know, but it is also completely new, and no one has been able to recognize it until now. This suggests that others were “blind” to the elegance right in front of them, making the feat all the more impressive. The

last line about the proof being impossibly short is one of my (Chris's) favorite boasts in mathematical circles. It often turns out that these two lines were printed on poster paper in landscape mode.

Think about this explanation from the perspective of someone who is not so familiar with this area. While it seems pretty helpful, you have no idea what is happening unless you know AAAA, BBBB, and CCCC. But it is claimed that AAAA, BBBB, and CCCC are all “classical”, which suggests the audience *ought* to know what these references are. If you have questions about how the proof works, you are faced with the claim that it is dead simple once you have the right insights. Since these insights are “classical,” you must have already had them if you were well-informed. Again, the only possible explanation for not understanding the argument is your own ignorance and oversight.

This can be contrasted with a much clearer explanation. It might go something like this:

Let's recall for a moment what AAAA did. If you are new to her work, no problem; let's go over the main idea. (Short interlude to capture the main idea.) Now, this idea worked, in part, because of this one assumption. But what happens if we relax that assumption a bit? There are many possible ways to relax it, and many people have tried different ways. We wanted to build a relaxation that would leverage our recent exposure to BBBB and CCCC. If you are unfamiliar with these two papers, it might be fun to look into them; we learned a lot when reading them. What these two papers do, in a nutshell, is say _____. That's very convenient for us because if we can leverage _____ we can recover much of AAAA's arguments as before. The real trick of our argument is making the right assumption to apply BBBB and CCCC. We got it through an iterative process of thinking about what we needed and what made sense to assume. This part was not easy. In the end, we came to something that, in retrospect, appears quite natural but felt completely unnatural when we first thought of it. Here is the clean way we came to think about it: _____. We are delighted with how it turned out.

Think of this explanation from the perspective of the audience. It opens the door for them to ask questions about AAAA, BBBB, and CCCC since the speaker admitted that it is quite possible not to be exposed to this work. Also,

it reveals more of the mechanics of how the argument works. It takes AAAA's argument and attempts to relax part of it so that the ideas of BBBB and CCCC can be leveraged. This gives the audience a big picture of the "work" that the speaker did. It also allows them to ask follow-up questions about what was hard about formulating the assumption or what was about BBBB and CCCC that gave the speaker hope the result would work. The speaker lets the audience know what parts of the argument were difficult and where much of the project's energy went into. This, of course, gives the audience members space to be confused and struggle. The speaker allows the audience to share in the sense of challenge and joy inherent in the work.

So how can you provide clarity in your explanations? An initial step is recognizing that your latest way of understanding the work (and maybe written up in the paper) resulted from an evolution in thinking. A clearer explanation might reveal some points on this evolution. In this regard, it is helpful to remember the intermediate steps or partial understanding you had along the way. Looking over a history of emails with collaborators or rough notes from the early days of a project can be helpful. Sometimes you can find an easier setting that communicates the idea more clearly.

This relates to another vital distinction between clarity and precision. Sometimes vague things are easier to understand. When someone asks where you live, do you offer them the longitude and latitude of your home? No. You might give the country, city, or street name depending on the context. The latter answers are less precise but are often more meaningful.

An academic talk goes by very quickly. To make your explanations meaningful to the audience, they must necessarily involve some higher degree of vagueness than your paper. Maybe the paper provides the longitude and latitude, but the talk only needs to offer the city name. For where the audience is at, the city name is probably more meaningful. I (Chris) truly believe that clear and somewhat vague is better than precise but impossible to follow. Of course, you should not be misleading (more on this below). But the idea that you need to be very precise out of fear of being misleading may only result in not leading anyone anywhere.

Making slides

Nowadays, preparing to give a talk almost always involves preparing slides for a presentation on a projector attached to a computer. A few fields might still see

“chalk talks” or entirely oral presentations, but slides are increasingly taking over these fields too. It is an important skill to come up with great slides.

Numerous YouTube videos, books, and references offer advice on making elegant and meaningful slides. I (Chris) am a voracious consumer of such advice. We will not re-create the wheel on this front. Instead, we offer a few high-level insights that we have found helpful in the slide-making process.

But before diving into our insights, we want to say a few words about the importance of slides. When it comes to writing papers, the physical paper is the entire output of the process. You cannot put *yourself* into the journal or conference submission system. It is, therefore, a mistake to create too strong an analogy between the *slides* of a talk and the *paper* in the writing process. Slides are only *part* of a talk. Some even feel that the slides are a minor part of the talk. More importance can be given to what the speaker says and how the speaker handles questions. Our opinion is more mixed because slides are a handy tool for the speaker to manage what they want to say *and* answer questions. Let’s look at a few things that we believe make for more effective slides.

First, it is a good idea to make an outline for your slides, just like you would make an outline for writing a paper (as discussed in detail in the last chapter). I (Chris) approach this through “storyboarding” and do not use a computer. I draw slides like cells you might see in a comic book. This gives an idea of the flow of slides and what is on each one. I have sometimes used index cards to move slides around to get the flow right. I prefer to do this by hand so that I have no hesitation in drawing pictures and placing things in the cells wherever I want.

It is tempting to put many words on slides as like a “record” of the story and flow you hope to go over. But writing many words is bad for several reasons. First, it makes the slides look crowded. There is a reason you are using slides and not just projecting your paper on the screen. There is a lost opportunity if the slides look just like a paper with a few words here and there removed. Aesthetically, busy slides may give the feeling that not much effort was put into thinking about the message.

Second, slides with too many words on them can distract the listener. They try to listen to you speak, track where you are on the slide, and maybe even read the slides on their own. Where should they focus their attention? You can use laser pointers and hand gestures to help focus, but people might miss where you are pointing if they are distracted reading a different part of

the slide.³ If you traverse the slides nonlinearly with lots of gestures, it can be dizzying to take in for the listener, even if they can maintain focus.

One way to combat “busy” slides is to use animations. By animations, we don’t mean cartoons; we mean setting up “clicks” for the content on a slide to reveal slowly as you click. This can be used to uncover material in a slide that is *eventually* busy and full of words but in a way that makes it more accessible. The “clicks” also help the listener be clear about where you are and what you are talking about.

But this reveals a third downside of busy slides. If you use “clicks” to reveal information slowly, *you* need to remember where the slide is going. When you are up in front of an audience, it can be easy to lose track. If the only clicks you need to anticipate are slide-to-slide, this is much less to keep track of.

This leads to a final downside of busy slides. You can easily confuse yourself when multiple messages are packed into a single slide! Like the audience, you can get tripped up in what you are saying, where you are pointing, and what the slide says.

Given the downsides of busy slides, what are some things you can do to avoid them? We thought it might be most helpful to give some examples. One essential tool is to use images. We have spoken about using images in the chapter on writing, but the scope for using images in a talk grows exponentially. Let’s look at a few.

Consider the slide in **Figure 10.2**.⁴ In this image, we have defined the concept of *kidney exchange*. But before examining the figure, let’s describe the concept in words.

The idea of kidney exchange is that two patients need a kidney, and two loved ones are willing to give them a kidney. Sadly, the patients and their loved ones are not a *match*, meaning neither patient can accept the kidney of their loved one. Happily, if the organs are *exchanged*, meaning that the loved ones offer one of their kidneys to the *other* patient, then the kidneys can be matched. Instead of having two patients suffering from kidney failure with no one to help them, both patients benefit from a new kidney.

³Some seminar rooms have the baffling (at least to us) practice of projecting the same slides on multiple screens at once. This only serves to amplify the confusion of the audience on where to look.

⁴The hand and kidney images are stock clip art in Apple Keynote. The image of the dialysis patient is from the copyright-free repository of clipart: <https://clipart-library.com/clipart/1465512.htm>

Kidney Exchange

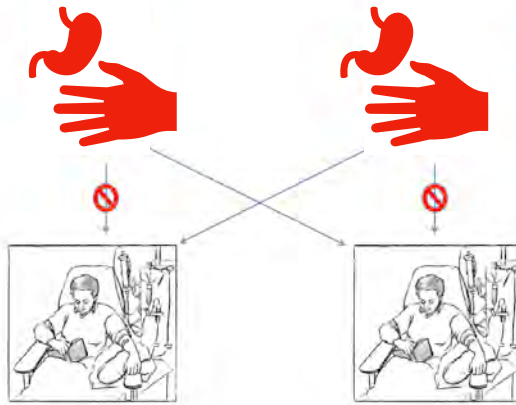


Figure 10.2: A slide used in defining a concept.

It was hard to write that out in words. The words might even confuse you (we wouldn't be surprised).⁵ How to condense these words onto a slide? We feel the description in the previous paragraph is about as short as we could make it. We could make the sentences into grammatically incomplete “notes”. Consider something like this:

- Kidney patient and loved one
- Patient cannot accept kidney from loved one
- Consider two patients and two loved ones
- Exchange: Patient 1 can receive a kidney from Loved One 2, and vice versa

That is pretty good, but there are still quite a few words up there for a presentation. You might also need to “click” to get the bullets out to ensure the audience listens to your logic in order.

But now let's return to the slide in **Figure 10.2**. The picture says it all; the speaker simply needs to *illuminate* it. Consider the following illumination:

⁵And we also guess that you looked back at the picture as you read. Were we right?

This picture illustrates our problem. You see these two dialysis patients [pointing to the bottom two images]. Each has a loved one willing to donate [pointing to the top two images]. But there's a problem. Do you see these arrows that are crossed out? That means they don't match! But what if we took these two diagonal arrows? That would give us a match. This is the idea of kidney exchange.

The illumination comes in the form of a picture and a telling of the picture in spoken words. The image does a lot of the telling. The images of the patients and donors tell you visually what defines them. The arrows are suggestive of the idea of donation. The concept of “match” does not need to be explained in written words; it is implicit in the picture: the donor cannot send her gift to the recipient. The meaning of the diagonal arrows is also implied.

This illustrates a concept well-known in journalism circles: *show, don't tell*. In the first case, we were telling the audience about the phenomenon of kidney exchange. With the image, we simply show them the concept of kidney exchange. We just need to help the audience understand what they are seeing. That is why we used the word “illumination” instead of “explanation”. The image remains the “explanation” of the idea; what we say out loud just brings that explanation to life.

You may be asking yourself, where to find the perfect picture to illustrate my concept? I (Chris) have a little trade secret for you. Search up your concept in Google Images. I found the images in [Figure 10.2](#) by entering the words “donor” and “kidney patient” into Google Images and selecting my favorite results. A picture is worth a thousand words, so why not let Google convert your words into a thousand pictures?

Let's consider another example, the slide in [Figure 10.3](#). This slide does not define a concept but instead sets up a mathematical model. The idea here is that those with a phone can download an app to become a “user” of that app. This is called *acquisition*. Of course, users are not required to stay users for very long; they can quit using the app. This is called *churn*. How the company makes money is not from the user downloading the app; instead, as the app is used, there are opportunities to *monetize* the user when taking certain actions. The decision is represented on the right with the “knob”. The designer can make the app more or less “extractive” or “accessible”. This changes the rates of acquisition, monetization, and churn. In the slides, ani-

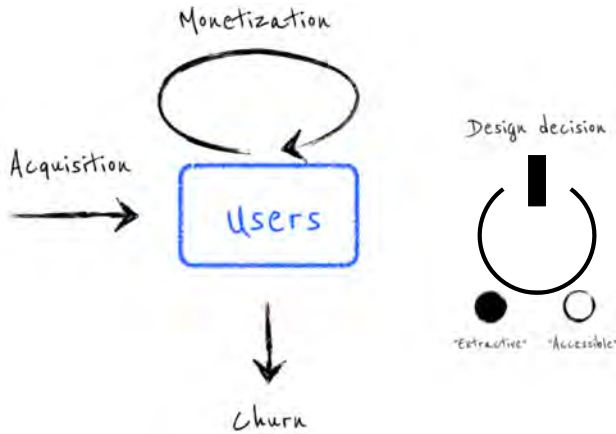


Figure 10.3: A slide used to set up a mathematical model.

mation can be used to change the size of the arrow as the knob is turned. The sizes of the arrows illustrate the tradeoffs, which become bigger or smaller as the “knob” is turned.

This image sets up the “physics” of the freemium business model and how this physics is controlled by the design decision. It is then easy to overlay this with mathematical notation, as in [Figure 10.4](#). Because the speaker has illuminated the model in the previous slide, which is still being explained by the picture on this new slide, adding the mathematical notation on top builds on the listener’s understanding. Taken together, the slide expresses a lot with relatively few words on the slide.

Of course, you don’t always need to use images to avoid clutter on a slide. Another approach is to carefully consider what is or is not essential to emphasize in your talk. If an idea is fundamental to stress, you can highlight this by having a very focused slide on that idea. Consider the slide in [Figure 10.5](#). This slide only has a title, a single formula, and two arrows labeling one variable “observable” and another variable “unobservable”. Now, if, for a mathematical modeling talk, you had a slide for *every* formula, the slide deck might be massive. But for a very important formula—say, the pivotal formula in the talk—having it on a single slide may be exactly what is needed.

I (Chris) use this slide in one of my favorite talks to give. The formula on the slide is the source of all trouble in analyzing the problem but is also the key to what makes the story of your paper interesting. I can sometimes

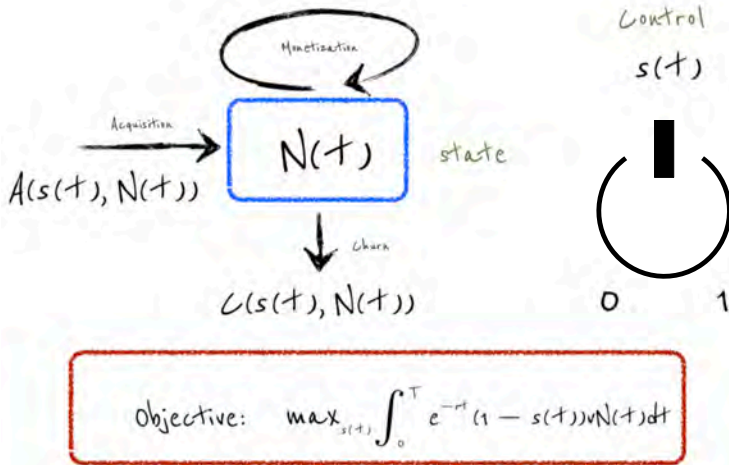


Figure 10.4: Adding math symbols

spend five minutes on this slide alone, explaining every single variable, why certain variables are observable, and why others are not. I can explain why this formula makes things tricky.

Now imagine I had buried this formula in a slide with five other formulas that are all roughly the same size. It makes those five formulas all look equally important. The audience may struggle to remember which of the five formulas I spent time emphasizing. But with a whole slide dedicated to this formula, it is hard to get distracted or forget. The audience knows this formula is important.

We could go on with many more details about constructing slides, and such an exploration would be valuable. But let's step back and try to take away a few points from this exercise. One important idea is that *you* are telling *your* story. You are not reading off the slides. You are not trying to replace yourself with the slides. The slides help you tell your story. Consider the kidney exchange example. Your role as a speaker is very clear. Here you have an image defining a concept. As an audience, we *need* you to tell us what it means.

One of the fun features of talks is that slides are not peer-reviewed. There is no standard format. This is an opportunity to tell your story how you want it told. If, given this opportunity, you produce a talk that could have been given by anyone or looks like all the other talks, then maybe you have missed

Demand Censoring

$$S = \min\{Q, I\}$$

↑
↑
 observable unobservable

Figure 10.5: A sparse slide that captures a key formula.

an opportunity.

I (Chris) want to end this section with a small rant. Of course, the whole book is a collection of opinions, but the last few paragraph or this subsection are especially so. I am not a big fan of using slides made in LaTeX. There are nice tools for doing that (including beamer), and I have used some of these tools to give presentations. Personally, I would advise against it and opt for PowerPoint or Keynote. Let me explain my reasoning.

First, LaTeX is a perfect tool for writing papers. I write all of my papers in LaTeX and always will. Even this book was written in LaTeX. Why do I use LaTeX? Because it is very good at (i) automatically labeling things like sections, figures, and equation numbers, (ii) automatically formatting text to look clean and neat, and (iii) it's super easy to write mathematical formulas. What's not to love?

Well, for one, (i)-(iii) are great for papers but *bad* for slides. Why? Regarding (i), you don't want numbered sections or formulas in talks. In a talk, people cannot refer back to previous numbers. If you want to refer to an old formula or figure, copy and paste it onto the current slide. Problem solved. There is no need for this kind of organization.

Regarding (ii), automatic formatting is great for papers because papers need to fit fixed journal templates. You can't change the design, so why bother thinking about it? Automatic formatting for talks is bad because the formatting is one of the things you can use to make your presentation stand out. "I

want my image to go *exactly* here” is an easy request in PowerPoint but difficult in LaTeX. And it’s not that it’s impossibly difficult; it’s just that it takes more effort than it should. It puts up a barrier of hassle and energy to place things where you want them. It’s easier to follow what LaTeX wants you to do.

Finally, regarding (iii), it’s easy to write formulas in LaTeX, but having *fewer* formulas is probably better in a presentation. So LaTeX makes it easy to do something you should have some discipline to avoid. In this sense, the fact it’s harder to type a formula in PowerPoint is actually a *feature*. It gives you that little incentive *not* to use a formula when an image or word would serve you better.

There are also many good reasons to use LaTeX for making slides. I am happy if people disagree with my characterizations above, so I categorized them as a personal rant. But there are a couple of reasons to use LaTeX for making slides that I feel are not as reasonable.

First, I have heard people say PowerPoint is for beginners, and LaTeX shows you are more sophisticated. I find this argument spurious because LaTeX and PowerPoint were designed for different purposes. It’s not like one is an evolution of another. Just because LaTeX is a little harder to use (in the sense you need to learn how to compile it) does not mean it’s better.

Second, they might say that using LaTeX is expected or standard in their field. It may be a *de facto* standard, but I have never seen any conference or seminar requiring presenters to prepare their slides in LaTeX. Now, if the purpose of your talk is to demonstrate that you belong in this community, and you strongly believe using LaTeX to make slides is part of that belonging, then this makes sense. I have even seen people use PowerPoint to make slides that *look like* they are made in LaTeX. But, as we have said above, because you have the freedom in talks to show yourself, choosing a format that looks like everyone else is, I believe, a missed opportunity. If you want your talk to be memorable, consider the possibility of “bucking” the standard.

Answering questions

So far, we have focused on how to prepare for giving a talk, including learning about your audience, communicating clearly, and making slides. These are all nice, but they are still only *part* of giving a talk. It misses one of the critical aspects, the speaker’s interaction with the audience. The primary form of

interaction is audience members asking questions and the speaker answering them. Effectively answering questions is essential but also quite difficult to practice. Even when you present your paper to a practice audience, you are not guaranteed to get good questions.

Answering questions is a way to communicate your understanding of your topic and the quickness of your thinking. As we discussed above, one purpose of a talk is to advertise your talents, and answering questions well is the workhorse of this purpose. It is important to note that it may be possible to be *too* quick. If you answer a question too quickly and mechanically, it can give the impression that you have already thought about this question and have a prepared answer. This can backfire if your goal is to demonstrate how you can think on your feet.

But it is essential to understand that answering questions also communicates your ability to understand your work from the perspective of others. It is quite common to get questions that are not exactly “on point”. This is entirely understandable. The audience is learning about the material in real-time, which might raise thoughts that may be only partially related to what you are saying. One strategy is to dismiss these with a response like: “No, that’s not related.” More impressive is when you can pick up on what connection their comments make. This shows you have thought about your work comprehensively and are open to making new connections. It is usually not helpful to think about your role as being responsible for correcting others’ misunderstandings. Your role is maybe more to help them make sense of your work in a way that builds excitement and possibility.

We want to share one useful analogy that relates to this issue. An amazing expression of human ingenuity is something called “improv” comedy. Here, a group of comedians improvises a performance based on contributions from the audience. One of the principles of improv is the principle “yes, and”. This principle says that no matter what someone brings to the performance, we acknowledge it (“yes”) and then add to it (“and”). For example, if the troupe is doing a sketch set in a living room and one player enters the room as an alien, the other players don’t invalidate that choice. They accept that there is now an alien in the room (“yes”), and we now need to react to that (“and”).

An academic talk, similarly, is a group performance of sorts. There is the speaker, and there is the audience. You can approach a listener’s question with a “yes, and” mentality. The question is now part of the performance, and your job is not to invalidate it or ignore it but to add to it. This builds on a sense

of unity between the speaker and the audience (more on this below).

But beyond the content of what you say in your answer to a question, there is the *way* in which you answer. I (Chris) am an avid collector of audio ticks that people use (often unconsciously) when responding to inquiries or challenges. Two that I have thought a lot about are “Yes!” and “Great question!”. Let me explain the context.

The excited and loud “Yes!” comes in response to questions from an audience member who notices some flaw or limitation in what is being presented. The “Yes!” has a hint of praise and recognition that celebrates what the listener noticed. However, the “Yes!” may have other connotations. “Yes!” can have a similar ring to when a teacher proclaims “Yes!” when a student answers a question correctly. There is almost a sense that the speaker was waiting for someone to ask, and part of the excitement is that she can now explain the answer. There may also be a hint of excitement that everything is playing into the speaker’s hands. The “Yes!” expresses excitement that the talk is coming off as impressive.

Now consider “Great question!”. Its usage shows respect and honors the effort put in by the questioner to develop a question and have the courage to ask it. But the overuse of “Great question!” has its own problematic connotations. If you say “Great question!” about every question, it loses its sincerity. Although it may even be true that every question is great, the listener who comes up with the tenth “Great question!” in a row feels a little less special about the compliment. If the “Great question!” is instant and automatic, the listener knows that the compliment has nothing to do with the content of the question but merely the fact of raising a question. An even subtler consequence of using a “Great question!” is the sense that a question is only “great” if the speaker says it is. The questioner might think, “Hey, I know it’s a great question because I came up with it. I don’t need you to validate me.” As you can appreciate, there are many ways to interpret things, and using the same response on many occasions makes the mind wonder.

When responding to a question, we do not advocate any one style of response. Using “Yes!” is great sometimes, and using “Great question!” is great sometimes. Using a variety of responses is maybe a good idea.

Reflecting on myself (Chris), I am most likely to almost unconsciously say things like “Yeah, I see” or “Hmmm, right”. Then I think briefly and conjure an honest emotion about the question. I may say something like, “You know, I have never thought about this before,” or “Wow, that’s really interesting” or

“Ah, I see, we did think about something like this before and got stuck on it.”

I am less likely to say something like “Great question!” I think the main reason for this is that, in my mouth, responses like that feel rehearsed. I often strive for spontaneity and the feeling that the talk is “hand-crafted” to the situation, not a culmination of deliberate preparation. In my mouth, anything that feels like a stock answer diminishes what I am trying to achieve. It may feel different for you.

One of my (Chris’s) favorite responses I learned while watching a talk is this sentiment: “Thanks for this question; I appreciate the opportunity to clarify a little further”. One amazing thing about this response is that it thanks the questioner. It acknowledges that the question can help the speaker to make a more compelling talk. Notice that there is no assessment of the quality of the question (“Great question!”), it is an acknowledgment that any question of any type is worthy of thanks.

But the follow-up (“I appreciate the opportunity to clarify”) is even more meaningful to me. The speaker acknowledges that the question is an opportunity for her to share more. The gratitude is sincere because the speaker recognizes that this question improves the talk. It makes the speaker appear more prepared and accomplished. This “appreciation” verbalizes, honestly and transparently, part of the meaning implicit in “Yes!” while removing the school-teacher connotation.

One thing that helped me (Runshan) with answering questions was a change in mindset. Previously I viewed talks as “defenses” similar to a dissertation defense. The main goal was to show my paper’s greatness and to answer all the questions perfectly. With this view, all questions looked like challenges, and my job was to fight back and prove I was correct. This made me afraid of questions. I panicked when someone raised their hand, and sometimes I just addressed questions in a haphazard way in the hopes that I could get away from them quickly.

After many discussions with my advisors, I realized that talks are more about communication. The purpose is to share with others what I have found and hear their thoughts. It is not so much different from when I was a child telling my friends about a playground I just discovered and seeking new ways to enjoy it. I want them to be involved and offer their ideas, which could make the playground experience more fun and exciting.

Knowing that no paper is perfect and most talks are about working papers, I became less defensive. I now view questions as interest in my research

and potential suggestions for improvement. Answering questions feels more like having a conversation. I can be true to my thoughts in the moment. If I think a question is great, I say it is great. If a question offers a perspective I have never thought about, I acknowledge that it is interesting. If a question is about clarification, I explain and note that maybe I should be clearer about this point next time I present. If someone questions my analysis, I offer my thoughts and ask for their suggestions. If I cannot address the question in the moment, then I admit that I need more time to think and cannot provide an answer now. This way, I do not worry much about being “correct,” and I start to enjoy the conversations that answering questions provided me.

Asking questions

Another skill is asking your own questions as a speaker to your audience. The good side of asking a question is that it allows the audience to participate in the presentation and become more engaged. Pausing for listeners to answer a question also gives them time to catch up with you and think about what is being presented. This can help you bring more people along.

The difficulty with asking questions is that you have to craft a question that is not too obvious and not too difficult. A question that is too obvious to the audience, ironically, no one will answer. We have noticed this a lot in our experience of teaching; if the answer to a question is too obvious, listeners may not want to point out something so obvious because it may make them look over-eager or show-offish. I (Chris) tend to handle this situation by asking the question rhetorically and then pausing before answering myself. Something like:

A natural question here is, why does this happen? (Pause) The answer is simply this: ____.

On the other hand, asking a question that is too hard or esoteric also creates distance from the audience. The chance someone can answer can be relatively low, while the likelihood that someone volunteers an answer that is incorrect or naive can be relatively high.

If you are interested in adding questions to your presentation, a good strategy is something that does not have a clear “yes/no” answer. Some possible question forms are:

- What are some ideas of what we should do here?

- Does this remind you of anything?
- Does anyone see any downsides to this approach?

If you plan to ask a question, brainstorming some possible answers is a good idea. Planning for how you will incorporate possible answers into the flow of your talk can make the question-and-answer exercise feel vital and energizing. Of course, there remains the possibility that someone comes up with an answer you did not anticipate, but if handled correctly, this also leads to a sense of excitement and possibility.

It is also a good idea to have your own answer to the question in case no one volunteers. But of course, if your question is genuine and not simply rhetorical, you need to give the audience enough time to think and speak before jumping right into your answer. The silence associated with the audience members' thinking can feel unbearable, especially if you have crammed a lot of material into your talk (which we suggest you don't do). If you don't have time to give 30 or more seconds for the audience to think, it may be better not to ask.

In our experience, asking questions is not that common. When we have seen them used, more often than not, they felt a little awkward. Use questions with caution.

Humor

Answering questions from listeners is not always the most cordial of affairs. We have seen talks devolve into small- to large-scale arguments. Sometimes these arguments have made us a little uncomfortable as audience members. These arguments have not always been between the speaker and an audience member; sometimes, arguments erupt among the audience members. A general principle we took from these experiences is that it is very difficult to convince another person that you are right and they are wrong, particularly in such a short amount of time and in such a public forum. An “agree to disagree” type stance, or even conceding the point, is often a good strategy to diffuse things.

But we wanted to mention one other strategy. Humor can go a long way to diffusing tense situations. Indeed, humor can lighten the seriousness of the whole affair, making the talk seem more fun and casual than serious and formal.

I (Chris) use humor instinctively when I am giving a talk. I don't believe that the content of my talks is life-and-death, and I treat it accordingly. I include funny images on my slides and rely heavily on self-deprecating humor to keep things light. I might say something like:

I wanted to show _____, but I settled for something far sillier.
Even that confused me for some time.

Of course, there is a downside to using humor. For one, you don't want to trivialize what you have done and make your talk seem frivolous. A talk may not be "life-and-death," but it is nonetheless consequential (at least to some extent) to the lives of those involved.

Humor at the expense of the other researchers is a major "no-no". Even if you are in a large academic field, the community remains small and quite interconnected. The chance that someone in the audience is a collaborator, student, or advisor of the person you are making fun of is very high. Plus, it's just wrong to belittle others in your field in any way, whether they hear about it or not. We believe it's best to avoid humor that puts anyone in a bad light except possibly yourself.

Also, humor may not target a specific individual but generally a group of people. Sadly, jokes have a long history of being racist, sexist, and otherwise insensitive to whole groups of people. Avoiding humor of this nature may not be as straightforward as you might think since some jokes have indirect or implicit meaning that is insensitive. Be cautious and take the time to understand what makes a particular joke funny. It is best to avoid even the hint of offensiveness.

A completely different risk with telling a joke is that it may not be funny. A failed joke is much more awkward than telling no joke at all. If you are telling a planned joke as part of your talk, be sure to try the joke out on an informal audience before your presentation. Also, much of what makes a joke funny is its delivery. Watching some stand-up comedians and observing how they deliver their material might be an idea.

On that note, I (Chris) am an avid student of stand-up comedy for learning how to give academic talks. Academic talks and stand-up comedy are surprisingly similar. First, both are solo performances (except for some rare exceptions). A single person is on a stage (of some form) in front of an audience. Second, both academic talks and stand-up comedy have the potential for audience interaction. Academic talks have questions and answers, while stand-up

comedy has heckling, roasting, and banter. One thing that makes stand-up comedy potentially even more challenging is that the use of “props” (including things like slides or notes) is viewed with some derision. The most accomplished stand-up comedians have the components of their act memorized (without slides to jog their memory) and deliver it in a way that sounds spontaneous and tailored to the audience. I am always fascinated by how stand-up comedians communicate their ideas with simple clarity and have seamless transitions between bits.⁶

Guiding the flow of conversation

It is a good sign when listeners ask questions or share thoughts during your presentation. It means they care enough about what you are saying to share something. But there is more to consider than just *how* you answer questions, but also *when* to answer them.

Consider the following example. Early in a talk, a listener asks a question about why you chose to approach the problem in this way. You have a slide that explains your choice, but that explanation is several slides later in the slide deck. You are left with a few different options on how to proceed. Each option has its advantages and drawbacks.

First, you can jump ahead to the pertinent slide and use it to answer the question. This may help in addressing the question but could also disrupt your flow. The slides were put in their current order for a reason, and “jumping around” can change how the message and emphasis unfold. It might also be that this later slide relies on some notation supplied in the skipped-over slides that make it hard to interpret.

A second option is for the speaker to try to answer the question without jumping ahead to the later slide. This has the benefit of keeping the presentation “on plan”, but it might make it harder to answer the question without the aid of a slide. Also, when the talk eventually reaches the later slide intended for the explanation, it will have less punch because some of its ground will already be covered.

A third option is to say: “I will get to this in a couple of slides and will be happy to answer you then.” This has the benefit of keeping the talk “on plan”

⁶A “bit” is a name for a part of a comedian’s act and can include jokes, funny scenarios, and humorous stories

and answering the question with a slide designed explicitly for that purpose. When I (Runshan) get to the slides that I have prepared to address a previous question, I will say, "Back to XXXX's previous question...." Sometimes, I ask the original asker, "XXXX, does this answer your previous question?" If they have additional thoughts, we can discuss more then.

As with the other options, there are also potential downsides. For one, if the answer to the question is delayed for some time, you may even forget who asked it. This is quite easy to do. The speaker has a lot to think about in the interim and can get wrapped up in getting back into the narrative of the talk. But failing to return to the original asker may fail to alert them to the fact that their question is now being answered. This can give the impression that the "I'll get back to you" is more of a way to dodge questions than effectively answer them. Overusing "I'll get back to you" may make you look rigid and defensive.

As you can see, there is much to think about and decide when attempting to answer the audience's questions, and each choice has consequences. The above example demonstrates how you must think about guiding the conversation. Maybe answering the "why did you do this?" question a little earlier makes sense. Think carefully about the ordering of your slides. Ultimately, you are the one giving the talk and should decide on a flow that tells the story you want to tell. You are responsible for what happens. Blaming listener questions for a talk that "went off the rails" will not be met with much sympathy.

Time management

The previous skill was about guiding the conversation. This is important for at least two reasons. The first, which we emphasized above, keeps the talk from going all over the place. The second is ensuring you have enough time to cover the content you plan to cover. But time management is more than just handling questions from the audience. You can mess up the timing of a talk all on your own. It is pretty easy to get stuck in explaining a slide a little more than you planned, spending extra time admiring one of your favorite results, or realizing that you overlooked something you needed to fill in.

We have noticed that running out of time is one of the biggest worries of novice presenters. You surely don't want to run out of time before getting to your takeaways, and ending early may give the impression that you don't have much to say. The latter appears to be quite unbearable because witnessing a

talk that is too short is exceedingly rare in our experience. By far more common are talks that run long. We have sat through many talks where presenters are rushing toward the end.

There are many ways you can combat going overtime in a talk. Rehearsing and timing a presentation is one good strategy. Give a talk to a group of your peers and see how long it takes you to explain everything you want. It can be helpful to identify slides that are easy to “skip” if you need to make up time. You may find ways to say things more simply and quickly.

But this is not the most important advice we have for time management. Our first advice is simple: present fewer things! There is no rule that a talk on a paper needs to include all the ideas in the paper. Pick your favorites.

There is a saying in English, “addition by subtraction,” meaning that taking some things away will yield a better result. The classic example is a sports team that has a talented yet moody player. If the player is removed, the team performs better. “Addition by subtraction” also applies to the design of talks. By removing slides from your paper covering less exciting things, you can allocate more time to describe what you enjoy.

I (Runshan) realized that my tendency to add too many things into a talk comes from my fear of being unable to fill in every minute of the allocated time. But I have found that ending the talk slightly earlier is okay (and sometimes better). Instead of thinking about what to “skip” if you run out of time, you can try to think about what to “add” if you have additional time. Sometimes I would prepare backup slides for less “important” sections and make links to them in the main slides. Depending on the situation, I may choose to jump to these backup slides from the main slides when I get there or say something like

In the interest of time, I will skip the part about XXXX, but I am happy to talk more about it at the end if I have additional time.

If I finish the talk early, I could bring it back up by saying something like

I see that we have about 10 minutes left; how about we go over XXXX now unless you have other thoughts or questions?

10.3 Qualities

Giving an academic talk can reveal much more of your character than someone reading your paper. If you struggle with impatience, the audience may sense that. If you have a courageous streak, a bold and original talk reveals this. Awareness of your strong and weak qualities takes on special significance as you prepare to give a talk.

Let's start with the six prevalent qualities highlighted in [Chapter 5](#). Our discussion of some of these qualities in [Chapter 7](#) and [Chapter 9](#) largely applies here. We highlight some additional considerations on one of the prevalent qualities: empathy.

Empathy is more visceral in talks than in papers. In writing papers, you anticipate your readers' emotions, while in talks, you face them directly. Being unaware of the audience's emotions can lead to frustration for all parties involved. For example, you may be going happily through your slides and not realize that confusion and frustration are building. This frustration may culminate in a surly question, but the audience often disengages and does not ask anything. Without empathy, you may think you have done a great job, only later to learn that people found your talk intolerable.

How to build empathy into your presentations? You can start with how you prepare. It's common to prepare a talk by thinking about what you want to say. Typical questions are, "What should I say here?" or "Should I talk about this or not?" But a more empathetic orientation is to think about what you hope the audience hears. The questions change to "What would the audience worry about here?" or "Would talking about this help/delight/or bring joy to the listener?" Focusing on what you want to say can lead to talks with too many things to explain. You may indulge in short-hand reasoning that makes sense to you but is inaccessible to your audience. Audience-focused questions build empathy by imagining what will be confusing or overwhelming.

A strategy that I (Chris) use in this direction is to structure my talk entirely around questions. While preparing, I continually ask:

- What is a natural question to ask here?
- Part of me wants to make this statement here. What question does this statement answer?

In this orientation, each slide in a presentation is an answer to at least one question. This questioning nature makes it natural to doubt and investigate. This welcomes in a potentially confused audience instead of keeping them at bay. This can make for more engaging talks.

Unity

As we discussed in [Chapter 7](#), academic talks are arenas where you see the collective expression of our field. Talks are opportunities for people to unite, meet, and build community. In a sense, every academic talk is a kind of celebration of the research community, as it performs the miracle of matching people who want to speak with people who want to listen, even about the most seemingly esoteric of subjects.

As a speaker, you are a convener of this collective space. This allows you to contribute to the field by building unity among the participants. Are you making people feel a part of something? Or are you alienating them? Do they feel welcomed by your words, or does what you say make them feel more like outsiders?

Early in my career, I (Chris) often experienced a lot of isolation at conferences. I felt like I don't belong. When I listened to a talk that I didn't understand, I felt inadequate. The times I have been closest to quitting my pursuit of an academic career were during or after academic conferences. I know this is not everyone's experience. Others are invigorated by everyone being together in one place and finding people with common interests. Somehow that has never been me. In talking to many conference participants over the years, I realize there is a broad spectrum of responses to the academic conference environment, from exhilaration to despair.

Based on how I tend to feel at academic talks, I take quite seriously how the quality of unity plays a role in my presentations. I don't want people to feel confused by me because I don't want them to feel that they don't belong. I want them to feel like I am together with them. We are sitting next to each other, trying to understand something together. Very practically, one way I express this is in how I question the audience. A common question speakers use to see if the audience is following you is to ask "Any questions?" I prefer, "Is everyone with me?" I learned this question from someone; I wish I could remember who now. I find its sentiment very powerful. In my mouth, "Any questions?" feels like I am distant from the audience. Like if they followed

me, they would not have questions. When I say, “Is everyone with me?” it expresses my goal—for us to feel that we are together. I want everyone to be with me on this journey. If I lose you, it is my mistake.

Another way to include the audience is by offering eye contact and having welcoming body language. When giving talks, it is natural to feel nervous and worry about your “performance”. It can be terrifying to even look at your audience. But if you view the talk as an opportunity to bring people together around your work, you can shift your focus from yourself to your audience. When you focus on them, you will naturally think about what makes them feel welcome and comfortable. When you are uncomfortable and nervous, it makes others uncomfortable and nervous. When you are comfortable and welcoming, it makes other people comfortable and welcoming. This is the power of unity.

Flexibility

Giving a great talk takes flexibility. It requires you to approach your material from multiple angles. The audience may be non-experts, so the most direct route may be too fast. You need to have the flexibility of mind to slow it down. But on the other hand, you have limited time to give the talk. You need the flexibility to cut out parts of the story that may interest you but may be less interesting to your audience. Your listeners may raise questions that cast your work in a light you have never thought of before. You need the flexibility of mind to see things from their perspective, even if that perspective is “not quite right”.

Rigidity is weak in the context of academic talks. If you think about your work too narrowly, it suggests that you have not thought too hard about how it can be extended or related to other areas. This can, inadvertently, reduce interest in your work. In this context, we like the analogy of water. Consider the following quote from the Daodejing (see [1]):

天下莫柔弱于水，而攻坚强者莫之能胜，以其无以易之。弱之胜强，柔之胜刚，天下莫不知，莫能行。

There is nothing in the world softer and more yielding than water. Yet, in conflict with hard, resistant things, there’s nothing better, and there is no way to alter this. What is yielding will de-

feat what resists. These facts are known to all, so why don't we use them better?

The image this quote brings to mind is a large rock in the middle of a stream. The rock does not stop the flow of the water. The water simply moves around the rock. When faced with a difficult question or a listener that insists on a flaw with your paper, it is probably best to concede the point, at least to some extent, and move on. Allow them to change your direction temporarily but not stop your flow. If you are not flexible enough to allow for this, you can get stuck in your tracks.

Enthusiasm

Giving a talk is an opportunity to celebrate your work, the field, and the help of your co-authors and collaborators. There is much to celebrate and much to be excited about. If you are not excited about your own work, you cannot expect others to be excited.

I (Chris) was very influenced by a researcher who always showed tremendous excitement when giving talks. I remember going to a talk at a conference where this researcher essentially gave a talk about *someone else's* work. He said so himself. It did not stop his unbridled enthusiasm. I did not think about it then, but later I realized how much courage he displayed in showing his excitement.

As discussed in an earlier chapter, I (Chris) love to read prefaces of books. I have found many tidbits of insights in prefaces over the years. One stands out in this context. In the preface to his classic *Lectures of Polytopes* [26], Günter Ziegler writes:

...I have made absolutely no effort to hide my enthusiasm for the mathematics presented, hoping that this will be enough of an excuse for being “informal” at times.

Why should you make an effort to hide your enthusiasm? The joy on display in *Lectures on Polytopes* is one of the reasons I do research in optimization.

Sincerity

One of the worst things you can do in a presentation is to pretend: pretend you know something you do not or pretend to be someone who you are not.

Insincerity is difficult to hide when you are standing in front of others and are the focus of their attention.

Luckily, looking into your thoughts and feelings, you can find genuineness. You can find a source of true excitement by remembering the moments of discovery and insight you had in doing your research. You can find inner calm by remembering that one of your goals is to bring delight and a sense of belonging to your audience. These are all things you can do, even if it takes effort.

I (Chris) have written more papers than I present. I like to find a few papers that I enjoy presenting and present as many times as possible. I choose the ones I most sincerely enjoy and feel represent my identity. I am proud of all of my research projects and can find some meaning in all of them, but some mean more to me than others. Maybe I did a project to support the vision of a co-author, so naturally, it may mean more to my co-author than to me. She should present the work. Or maybe I did a project chasing a fad that seemed like a good opportunity but was not an authentic expression of what I am about as a researcher. I don't like to "stretch" to pretend a project meant more to me than it actually did.

On the other hand, some papers spring more from my heart. These are things I would naturally like to share. Maybe they built on an idea that initially came to me, or I had some realization that impacted me while doing the research. Often my favorites are the ones that confused me the longest, and we found some resolution. I find it easiest to tell the story of projects with a narrative of overcoming confusion. I think this is because the journey of overcoming confusion is so personally meaningful to me.

In the early days of my Ph.D. studies, I (Runshan) once tried very hard to copy a presenting style that I thought was very cool, with a complex and eye-catching slide deck and an excited and energetic speaking tone. I enjoyed listening to talks given in that style. But it was quite different from how I normally talked, and I felt it unnatural to give a talk that way. I practiced more than forty times from beginning to end in preparation. My final performance was significantly better than when I first started practicing, and the talk even attracted some attention and positive feedback from the audience. But I still felt awkward about acting out a style that was not mine.

After the talk, one of my advisors told me it was a good talk, but it would be even better if I used a style that suited me better. At that time, I realized that hiding my personality and acting like someone else was almost impossi-



ble, no matter how much I practiced and prepared. I also realized that there was not one “best” style. I should be comfortable with the style my true self reveals. After spending a few more years attending talks, I noticed that the talks that impressed me most did not all come in the same style. In fact, the more memorable ones were those that felt most natural to the presenter. You can learn presentation skills from others, but it is usually not the best strategy to impress your audience by trying to be someone else.

Being sincere in a talk requires accepting who you are and what you have done. When you consider yourself or your work inadequate, this can increase the desire to pretend. It was an important realization that you do not have to be perfect. I will likely never be a humorous speaker who cracks funny jokes throughout a talk. My papers will always have limitations that are impossible to hide. But if I show my genuine thoughts and feelings, people will see that I have a sincere passion for my work, even though my work and I are imperfect. Since none of us are perfect, this is all we can hope to do.

10.4 Attitudes

“The audience is here to listen to me.”

While it is true that a talk reveals a lot about you, it is a mistake to think it *all* about you. People also come there to be a part of a group and to connect. By making the talk all about you, this over-emphasizes the *persuasion* role of giving a talk in the process of acceptance (see [Figure 2.1](#)). Part of the effort is to *empower*. There is a service component to giving a talk: you are helping the audience members in doing their research by explaining yours. Maybe you are clearing up their confusion or giving them an insight that may get them out



of a corner. We prefer the attitude: **“The purpose of the talk is exchange. It is about me and my audience.”**

I (Chris) would be horrendously nervous about giving a talk if I focused my attention on myself. The only way I can find some calm is in the idea that my talk can benefit my listeners. If the talk is for them, it makes me feel much calmer.

“It’s easy to make a presentation once you have the paper.”

The thinking here goes that you can just cut and paste things from the paper into the slides to make a presentation. It is important to stress that the paper and the talk are two different things, and adapting one to the other requires thought. For example, a paper might be very long, but talks are typically between 20 and 90 minutes. It is not a cut-and-paste job. It is probably not even a good idea to think of the talk as a summary of the paper. We prefer the attitude: **“Papers and talks can have different purposes, and adapting one purpose to another can be difficult work.”**

Some of my (Chris’s) favorite papers are *not* the papers I like to present. Some ideas are better communicated in papers. For example, a paper that makes a big deal of how it chases down a lot of fine details may be hard to present. I like to present papers where the main idea can be communicated simply. For these ideas, giving a talk is probably a superior way to get across the idea than reading the paper.



“The talk should spend the most time on the difficult and new parts of the research.”

Your view of the relative importance of the research aesthetics presented in [Chapter 3](#) can give rise to talks that may overemphasize certain aesthetics at the expense of others. For instance, you may believe that novelty and difficulty are the key dimensions of the beauty of your work and focus your energy there. This makes perfect sense.

But it would be wise to avoid going to extremes to establish your aesthetics. To strongly show difficulty, it is tempting to spend a lot of time on esoteric details and can easily lose your audience. It may be hard for them to distinguish difficulty from poor exposition.

Novelty, too, can be taken too far. To establish novelty, the speaker may feel they can forego the need to connect their work to what was previously known. But lacking context, research is often difficult to follow. Much of what makes research interesting is its interaction with what is already known.

I (Chris) was inspired early on in my career by a talk I saw by a well-known researcher in my area. The vast majority of the time of the talk was spent on building the context. As far as I could tell, only in the last 5 minutes did he get around to what was new to this paper. The relative amount of time spent on “novelty” was minimal, but the time invested in setting up the context made the “novelty” jump off the screen when the time was right.

In short, **a talk can highlight the aesthetic strengths of your research, whatever they are.** Difficulty and novelty are only a part of that aesthetic mix that, when overemphasized, could detract more than augment.



“My role is to explain things to the audience.”

If you are not careful when presenting, you get overly invested in your role as a knower and explainer. This can set you up to be in subtle conflict with your audience, who in academia is rightfully proud of their knowledge and abilities. This resonates with the perspective of French philosopher Jacques Ranciere who writes:

To explain something to someone is first of all to show him he cannot understand it for himself.

It is condescending to explain to someone something that they already know. This is what is happening in the phenomenon called “mansplaining.”

So how can you cover your material without “explaining it”? Effort needs to be put into presenting your findings and ideas so that the audience can observe the ideas for themselves rather than explaining to the audience what you have observed. Instead of saying things like: “Let me tell you what this means,” you can say, “Let’s look together at this figure. What do you see? (Pause) One thing that may strike you is that ...” Instead of explaining, you are offering the audience an appreciation of what they are observing. The approach is maybe best captured by another excerpt from the Daodejing (see [1]):

江海所以能为百谷王者，
以其善下之，
故能为百谷王。

是以欲上民，
必以言下之；
欲先民，必以身后之。



How do rivers and seas secure mastery
 Over hundreds of lesser streams?
 By lying lower than they do.

So to govern or teach, you must stand
 And acknowledge you are beneath the people.
 To guide them, put yourself at the rear.

One concern that someone may have about this approach is that presenting things in this way undermines the speaker's authority. After all, if the audience is guided to make meaningful observations in real-time for themselves, might they lose some appreciation for the ingenuity of your work? The good news is that many people appreciate that helping others to see things for themselves can be more impressive than telling others what you see and hoping it connects. We prefer the attitude, **"My role is to help the audience understand."**

"My work is so sophisticated, I am bound to lose someone along the way."

While this may be true, this attitude can weaken your resolve to strive for clarity and pedagogy in your presentations. A rule of thumb I (Chris) tell students is whether they think a third-year Ph.D. student in their field can follow their talk. This is a relatively high standard, all told. The audience of people who have finished two years of a Ph.D. is a vanishingly narrow slice of humanity. But it often feels like a "low standard" for many young researchers who tend to target their talk to experts. My response is that even the experts will appre-

ciate something written clearly that they do not need to struggle over. They may even applaud you for making plain what is exciting and interesting about their research area. We prefer the attitude: **“Bringing people with me can help more people appreciate more what I have done.”**

10.5 Putting it all together

As a form of summary, we provide an example of young researchers exploring the ideas in this chapter. The story is fictional but inspired by real-life interactions with many young researchers over the years. The main purpose is to provide you with an example of how natural it can be to discuss your approach to research with those you trust.

Jun was shocked. He had given a rehearsal of his job market presentation to the faculty in his department last Friday and had just met with his advisor. Going into the meeting, Jun felt he had done a great job—no one asked any questions, so he assumed everyone must have understood him. He was able to get through all of his 80 slides, something he had worried about before the talk.

But now, his advisor told him that the talk was not well-received. Other faculty members had spoken to her, raising concerns about Jun’s readiness for the job market. Jun took careful notes during the meeting with his advisor—which lasted about 30 minutes—but his thoughts were elsewhere. He wanted to get out of her office as soon as possible. It was distressing to him that he did not realize how poorly he had done.

After the meeting, Jun might have cried if he wasn’t in such a rush to talk to his cohort-mate Xiao. Jun and Xiao joined the Ph.D. program together five years ago, and they shared a lot of ups and downs to get to this point. Jun did not want to think about the comments he received without her being there. That would also give him a temporary reprieve from dwelling on his growing worries about this year’s job market.

Xiao: Jun, you seem worried. What happened?

Jun: My advisor gave me feedback on my job market rehearsal. It was not good.

X: Did she upset you?

- J: No, I think her advice was great, and she was very kind and understanding. I am just upset because I realized just how much I need to learn in such a short time.
- X: What did she say to do?
- J: So many things. A big one she emphasized was that I talked too fast. I did not notice that. Did I talk fast?
- X: Yes, I noticed that too. It seems you were really worried about finishing the slides on time.
- J: I thought we were supposed to finish all the slides. Doesn't it look bad if we leave a bunch of slides at the end and skip through them?
- X: I guess it's not great, but talking fast to finish them may be worse. It felt like you cared more about getting through the slides than whether anyone understood you.
- J: Oh, I see. But if people did not understand, why didn't they ask me questions?
- X: Do you ask questions when the speaker talks quickly, and you feel lost?
- J: No, I guess not. I would not want to interrupt the speaker.
- X: Not only that, the audience may have been so lost they did not know what to ask.
- J: You think they were that lost?
- X: Well, to be frank, I was lost, and I even knew quite a bit about your stuff before going to the talk.
- J: Wow, I didn't think that even *you* would be lost. That surprises me.
- X: You didn't see me fall asleep for a while during the talk? I apologized to you afterward.
- J: I thought you were just bored because you knew all of it already.

- X: No, quite the opposite. I find your topic interesting and always want to understand it better. I think I fell asleep because I got confused at one point and could not catch up. Once I lost your train of thought, I got distracted and fell asleep. It happens to me all of the time. I would probably fall asleep in my own talks if I was not presenting!
- J: Wow, I had no idea. I need to pay closer attention to my audience.
- X: What else did your advisor say?
- J: Let me see ...next on the list here is to cut out some of my slides. I see why now; I was going too quickly. But which slides can I cut? They all seem important now; I already did my best to cut them down.
- X: Do you think all 80 slides are necessary?
- J: Yes, I think so. I don't see how I can do it in less than 80 slides to cover all the results in the paper.
- X: Who said you need to cover everything?
- J: I thought to fully represent the paper, I should present everything, no? Doesn't that look most impressive?
- X: But, Jun, all the results are already in the paper. There is no need to put them all in the talk. For instance, that last result you showed. That one is very hard to understand; I remember you worked on it for many months. Why did you include that result? I don't think it's central to your main idea.
- J: Oh really? I thought because that one was the hardest and took the most effort, that this would be the most impressive one.
- X: You keep getting stuck on this impressive thing, Jun. Is it impressive if no one understands it? Plus, I know that the last result is quite technical and hard to develop, but that does not automatically make it interesting to others. They are probably still struggling with your first main result. If people can leave with that, it's already a lot. The talk is only for one hour after all.

- J: I see your point; I guess I am just attached to that last result. Let me think harder about what might be most exciting to the audience. This may help me in cutting some slides.
- X: Great! What else did your advisor say?
- J: Are you sure you don't need to be somewhere, Xiao? I have already taken a lot of your time.
- X: Don't be silly; I am happy to help. This discussion will also help me with my own job talk rehearsal next week.
- J: Okay, great, thanks. Next one She also said that my slides had too many words on them. I found that odd. Isn't having words on the slides good for the audience to read and follow? It makes all the definitions and notation clear, no?
- X: But people come to hear you speak, not read slides. I like to follow the speaker, and when the slides are full of words, I don't know where to pay attention: the slides or what is being said. I get super confused.
- J: Really? I would have thought you would want to look at the definitions carefully on the slides and not just listen to me. You need those careful definitions to follow the argument, don't you?
- X: No, it is up to you to help me follow the argument. We don't have time to follow precise definitions, especially when you went over them so quickly. All you can do is give us a piece of the idea to hold onto, not the whole thing.
- J: But that is not very precise. I am afraid I will mislead them if I don't state the definitions carefully.
- X: It is only one hour, Jun! No matter what you do, our understanding will be partial. Do you understand everything in detail in the talks you attend?
- J: Definitely not. I would be delighted even if I walked away with even a vague idea.
- X: Precisely. Why would your talk be any different?

J: I guess I see your point.

X: Isn't giving them a rough idea they can follow better than giving them a precise idea that goes over their head?

J: I see the problem much more clearly now.

X: What else did your advisor say?

Jun and Xiao go over a bunch of other minor details. Re-arranging slides to improve the flow, consistent font, using colors to help people remember certain concepts, etc. Jun leaves Xiao with a much better idea of how to improve his presentation.

Two weeks pass, and Jun only has one week before his first job interview talk. Still nervous, Jun asks if his fellow Ph.D. students may be willing to hear him talk. Many of them kindly agree, themselves interested in what goes into a job talk and how Jun made edits since the last time.

Jun felt much less rushed this time because he had fewer, more focused slides. Also, the audience now had more questions. Jun felt good that he could dispatch the questions quickly and efficiently; it did not slow him down very much. Jun could quickly identify where the questioners were confused and diffuse their concerns.

After going home for a brief nap (Jun was very drained after the talk), Jun returned to the office to meet Xiao to debrief what had happened.

Xiao: First of all, this was a huge improvement over the last time. Congrats!

Jun: Thanks! I thought this time was perfect.

X: It's awesome that you feel good about it, well-deserved.

J: So Xiao, tell me. I can tell you have something to share.

X: Well, during your nap, I spoke with some of the other students. Generally, they liked the talk and understood much of it but were concerned about one thing. (Xiao hesitates)

J: Go ahead and tell me.

X: Well, some of them were pretty upset about how you treated their questions. One even seemed angry because she felt you dismissed her.

- J: Wow, really? Oh ...you mean Sheila? Even you must admit, that was a stupid question.
- X: Jun, you can't think this way. Given she has only thought about these ideas in the 30 minutes before her question, having any question takes courage and is, in itself, an achievement.
- J: I guess so. But don't I need to correct people's misunderstandings of my work? Especially such an obviously wrong misunderstanding?
- X: You can clarify your stance and maybe reiterate things, but to attack the question like you did ...you seemed angry.
- J: I guess I was a little angry. Her question showed she did not understand me from the beginning. I guess I was a little frustrated at not being heard.
- X: I understand Jun. But what is more important, that they have the correct understanding of what you said or that they feel good about being in the audience of your talk?
- J: Well ...I guess the latter. Now that I have time to think about it more, I realize I had the same question as Sheila at one time, maybe a couple of months into this project. I had forgotten about that. I was lucky that my advisor supported me then; she did not react as I did to Sheila.
- X: Exactly. So you can be supportive of the people in your audience, no?
- J: Yes, I think I can. That is indeed a much better way to think about it. I want people to feel some of my joy when I did this work. You can feel some joy even if you do not understand it in detail.

Xiao and Jun went over a bunch of other small details. Jun felt comfortable with his slides now and the topics he chose to cover. He knew he needed to work on his attitude toward the audience and was quite confident he would do better the next time.

10.6 Reflection questions

The questions below are provided for you to reflect on what you have read in this chapter. If possible, it is beneficial to discuss these questions in a group of your peers.

1. Describe an experience you had giving a presentation (it need not be a research presentation, it could be a presentation in a course, for example). How did it make you feel? Did you feel confused? Did you feel empowered? What did you learn?
2. What fears do you have, if any, of giving a presentation? To what extent are these fears reflective of your concrete experiences?
3. Have you been asked a question while presenting? Describe what happened.
4. Would you rather present to an audience of experts or an audience of novices? Why?
5. : How can you tell you are losing your audience when giving a presentation? What are some ways you might be able to get them back?
6. Describe your process for preparing for a presentation. Why do you do it this way?
7. How do you approach making a presentation differently than writing a paper?
8. How do you decide what material to include or exclude in your presentations? Provide concrete examples of your choices, if possible.
9. How do you stay calm when giving a presentation? Have you ever lost your calm? If so, describe what happened.
10. In what way, if at all, do you relate to Jun and Xiao's story? What do you take away from the story?

Chapter notes

YouTube is an excellent source of tips for giving presentations, including academic presentations. [22] has guidance for speaking in the context of economics.

Chapter 11

Collaborating

Part III ends with a topic not often discussed, especially among Ph.D. students: finding and managing research collaborations. For instance, we (Chris and Runshan) learned hardly anything about the nature of collaboration during our Ph.D.s, and it took me (Chris) years to figure out how to approach the topic. By contrast, we often heard about the importance of being independent as researchers. This emphasis suggests a slight drawback of collaborating with others—if you are always doing work with other people, this sends a poor signal of independence.

On the other hand, the “bar” for acceptance at job, tenure, and promotion decisions often rely heavily on a strong signal of research productivity. At many universities, this translates into having a high enough “count” of papers published at sufficiently prestigious outlets. It can be difficult for a lone and independent researcher to produce the work needed to meet these bars. We believe that, for most researchers, finding research collaborations that demonstrate the strengths and vision of each collaborator is essential for success in a research career.

We believe the lack of systematic training in effective collaboration hinders academic research success. This is especially true for those young researchers with little access to established academic relationships, such as networks of former students of influential advisors or an affiliation with a research institution that can open doors. For many of us, we must build our networks roughly from scratch, possibly with a bit of help here and there.

But more than hindering academic research success, we believe insufficient training in collaboration contributes to a sense of isolation. Like every-

thing we discuss, finding collaborators is no guarantee of academic success. But it certainly helps to avoid the feeling that you are tackling a difficult task alone.

All of the “Putting it all together” sections in the previous ten chapters have illustrated aspects of informal research collaborations. We continue these deliberations here.

11.1 Purpose

Why should you seek collaborators? We hope that you don’t view seeking out collaborators as some admission of defeat. The narrative that if you were good enough at research, you wouldn’t need anyone else is the wrong narrative. Even if you were amazingly talented in every way, seeking collaborators would make sense. Human beings are social animals. It is probably harder to explain why you *wouldn’t* seek collaborators in your work. But not everyone seeks collaborators for the same reasons. We explore a few possibilities.

Multiply your efforts

Getting a paper published is difficult work; it is natural to want to share the load with others to ease some of the burden. If you could make a clone of yourself, you might be able to work twice as fast or on two papers instead of one. Since cloning yourself is impossible, one strategy is to find a research collaborator similar to you (similar interests, training, and goals) to approximately “duplicate” yourself. If you are lucky, you may be able to find someone that fits this criterion.

However, much more likely is that you will find others who have *different* strengths, interests, or goals than you. There are, of course, still grounds to multiply your efforts by collaborating with someone different than yourself. This relies on the classical economic concept of “division of labor”, eloquently described by Adam Smith in his classic *The Wealth of Nations* [20]. In this concept, workers specialize in certain aspects of a job and coordinate with each other to boost productivity.

As we have detailed in this book, there are many steps in research. Someone needs to read papers, someone needs to listen to talks, someone needs to come up with ideas, someone needs to write down progress, and someone

needs to present findings to others. Of course, each of these steps has many sub-steps. The good news is that some of these tasks give people energy while others drain people of energy. Why not divide up the work following a natural division of labor? If you don't like reading papers, find someone who does. If you tend to avoid getting down to write, find someone eager to dive in. The ideal collaboration is one where everyone is doing that part of the job that gives them energy—sembling such a team multiplies efforts, energy, and, ultimately, results.

To learn from others

Since research always pushes for novelty, you can never fully rest on your laurels. I (Chris) have often lamented this point, wishing I could “stick with what I know” and still publish papers. Of course, that's impossible in the long run. Writing only what you know quickly dries up the novelty of your work.

But continuously exposing yourself to things you don't know is exhausting and potentially dangerous. I (Chris) transitioned after my Ph.D. from an area I focused on during my dissertation to something related but, ultimately, quite different. It took me three years to get my next paper published. This was much too long, given the pressures of my academic position at the time.

Looking back, I think it was a good idea to learn something new and make a transition. My mistake was learning this area from scratch without the help of an established expert. A more effective approach might have been connecting with someone with expertise in the new area and learning “on the job” with them while writing papers. Instead, I spent several years reading papers and books that were enlightening but far from the cutting edge that could appear anywhere as a published paper.

Nowadays, I view collaboration as an opportunity to learn something new from other people. I have learned so much from my collaborators that I know I would never have been able to figure it out on my own. It is such a benefit to have a person ask “Why?” rather than a book, paper, or computer screen to interrogate. You get personalized answers that help the inquirer while emboldening the explainer. Indeed, you can put so much time into esoteric things as a researcher that having someone ask you how your ideas work is itself a form of encouragement.

Companionship

As we discussed at the outset of this chapter, research is a difficult and uncertain journey. It may not be a good idea to tackle it alone. No matter the skill of a mountain climber, a solo journey up Mount Everest is ill-advised.

One reason to have someone along the journey with you is pretty basic: companionship. Human beings are social creatures. Too much time alone drives them crazy. Solitary confinement is one of the most severe punishments you can inflict on a human being. Prison inmates put in solitary confinement often prefer death or injury to remaining alone, with extremely high rates of self-harm and suicide [12].

We are not suggesting research is anything like solitary confinement, but there are times when it may feel that way. Being in a collegial environment where others are working around you on their projects can provide enough companionship for many people. They don't need to work on the same project to feel connected. But just like how many of us become more socially isolated as we go from high school to undergrad to grad school, social isolation often increases as we go from grad school to postdocs or junior faculty. I (Chris) used to lament being crammed in a room full of cubicles with a bunch of other Ph.D. students but later came to reminisce about that time because at least we were together. As a junior faculty member, I was often alone in my office.

Research collaborations are one way to stay connected with people. As you lose those environments of being surrounded by engaged people that you can have a feeling of joint adventure with, you need to ensure that some connections remain. Having a research project together is one concrete way to achieve this.

I (Chris) have started projects with people whose primary motivation was that it would allow me to stay connected with them. I liked them as people and wanted a reason to keep conversing with them and grow together. But now that I think about it more systematically, this seems as legitimate a reason as any to start a collaboration.

One simple reason for having companions is to share the ups and downs that are often associated with the research journey. If your paper gets rejected, you have someone to complain with. If your paper gets accepted, you have people to celebrate with. The following saying captures it best¹

¹We could not find a definitive origin for this saying, although there are some hints it

Happiness doubles, and sadness halves when you share them with another person.

Another source of motivation

As we discussed in **Chapter 4**, researchers are motivated to do research for different reasons. One motivation for a research career is to have a stable job that supports you and your family. We find this as meaningful a motivation for working hard in one's profession as any. But adding collaborators to your research projects adds a dimension of motivation to the work. If your paper succeeds, you are supporting not only your career and family but also your collaborators' careers and families. When you publish a paper, whether it is "useful" to society at large or makes a meaningful contribution to human knowledge or not, at least you know that it has helped your collaborators.

Reflecting on my (Chris's) research, I think the feeling I get from publishing a paper that helps a younger researcher in their career is probably the strongest sense of joy I get doing research. It is the sweetest of all payoffs. Achieving this feeling strongly motivates me to keep plugging away at research even when I am tired or disillusioned. But it goes beyond helping others achieve success. I am very heartened by the sense of justice I feel when I help someone I believe has great talent earn some of the recognition they deserve.

One of my fondest memories of high school was when I worked with another student to study for a big province-wide chemistry exam. In those days, I used to have fellow classmates over to my house to study. Being the keener I am, I made my own exams, graded them, and helped them see where they were making mistakes. I could tell right away that this student had a lot of talent but had not practiced his skills for some time. After a few sessions, he was schooling me on how to approach the problems. Unsurprisingly, he eventually got a higher grade than me on the province-wide exam. I consider that one of my greatest achievements in high school. I could help someone with great talent achieve something that showed that talent to the world. I feel much the same when it comes to research collaborations.

Collaboration gives me (Runshan) a greater sense of responsibility. As much as I enjoy the research process, there are countless times when I encounter difficulties and setbacks that I do not want to face. I would be more

might be Korean.

likely to avoid facing those challenges if I were alone. This dramatically slows my progress. But when collaborating with others, my effort is not just for myself but something meaningful for a team. The desire to take responsibility and be reliable to others has motivated me to work on many things that I did not necessarily enjoy. These experiences have also helped me to grow. I don't mind doing some of the things I used not to enjoy. I grew in the context of relationships with others.

11.2 Skills

Finding collaborators

The first, and maybe most important, skill in the capability of collaborating is to find good collaborators. In our experience, this takes work. Few of us are fortunate enough to have great collaborators approach us with ideas and projects to work on. You often need to go out there to make connections and propose collaborations yourself.

What should you look for in a collaborator? There are various dimensions that you can “match” when approaching people. These include things like

- common research interests,
- compatible personalities, and
- complementary skills.

A collaboration based on shared research interests is maybe easiest to understand. Two or more people are interested in the same research question and should join forces rather than compete to carve out some novelty. Also, this type of collaboration takes the least investigation to find grounds for. Unlike finding compatible personalities, skills, or needs, people's research interests are relatively public and easy to learn. People post research interests on their websites, and clues can be found in their history of publications and working papers.

However, starting a collaboration based on shared research interests alone can be risky. Just because someone is interested in similar research topics does not mean you will get along. The complexity of the human spirit is such that

even if two people share an interest in a very narrow and esoteric topic, they could be completely incompatible in other ways.

The difficulty with finding matches based on personalities and skills is that you have to do some investigation beyond perusing some websites.

Learning about people's personalities takes some level of intimacy. People's "public" personas are much more uniform than their "working" personas. In public, people are more likely to appear excited about research, interested in new ideas, open-minded, patient, and eager to connect. Outside the public eye, other sides of their personalities may come to the fore, including pessimism about research, controlling or dismissive attitudes, and aloofness. If you work nearby someone over a period of time, say as office mates during your Ph.D., you can learn a lot by observing. But what about people you don't spend this amount of time observing?

Getting "to the bottom" of someone's personality in a short period of time (say, at a conference) requires interacting with them about something with some degree of consequence. A basic example could be asking them their opinion on a paper or a talk you just sat through together. This gives you an idea of how critical or forgiving they are of other people's work. You may also ask them to share an experience of dealing with a difficult situation to get a sense of how they deal with adversity. Even better, you can see how they handle adversity when they give a talk. Another approach is to see how they handle organizing a meal or outing among conference participants. Do they go with other people's plans or have their own ideas? Do they insist on their ideas?

It is important to note that two people's personalities need not be the same to forge a good collaboration. For example, it can be good for a team to consist of pessimists and optimists. Pessimists may be more likely to see flaws in the current work, while optimists provide the energy needed to finish a project. Another example is when some team members are outgoing and expressive while others are more thoughtful and shy. As long as they can communicate with each other (more on this below), having various personalities typically enriches a project.

It can even be more challenging to learn about people's skills. One reason is quite simple, in many academic circles, the mythology is that everyone is talented at and likes all steps in academic research. Who wouldn't want to be the person that comes up with great ideas, finds ways to bring them to life, writes elegant papers, and gives entertaining talks? But by pretending to be

talented at all of these, you may turn away potential collaborators. After all, if you are good at everything, why would you need anyone else's help?

To learn about people's true strengths and sources of energy, you have to reach a point of honesty with them. Maybe you can start with an honest admission that perhaps you like to do certain parts of the research process more than others. This gives the other person the opportunity to give a similar admission. Through this kind of sharing, you can find opportunities for synergy.

Dividing up the work

Effective collaborations find ways to divide up the work in a way that draws on everyone's strengths. This may require you to think about the research in a modular way and develop a sense that some collaborators will do more (or less) at different stages of a research project. We have already detailed several of the component steps in doing research, but let's review a few here:

- idea generation,
- literature search,
- coming up with research questions,
- analysis,
- summarizing and clarifying findings,
- writing,
- presenting, and
- revising.

Of course, some of these components are best done together. I (Chris) like to generate ideas and develop research questions in conversation with others. Left to myself, I am unsure how many I would come up with. Something about the social situation of a group of people thinking together gets my mind going.

On the other hand, some activities are easier for some people to tackle independently. Maybe they need quiet to think. Maybe hearing too many

different voices crowds out their own. I (Chris) often like to tackle writing by myself. It takes me a while to find the “voice” I want in the writing, and when I am contending with competing voices from other authors, I tend to get overwhelmed. I would rather complete my version of what I want to write and then see how others react to it. Too much back and forth makes me lose a sense of progress which is important to keep my energy flowing in writing.

In our experience, one thing that is critical to successful collaborations is that the members of the team value each other’s talents and do not resent what others are good at. For many years, I (Chris) felt sensitive about the fact I was relatively weak compared to many of my co-authors in analyzing and solving problems. I could develop ideas for research, summarize, and clarify our findings, but I often struggled to marshal the massive stores of energy it takes me to tackle hard problems. When my collaborator displayed a lot of talent in problem-solving, I would feel jealous or resentful. This would sometimes even slow down projects because I wouldn’t let my collaborators “run” at the speed they needed to solve the problems in a comfortable way. I would ask too many doubting and pessimistic questions that took away their momentum.

Over time, I learned instead to be a champion of my collaborators’ talents. If they were running, I would try to keep up and admire what they were doing. I would always be careful not to slow them down but instead, show them how excited I was to witness their work. I got to be comfortable with letting the weight of a project lie heavily on their shoulders when they were the right people to move things forward. I knew my time would come later.

Another insight is that it is not necessary to break things down according to people’s talents but according to what gives them energy. I (Chris) have worked with people who honestly have no weaknesses. They come up with insightful research ideas, produce wonderful theorems, write beautifully, and give great presentations. Even in this case, there is scope to divide up the work. Just because someone writes well does not mean they have much energy for it. They can do it, but it exhausts them. It would be better for them to focus on what they enjoy. I have also known people who greatly enjoy reading the literature and coming up with ideas, even though it is hard work for them to generate ideas that eventually turn into great papers. They know it is hard for them, but they enjoy doing it anyway. Such a spirit is highly valuable on a research team. We can all admit that the work is hard, but we can enjoy it regardless.

When dividing up the work, maybe the most important thing is finding everyone a role they have energy for and feel valued doing.

Communicating

At the start of a collaboration, everyone can draw on their strengths and work on their part of the project. All are happy. But then things can change. This happens quite often in research. Maybe the project started out in what appeared to be a promising direction, but then the direction had to change to something potentially less promising. A project can morph into something completely different than how it started. How to keep the team united and excited? A key skill here is communication.

We can think of three major forms of communication in a research collaboration:

- meetings
- correspondence (email, slides, and reports), and
- instant messages.

While meetings and correspondence are standard tools for communication, we wanted to add instant messaging (using tools like iMessage, WhatsApp, Slack, or WeChat) as a technology we find ourselves using more often in recent years that we believe has a distinct flavor.

By meetings, we refer to gatherings using online conferencing tools like Zoom or in-person meetings with subsets (or all) of the collaborating team members. We believe meetings to be critical to any collaborative project, as it allows everyone to chat, share challenges and victories, and think together. Meetings are often the social glue of the collaborative process. Seeing each other's faces and hearing each other's voices makes things real and personal. It brings shared emotion and shared fate into the work.

We have worked with many collaborative teams who run their meetings differently. We can classify the meetings we have experienced into three rough types:

- thinking meetings,
- reporting meetings, and

- deciding meetings

Thinking meetings allow everyone to sit and think together. The feeling here is that making progress on the work is so difficult that we sometimes need to “put out heads together” to get anywhere. This type of meeting can be very long (lasting several hours) with periods of time where everyone is silent in thought. We like thinking about meetings because they get everyone on the same page. Everyone learns not only what works but also what doesn’t work. Times in between thinking meetings can be used to write up ideas, pose new approaches, and summarize what was learned in the meeting.

Reporting meetings have a different flavor. Here, the idea is that collaborators think on their own time and come together to report what they have found. These meetings take on the style of one or more collaborators explaining what they have done while others raise questions, express doubts, and make suggestions. These meetings can sometimes be very brief. If no one has anything to “report”, they can even be skipped. This style of meeting is common in many advisor-student relationships where a student is meant to be working on their own ideas and asking the advisor for advice. The advisor asks questions and assesses whether the student has done a good enough job on what they are doing to possibly go onto another question or start writing things up in a paper.

The contrast between reporting meetings and thinking meetings is quite stark. It does not necessarily make sense to “skip” thinking meetings because they are designed for making progress *together*. All that is needed for a thinking meeting to run is something to think about. There is an implicit assumption that the attendees of a thinking meeting cannot “waste” each other’s time. It is a collective activity. By contrast, reporting meetings are often more characterized by an imbalanced power dynamic, where the reporter needs to justify the meeting in order to not “waste the time” of those they are reporting to.

A final type of meeting is what we call a *decision meeting*. In this meeting, the collaborators must decide where to take the project next. This can include deciding whether the paper is ready to submit, whether a revision is complete, where to submit a paper, whether to present the paper at a conference or not, etc. Of course, the decisions can be more granular than this, such as whether to include a particular figure in a paper that is being written up or not. It can be very useful to meet in person to tackle decisions because it is good to have

consensus or agreement in a research project and not to waste time working on things others are not on board with.

In practice, meetings can take on some combination of thinking, reporting, and making decisions. What is important is that the purpose of the meeting is clear. No one wants to waste time meeting with no purpose. It is also important that the results of the meeting are recorded in some way. This can often be done by adding comments or notes to a written document (like a draft paper, slides, or a manuscript). It can be frustrating and discouraging to meet about the same issue again and again and feel like you are not progressing. Recording what happened in the meeting helps ensure the same ground is not covered too many times and builds a sense of forward momentum.

Between meetings, collaborators can communicate through correspondence. The most common example in this era is email. Email is used to send links to papers that are found, to alert collaborators to changes in a draft of a paper, or give some follow-up ideas that come out of a meeting. Of course, email is also a way to coordinate meeting times and make small decisions.

When I am very engaged in a project, I (Chris) tend to send many emails to my collaborators. Part of the reason for this is that I tend to be quite overwhelmed during thinking meetings. It takes me some time to come up with my own ideas, and I prefer having time to sit and carefully sort out what I want to say. I also like to use emails as a way to take note of some of the things I am learning. I have found that people tend to read their email (especially if it is short) more than read a draft paper or set of notes. The bite-sized nature of emails can get more of a conversation going in between meetings.

I find it very gratifying to get emails from my co-authors showing some thinking or progress on our work. I love the idea that people are thinking about our collective work even when we are apart. For this reason, I try sending emails with thoughts, even simple ones, between meetings. I am hopeful that it is encouraging to my collaborators. This type of communication buoys the hope that we are getting somewhere in our research.

This last idea leads us to instant messaging as a research collaboration tool. This is even more informal than writing an email since it is designed to be read throughout the day, even when away from a computer. I (Chris) tend to use these messages to update my co-authors on my current feelings towards the project and possibly what small doubts or issues I am pondering at the time. It is also a good opportunity to celebrate victories. I might write things like:

- “Excited about our progress in the last meeting. I have a sense this new direction will work.”
- “Reading the paper that you sent. It’s making sense so far.”
- “You said something in the last meeting I want to follow up on. Can you explain again how you got to this?”
- “Congrats on presenting our paper at the conference! I heard from others that they enjoyed your talk.”
- “I am happy we can work on this project together.”

The last message (“I am happy we can work on this project together”) may seem to serve very little function in making progress, but I disagree. When you do research, there are times when you can lose motivation. Maybe the project’s message is crumbling, or a paper got rejected. Knowing your collaborators are happy working on this project with you can give you the energy to keep fighting.

We end this section with a few more practical thoughts on executing the different tools of communication. Regular meetings help me (Runshan) keep a predictable schedule. With regular meetings with my collaborators, I keep a steady flow of work regardless of the state of the project or my mood. It also saves the time and energy needed to coordinate meeting times. As an introvert, I enjoy working on my own most of the time, and I do not feel the urge to get connected with my collaborators all of the time.

I used to treat all the meetings as reporting meetings, and I strove to show only well-thought ideas and complete results. I would also send very long emails with sections and highlights to my collaborators once in a while. But I realized the importance of more frequent and casual conversations over time.

First, long emails put pressure on the receivers to respond with some also elaborate. This, I think, made my emails less likely to be read. Second, preparing for meetings and emails took me a long time. I would be so focused on these “reports” that I would not communicate with my collaborators during the interim. My collaborators did not know whether I was working on the project or not. This increased the feeling of isolation for all of us.

Finally, it was common that a long email took the wrong direction in the first step. For this reason, I could waste a lot of effort writing something I

can not use. Had I delivered my progress in more “bite-size” chunks, I would have been able to save a lot of wasted energy. I learned to touch base with my collaborators more frequently and became comfortable sharing my half-baked ideas and unsuccessful results.

Despite being an introvert, I enjoy meeting and discussing with my collaborators even when I don’t have much that is “impressive” to report. I learn much faster from my collaborators and build stronger connections with them through frequent communication. For the most part, I have ditched the long and complicated emails.

Accountability

Good communication can help maintain collaborations, but ultimately, successful collaborations lead to concrete output. This means writing, submitting, and (hopefully) publishing papers. A good team takes these goals seriously and makes each other accountable to deadlines and milestones.

It can be helpful to set concrete deadlines for when the team would like to complete a project. Of course, it isn’t easy to set firm deadlines for creative work like academic research. I (Chris) have never been able to predict how long it will take to finish things. I am usually optimistic, thinking we can finish sooner than we actually do.

Despite the difficulty in timing research work, it is nonetheless a good idea to set targets and strive to meet them. Some academic fields have many natural built-in deadlines (like computer science with its emphasis on conferences), but in other fields, the deadlines are a little more amorphous. You can submit a paper to a journal at any time, so it always feels like there is more time to prepare things.

A popular strategy in exercise and weight-loss programs is to have an “accountability partner”. These are friends who help each other stick to a program or regimen. Not following the program would not only let you down but your partner as well. This added motivation gives you more energy to persist. We have found roughly the same notion helpful in research. Everyone benefits from working hard on research, but it is not easy to stay dedicated to working as hard as we should. The commitment we make to each other, and the fact we are accountable to each other to make progress, helps the work progress.

Compromise

One challenging thing about research is that it basically never goes according to plan. You and your collaborators may have started with a grand vision for your project, but that vision needs to be adjusted over time. It may be that different collaborators were excited about the project for different reasons, and as adjustments are made, some of those reasons may need to be sacrificed. Sacrificing some of your dreams to save the dreams of others is one of the challenges of working together in groups. It can be the case that you end up with a final product unlike anything you would produce on your own, but you go with the will of the group.

It is a skill to think about what's best for a project and the people in it instead of what is best for yourself. This is not to say that you should easily give up on your vision for a project if you encounter difficulties. You can often find solutions that work for all parties with additional thought. But this is not always the case.

I (Chris) have worked on many projects where the overall vision shifted with time. In these instances, I try to remember that my loyalty is more to the people on my team than any particular thing we produce. Projects end, but relationships can live on. If this project did not turn out as we wanted, maybe the next one will. We will only know if we keep on working together. I truly believe everyone will have their chance to shine if the group is united and dedicated to each other.

Turning down collaborations

The previous few skills we have discussed focused on maintaining solid collaborations. This is, of course, a good idea if the collaborations are fruitful and supportive. Unfortunately, this is not always the case. Sometimes you can get involved with toxic collaborations that drain you of your energy and joy for research. It would probably be best not even to call these arrangements “collaborations”, as they often involve one party taking advantage of another.

One way to avoid these toxic collaborations is never to start them. We already talked about finding good collaborators, but we should also comment on identifying bad collaborators. We can speak to a few experiences we have had that illustrate some potential pitfalls.

It can be wise to avoid research collaboration propositions that involve a

very stark *quid quo pro*. For example, one party might say for an empirical paper: “I will give you the data if you do the analysis and write things up.” These arrangements tend to underplay how risky and uncertain research can be. What each collaborator might need from the others can change unpredictably as the project evolves. You would be wary of a transactional approach. Some longer-term commitment of the collaborators to each other, rather than just a single project, is likely to be more successful in our experience.

Another worrisome sign is if a potential collaborator asks for some “exclusivity”. A version of this is when a potential collaborator promises to work with you only if you can dedicate much or most of your research effort to your shared project. We find this problematic for a couple of reasons. First, research can be very risky. Putting “all of your eggs in one basket” with another collaborator could expose you to many risks if the collaboration does not work out. Diversification in research is almost always a good idea. A second reason to worry is that it may create a power imbalance. Suppose the potential collaborator does not commit to dedicating most of their time to your shared project. In that case, there is an expectation that you will be dedicating *more* effort than them. Depending on the circumstances, this allocation of effort could be unjust.

An important skill is knowing how to turn down collaborations that will not benefit you. This is easier said than done. In the beginning, many projects look enticing and promising. Having the discipline to turn down something that could be beneficial requires you to have confidence in yourself and your current group of collaborators. For young researchers, this confidence is sometimes hard to come by. Unfortunately, people looking for young researchers to share a significant burden on a project know about this confidence gap. It might be an idea to consult with someone you trust before starting any collaboration. They may be able to give you more perspective on how fruitful and healthy a potential collaboration may prove to be.

Ending a collaboration

You can start a collaboration with the best intentions, using all of the information you had at the time to make what you thought was a wise choice. But not everything in life works out. How to get out of a collaboration that is no longer working for you? This can be easy or tricky, depending on the circumstances.

For collaborators with a strong relationship who have kept up communication, it can be as straightforward as saying that you are just not into the project anymore and want to concentrate elsewhere. Depending on where the project is, this may mean removing your name from the paper or possibly keeping it there but in a more diminished position (say at the end of the author list). I (Chris) have experienced a few scenarios where I asked to leave a project, or someone asked to leave a project. In retrospect, parting ways was a good decision in each case. It can be a real drain on a project if someone no longer wants to be involved and is only going through the motions.

Of course, the harder case is when there is poor communication among the collaborators and a lot of pressure or expectation is placed on you. Leaving a project where the “next step” is in your hand can be especially difficult. It can look like you are dodging responsibility or abandoning others in their time of need.

Before considering ending a collaboration, you may ponder why you want to “get out”. If it’s because you are facing a roadblock in the research, it’s probably better to discuss this with your collaborators and try to approach things differently or change the research direction. If it’s because of a lack of trust and reciprocity in the relationship, then it is unfair to characterize your departure as dodging responsibility or abandoning anyone. In this case, the problem is not wholly your responsibility; the collaboration is stalling because of interpersonal dynamics.

Having said all this, ending a collaboration is one of the more challenging social situations you may face as a working researcher. But fear of this outcome is not a good reason to avoid collaboration. We strongly believe that the upside of finding a good collaboration far outweighs the downside of having a collaboration or two turn sour.

11.3 Qualities

The qualities most pertinent to collaborating are those related to interpersonal interactions. This makes sense, of course, and is a point of departure from earlier capabilities that drew more on the qualities of individuals and how they approach their work. Let’s examine a few qualities that stand out for solid collaboration.

Respect

When you collaborate, often the *hope* is to find researchers who have different talents and dispositions than you. This is how your abilities can be complemented and multiplied. On the other hand, this requires you to have respect for others who are potentially quite different than yourself. It is especially important not to overlook or devalue people's contributions when they differ from yours.

I (Chris) enjoy working with people who are strong at solving problems. But this was not always the case. As I described earlier in this chapter, I used to feel jealousy towards those who were strong in problem-solving skills. I would try to convince myself I didn't need people with this talent; I just needed to make more effort myself. When inevitably my problem-solving skills proved lacking, I would try to intellectually downplay the importance of problem-solving in research, avoiding the issue. These were signs of immaturity and lack of respect for others.

It took me several years to fully accept my admiration for skilled problem solvers. This required respecting their craft and respecting their gifts. This allowed me to approach adept problem solvers humbly and make more genuine connections with them. This proved to be a significant change in my research trajectory once I formed some meaningful collaborations with strong problem solvers. Nowadays, I count a few of them as my closest friends in academia.

Respect is very much needed when you and your collaborators disagree. There were times when my (Runshan's) collaborators pointed out a problem in our work, and I allowed my arrogance to take over. I refused to listen, thinking I should know more than they did. Very often, I was wrong. Even in the cases where I was right, a lack of respect only worsened the conflict. As I learned to be more respectful, I found it easier to focus on the problem at hand rather than my ego. This helped us all make better progress, and I learned more from my collaborators.

Open-mindedness

Collaboration is more effective when the collaborators are eager to learn and understand each other's opinions. A good collaboration is not necessarily one where all the parties agree on everything; this can lead to bland output. Work

can be more exciting when an evolution of thought arises from collaborators challenging each other's ideas.

Of course, such a challenging environment has the potential to lead to fights and entrenched disagreements. Even the possibility of conflict (if not actual conflict) can silence the voice of some collaborators. This is where open-mindedness proves to be very helpful.

I (Chris) have worked with researchers whose research personality differs from their non-research personality. In a research context, they will entertain any idea and are always open to the possibility that they are mistaken in their thinking. Outside of research, they can be much more insistent. This open-mindedness in research arose out of practicality. Stubbornness in research typically yields delayed projects and less productivity.

Reliability

When you collaborate, not everything goes at your ideal pace or in your preferred way. But these are things that you cannot easily change unilaterally. All you can do is hold up your end of the bargain and contribute what you can to the group's success. It is essential that you can reliably implement your part of the agreed-upon vision of the group. One of the major sources of delays in joint work is when collaborators do not execute what they agreed to do in a timely manner.

When working on research projects alone, you only let yourself down if you don't do something when you say you would. This has relatively few consequences other than hindering the progress of your project. But when you work in groups, letting others down can create a cascade. You maybe don't do something that your collaborator needs for them to do their part of the project. This can give rise to frustration.

I (Chris) consider this degree of reliability needed for group work to be a feature rather than a drawback. I find the feeling of letting others down quite uncomfortable. As for letting myself down, that is much less bothersome to me. The fear of letting others down gives me a lot of energy to finish my work. It is one of my secret weapons to push projects through, especially when they are near completion.

Gratitude

Respecting your collaborators is one thing, but being grateful for what they do is another. Even if someone only does what they said they would do, this still deserves gratitude. Even if someone does something you could have done yourself (you just didn't have time), this still deserves gratitude. After all, something happened even if you didn't have the time to make it happen.

In Vancouver, where I (Chris) currently live, it is common for people to say "thanks" to the bus driver when they get off a bus. They are grateful that the bus driver ...drove the bus. A more pessimistic view would say that the bus driver is simply doing his job, not any particular kind of favor for you. The driver would have moved from A to B whether you were on the bus or not. So why say thanks? We prefer to be grateful than pessimistic. Gratitude springs naturally when someone does anything for us, whether they "had to" or not.

We (Chris and Runshan) are very grateful for all that our collaborators have done for us. We wouldn't have an academic career if it were not for them. But even with this gratitude in our hearts, there have been times when we think we have taken people for granted. They so routinely help us out of tricky spots in our research that we came to expect it, and maybe we were not as good at showing gratitude as we wanted to be. Having gratitude takes discipline because we want to recognize and acknowledge even small, regular kindnesses.

Unity

The quality of spirit that brings people closer is particularly relevant in collaborations. Unity brings research from a collection of "I"s to a consolidated "we". Unity can turn a one-off joint paper into a career of shared research.

Of course, collaboration is about getting work done. You are together to do research, write papers, and advance your careers. But the interaction can (and typically does) mean more than that to the participants. Your collaborations give you a place to belong.

I (Chris) have worked with groups of collaborators who start research meetings with 20-30 minutes of just catching up. They share what they have been doing, how their families are, thoughts about current events, and even life philosophies. We know we will get to the research eventually; we are not worried about that. Projects will come and go, but this time together is about us. These bonds will weather rejections and a published paper here and there.

Research is what this group does, not who we are.

I (Chris) try as much as I can to be loyal to my collaborators. I want the collaborations to last a long time, hopefully, our whole careers. Maybe we aren't always working on a paper, but perhaps we could. Of course, it does not always work out this way, but it is an ideal that I strive for and sometimes (at least so far) have been able to achieve.

When discussing my research approach, people sometimes ask me whether I am “application-driven”, “theory-driven”, or “data-driven”? I like to tell them I am “relationship-driven” (a concept we first discussed in a different context in [Chapter 4](#)). This means I could find myself doing any research project in any way as long as it's with someone I have a good relationship with. If one of my trusted collaborators had an idea to do something very new and discipline-bending, I would be game. I would follow them pretty much wherever they want to go.

Trust

This leads to a pivotal quality that can easily make or break a collaboration: trust. Collaborations are doomed without trust in each other's skills, work habits, ideas, and worries. With trust, almost anything is possible.

Of course, trust is one of the most difficult qualities to practice. A couple of English sayings underscore its delicate nature: “Trust is earned” and “Trust can be lost in seconds, but take a lifetime to regain”. We (Chris and Runshan) consider these to be a little overdramatic. While trust is earned, not everyone is naturally mistrusting. Quite a few researchers I have worked with give new collaborators “the benefit of the doubt” and are eager to see them get involved.

A breach of trust can surely harm a collaboration, but life goes on. For many of us, research is, after all, just our job. If things don't work on a research project or two, we move and do other projects. Trust can rebuild in the context of “getting things done”. We are building papers together; it's not life and death.

Trust is something that you need to offer in order for this quality to be strengthened. If you never show any trust that a collaborator can contribute to the project, then they will never have the chance to show you that they can. Practicing trust means giving your collaborators chances to do great things and expecting to be surprised. This may require giving up a little control over where a research project is going. By giving up control, you can sometimes



find yourself in an unexpected (and hopefully better) place than you started.

One way that we (Chris and Runshan) approach the quality of trust is to try to cherish the possibility that our collaborators will teach us something new or change our perspective. We find this grows the trust we have for them. Indeed, learning from others requires you to “walk with them” for a while. If what they have to teach you is very different from what you currently know, we may have to walk with a “blindfold”. This journey is impossible without trust.

11.4 Attitudes

“The real contribution in research is X; everything else supports that.”

Depending on your field, researchers may emphasize certain research tasks as more important than others. The emphasis could be on generating solid research ideas, securing novel data sets, or proving hard theorems. The view goes that the other, less important, research tasks simply “support” the important tasks.

This emphasis on the primacy of certain research tasks can carry over to create a subtle hierarchy among collaborators. Those with skills in the “more important” tasks may be viewed as playing a more important role in the team. Such a hierarchy can make collaboration difficult, especially if the work of some collaborators is under-valued or taken for granted.

Research is not easy. Most successful projects require the work of more than one person to succeed. In the end, whether a particular step is “harder”



or “more important” is subjective. Some steps are more or less difficult for different people, and what is “most important” early on in the project may become “less important” as the project careens towards completion. A more productive attitude is: **“Everyone has a role to play in research. All the tasks needed to complete a project have their own importance and make their own contributions.”**

When I (Chris) look back at my research projects, I realize even the smallest of things that one of my collaborators did for the group would fill me with energy and hope. I can think of one collaborator who, no matter how busy she was or stuck we all were, would always find the time between meetings to raise a new question or send a new paper for us to look at. Her consistent effort to send us her earnest and simple thoughts was the fuel that kept the project going. Later insights the other collaborators on the project came up with would have been impossible without these consistent efforts.

It can be natural to value the parts you have done on a project or are good at and downplay the parts your collaborators take on. I (Runshan) have heard people complaining about their collaborators doing little in carrying out necessary, if tedious, analysis. I have also heard people complaining about their collaborators’ lack of intellectual contribution. I have also heard complaints that collaborators “just collect data” or “just write the paper”. To me, all these types of work are challenging and require both talent and patience. The goal of collaboration is not to compete with your collaborators. Carefully calculating your contribution and your collaborators’ contribution does not help and could make you resentful. I am grateful for any serious effort my collaborators put into our projects. Such appreciation also allows me to learn more from them.



“People will only collaborate with me based on my strengths”

You can stop yourself from seeking collaborations with others with the thought: “What do I have to offer? I have so little.” But this suggests that people are only willing to collaborate if you have much to give. While having a lot to give is great, it is not necessary. Sometimes you have to offer dedication, energy, and a promise to see things through. And that is enough. We prefer the attitude: **“I have many things to offer others in collaboration; what I can offer depends on what the team needs rather than just my strengths.”**

Sometimes you can even offer your weaknesses as a way to encourage others. As I (Chris) have said throughout this and earlier chapters, problem-solving is not my strongest skill. Typically, my research improves if I can work with someone with strong problem-solving skills. But what do I have to offer them? Well, for one, I have my natural awe and appreciation for their problem-solving talent. Every person would like their talent to be recognized and valued. How better to achieve this than having a collaborator who genuinely values their work? In retrospect, maybe the best thing I had to offer in some of my collaborations was my admiration for the talents of my collaborators. Only where I have a weakness can I truly appreciate the balm offered by those with the talent to overcome my weakness.

“A good researcher must show independence”

We find “independence” to be one of the most complicated notions in academia. It speaks to an ideal that a strong researcher is not dependent on others to produce research. Most Ph.D. programs train their students to be independent researchers. These programs train students in all aspects of the research

process—generating ideas, executing a research plan, writing up papers, and presenting work—and for students to be able to undertake these tasks on their own. Of course, a research advisor and committee assist and guide the student, but ultimately the student’s work is their own.

However, after graduation, the reality of getting or keeping an academic job kicks in. In this arena, requirements for acceptance by the community may demand an amount of research output that is impossible for one person to produce independently. A team of researchers working together is *de facto* required for many researchers. And so the effort put into being “independent” becomes tempered with the practical need to become “dependent” on others to meet the requirements of the job.

Collaborating with others has its own drawbacks from an evaluation perspective. It is harder to assess the individual talent of each team member when they produce a project together. Your contributions to the team may not be apparent or even falsely associated with other collaborators on your team. A common potential for misunderstanding is when a young researcher works with an established one. It is easy to assume that the established one gave the project its “secret sauce”, and the young researcher was doing more of the grunt work along the way. This may be far from the reality, but this perception could be hard to change.

Writing papers on your own sends the strongest signal of your individual talent. Another strategy is to work on a research agenda that is strongly identifiable with you and find resources to help you execute your vision. If you can succeed in this way, all the power to you.

But we think avoiding the opportunity to collaborate or overly emphasizing your own research agenda is a mistake for some researchers. This is especially true for people who start their research careers with little confidence in their skills or who do not have a strong sense of their research agenda. Insisting on your research vision when that vision is underdeveloped can lead to close-mindedness and a narrowing of opportunities.

For people in this position, finding collaborators is one way to keep developing their skills, gaining confidence, and maybe eventually have a stronger research vision. For some, independence only comes with time, if ever. Until then, you can do well being a team player who produces research while continuing to develop. You cannot field a football team full of quarterbacks. Specializing in a subset of skills can get you a lot of victories. We prefer the attitude: **“I do my best research when I put what’s best about me into**

it. Sometimes I need help from others to make this happen.”

I (Chris) took many years to develop a sense of the type of research that I wanted to do. Even today, I am not the most independent of researchers. I greatly rely on others, particularly in their problem-solving talent, to bring my research projects to fruition. I also work on many projects closer to my collaborators' research vision than my own because I believe in them and their work. I am happy to offer my talent for the team to succeed. I have accepted that I am not one of my generation's most visionary researchers nor the most technically talented. I have the career that I have, and it has worked for me. I continue to grow, but I have accepted that I am someone who will likely always depend on others to make my research successful. I am sincerely grateful for their help.

I (Runshan) have learned to be more independent through collaboration. Research is a complicated process that cannot be learned in a few classes or books. Sometimes it is more about how to generate ideas, how to approach questions, how to address challenges, and how to communicate ideas. While you can learn such things by reading papers and talking to people, I think working with others is the best way to learn them. A great thing about collaboration is that you can work with many people and learn from what they do best. This is precisely what has happened to me. I could never become an independent researcher without my collaborations.

11.5 Collaborating across orientations

This chapter includes one additional section beyond the usual purpose, skills, qualities, and attitudes sections in the chapters of **Part III**. We thought it helpful to give concrete examples of how collaborations may work among different types of researchers. Here, we are interested in how researchers with different research orientations (as discussed in **Chapter 4**) can find grounds for collaboration. The goal is to underscore how researchers with very different views of the research process can find grounds for collaborating effectively.

One-to-one synergies

Let's first explore how two people with different orientations might find synergies working together. With six orientations, there are 15 pairs of researchers

with different orientations. We will not explore all 15 pairs; that would only belabor the point.

A Discoverer and a Promoter

A Promoter has exhausted a previously popular line of inquiry and no longer has much to say. She is looking for a new “hook” into an upcoming hot area. Maybe there is a Discoverer out there who has discovered something cool that no one knows about yet. Can she find such a discovery and shine a light on it?

Conversely, a Discoverer has struggled to get recognition for his work. He enjoys research but is reluctant to invest in presenting his ideas before the “big” breakthrough. There is probably some “low-hanging fruit” he could tackle, but why bother? Maybe a Promoter can help motivate him to tackle the low-hanging fruit and get news of their efforts out there?

A Discoverer and an Artisan

A Discoverer enjoys research in its early stages when things crackle with possibility but finds it hard to motivate herself to finish projects in later stages. This is especially true when the grand vision of what something could have been turned into the limited reality of what was. An Artisan can help take an unfinished product and put it out there. Sure, it may not be the most important paper in the world, but a paper is a paper. The job requires publishing papers on the path toward maybe publishing a great one.

An Artisan has been producing elegant and technically competent papers that are not gaining much attention from the community or pushing forward knowledge. This Artisan doesn’t want to be bothered coming up with a brand new idea that could make a splash; it’s time wasted that could be used crafting another paper. A Discoverer can help to bring some intrigue and excitement to what otherwise would be nice, if routine, output.

A Discoverer and a Problem Solver

A Discoverer uncovers many interesting observations and has a collection of enticing conjectures. But turning these observations into formal arguments and proving these conjectures is exhausting. A Problem Solver can help with this heavy lifting.

In turn, a Problem Solver likes to tackle problems but is constantly torn on what problem to tackle. Sure, solving a problem is fun and rewarding, but does anyone care once the problem is solved? A Discoverer can put good problems in from a Problem Solver to solve. This Discoverer can give meaning to the sweat it takes to solve tough problems.

A Discoverer and the Student

A Discoverer is drawn to the unknown. They want to spend their days thinking about things others have never considered. But there are so many papers published already; how can they be sure that what they are thinking about is truly new? A Student can help. A Student is drawn to the known. They want to swallow the ocean of existing human knowledge. Who better to ask about what is known and what is not known?

A Student is overwhelmed by all that they can learn. They have a bookshelf full of textbooks they hope to get to in their lives, but they likely never will. A Discoverer can shine a light on a few books that could be a good place to learn about next.

An Artisan and a Promoter

An Artisan loves crafting papers and getting them published. They let the work speak for itself. Others can come to admire their handiwork. But some of the pieces this Artisan produces are not getting much attention. Maybe a Promoter can bring it to the attention of others. While this Artisan works on the next paper, a Promoter can tell others about the last.

A Promoter needs an evolving portfolio of ideas and results to tell compelling stories to the community. But who has the time to write up so many things? The Promoter feels the grand narrative is more fascinating than all these details. Maybe an Artisan can help chase down all the details and put them in order.

A Producer and an Artisan

A Producer knows what it takes to get a research project through from inception to fruition but needs the talent to bring it to life. An Artisan is someone with a finely honed talent who finds joy in practicing that talent.

An Artisan is always looking to leverage their honed craft. A Producer can direct them to their best use.

Synergies over the research process

We have looked at how orientations interact one-on-one. Now, let's look at things from a different perspective. We examine how each of the six orientations can collaborate across the lifespan of a research project. This chronological view reveals how different researchers with different interests and inclinations all find a space to stand as research evolves.

A common first step in the research process is setting goals and brainstorming ideas for the project's potential. A natural question here is: "What do you want to tell others (or maybe just yourself), and what do you need to do to achieve this?" Discoverers and Promoters are active in this stage, though their approaches may differ.

Discoverers pursue the "truth". They look for areas they are interested in and propose answering questions that could lead to insights and inspiration. Promoters pursue "impact". They look for trending areas and propose to work on ideas that other people in the community would be interested in. Instead of directly proposing the research questions, they may work backward from what might be an answer that would "sell".

While Discoverers and Promoters take different approaches to generate ideas, they can collaborate and end up with a goal that is aligned with both of their interests. The Discoverers may come up with many ideas they are interested in, and the Promoters can filter or shape these ideas, offer alternative perspectives, or frame the ideas in ways that would interest others.

The next step is to uncover answers, solutions, or approaches that will achieve the project's goals. Problem Solvers and Students can play a vital role in this stage. Problem Solvers are drawn to difficult problems and enjoy proposing solutions. Students are also active because finding answers means learning new things. Students will have a lot of energy to search in books and papers for solutions others have already proposed.

Once satisfactory answers, solutions, and approaches have been found, it is time to summarize and write the results. This research stage has a life of its own and can still take a significant amount of time. For some, this part often sounds less "exciting" because there is less uncertainty and discovery to be had. For others, this is when they come alive, as they can see the whole picture of

the project and now need to piece it together.

These tasks fit the eye of the Artisan. Artisans are drawn to work that requires consistency and a view toward the final product. Artisans enjoy this process of making steady progress towards a concrete goal and find motivation in elegantly “wrapping up” a project long after the intrigue and excitement of discovering and problem-solving are gone.

But writing the paper is not the end of the life of many research projects. When you are in the process of doing research, you need to collect feedback on how to improve the paper and what the main concerns are. When you finish a paper, you must share it with others to gain acceptance. The Promoter is the best candidate for this job. They are motivated by conversation and acceptance and know when to listen for feedback and speak to others when they have an audience. They have sharp eyes for identifying what interests people and have the skills to frame a paper in exciting ways.

So, what is the role of the Producer in all of this? They are the ones who find the right people to do the right job at the right time. While the “task assignment” described above seems natural and efficient, it may not easily happen. People may be unaware of their own strengths and weakness, or they cannot find places to utilize their talents. These are the natural frictions of collaboration.

The Producer understands the team members, knows their strengths, and sets them up to succeed. They usually have a holistic perspective and can quickly identify potential problems in the whole research process. Whenever something goes wrong, the Producer knows who is the right person to deal with it and what resources they can play with. They ensure the project does not stall for long at any one stage.

11.6 Putting it all together

As a form of summary, we provide an example of young researchers exploring the ideas in this chapter. The story is fictional but inspired by real-life interactions with many young researchers over the years. The main purpose is to provide you with an example of how natural it can be to discuss your approach to research with those you trust.

Chanettia just started her job as an assistant professor at a research-focused university. After settling in for a few weeks, a senior faculty member came to

provide her with career advice. He emphasized independence as the key ingredient to getting tenure and that Chanettia needed to learn how to break free of working with her advisor, whom she collaborated well with during her Ph.D. studies.

At first, this seemed perfectly reasonable to Chanettia. She had her own ideas and felt some degree of confidence she could implement them herself. Over the next year or so, her view slowly changed. Although she had ideas and was still confident in her skills (at least to some degree), her enthusiasm for research progressively declined. Progress was much slower than during her Ph.D. studies.

She found this odd because she had always been a very independent student, meeting her advisor only once a month for advice and guidance, except near the end of projects when her advisor carefully reviewed her writing. But now, it all seemed different. Sometimes she spent weeks wondering where the time went, not producing research documents or making other types of progress. She knew she needed to produce to get tenure, but it was becoming harder for her to visualize how this would ever happen.

She also started feeling strongly nostalgic for her time with her Ph.D. cohort and even reminisced about her monthly visits with her advisor. She contacted some of her cohort mates, but they all seemed too busy with their tenure climb to discuss research with her.

Chanettia slowly realized that was becoming isolated and increasingly disaffected with research. She thought maybe she needed a new topic to excite her, so she took the opportunity to explore one at the next big academic conference. There, she met Agustin. They met in an elevator at the conference hotel. Agustin was wearing the conference badge around his neck and looked to be lost entirely.

Chanettia bravely introduced herself and soon realized they were both going to the same cafe for coffee. They decided to go together and sit for a while to chat. After a few minutes of small talk about the weather and how intimidating the conference was, Chanettia felt brave to take the conversation a little deeper.

Chanettia: So Agustin, what talks are you excited to go to?

Agustin: To be honest, none of them. I am not even sure why I am even here.

- C: [Shocked by his honesty] You know what, me either. I guess I am looking to learn something new to find some inspiration. I have been short on that these days.
- A: What good is inspiration if it does not go anywhere? The last three things I tried got rejected. I really don't know anymore what people want.
- C: Sorry to hear that. I can imagine that it's very frustrating to be rejected. I have been lucky so far; all of the chapters from my dissertation got through. But since then ...well, I envy you that even had three things to try with.
- A: Rejection is all I have known. I eventually got two papers from my dissertation through lower-tier outlets after several rejections. It seems reviewers never get what I am talking about. I don't want to be bothered with reading their minds. I want to get my ideas out there and move on.
- C: I can understand your frustration, but I must admit I find your words inspiring. You keep getting rejected, but you still have the energy to go on to the next project and still have the desire to get your voice heard. I realize that starting something new is hard for me, but I enjoy polishing a paper and selling it to the reviewers. It feels like a game of sorts. My advisor always commented that I was good at anticipating the concerns of others.
- A: Wow, I could really use someone with your kind of skills.
- C: How do you feel about me helping you with one of those papers that you got rejected? I am looking for something new to work on, and maybe I can help.
- A: You would do that? That would be a huge help. I am worried the topic won't interest you, don't you have your own thing?
- C: The topic is not hugely important to me; I am sure I can find interest in it. I am more excited about how I can help you and solve a problem for both of us. What do you say?

Over the next several months, Chanettia and Agustin started to meet virtually multiple times a week. First, they worked on revising Agustin's latest rejected paper. Chanettia could see that the idea held much promise, but it was hastily written. It took some months of meeting with Agustin for her to fully understand his work.

Once she understood what he had done, she suggested changing the setting entirely and only keeping the kernel of the idea. She thought a different setting would make the idea stand out more. By this point, Agustin trusted Chanettia, so he agreed to try it.

Chanettia took the time to completely rewrite the paper from the ground up. In the process, Chanettia uncovered a few subtleties and points of confusion that they solved together. Chanettia began to deeply trust Agustin's ability to get them out of tight corners; he always seemed to have a way to resolve her confusion or resolve a logical flaw.

Agustin was delighted with the resulting draft. It looked much more like the papers he saw getting published, papers he struggled to produce himself. Chanettia and Agustin sent the paper to a top journal. They were pretty confident but did not wait around for a reply. They had found a groove working together and wanted to keep it going.

Chanettia asked Agustin if he might be interested in working on one of her research questions next. He felt it would be fruitful; maybe he could learn more about Chanettia's talent for crafting papers in the process. Chanettia had been kicking one idea around for months (perhaps years) and felt confident that Agustin could get it beyond the idea stage. She was also keen to learn from him how his mind worked.

After a long virtual work meeting, gaining clarity about the research question, and making a few crucial initial steps about how they would approach answering it, they were exhausted. In the last few minutes, they had the following exchange.

Chanettia: Agustin, I wanted to thank you for your help on this. I felt so alone and lost six months ago, and now I feel different. Research has started to make more sense to me again.

Agustin: The pleasure is all mine. Every time we work together, I feel another piece of the paper-writing puzzle falls into place for me. Your ideas are truly something I could never imagine coming up with them on my own.

- C: Happy to hear that, but you are too kind. My contributions are small.
- A: Not at all. I thought my papers and, by extension, my career was dead. What you did to resurrect my paper got me thinking positively again.
- C: Then the feeling is mutual.
- A: But it is more than just enthusiasm; the work feels easier when I have someone to talk to.
- C: I agree. When I used to get stuck, I doubted myself, which could lead to days of wallowing. It was bad. Now, if I have doubts, I share them with you. I can avoid all of that self-doubt. The doubts remain about the work, but no longer about myself.
- A: Glad I can help you overcome your doubts. I like to solve puzzles for their own sake. But it feels even better to see the relief that comes to your face when I can solve them. But having said that, I am not even sure I do all that much sometimes; once we start talking, it seems you solve half the things on your own anyway.
- C: That is most certainly an exaggeration, but I do surprise myself sometimes. Thanks for the compliment.

Just then, an email arrives in both of their inboxes, their phones dinging simultaneously. Their first paper together got a major revision at the top journal they submitted to. Both were overjoyed.

11.7 Reflection questions

The questions below are provided for you to reflect on what you have read in this chapter. If possible, it is beneficial to discuss these questions in a group of your peers.

1. Describe an experience you had collaborating with others (not necessarily in the research context). Was it frustrating? Empowering?
2. When collaborating, how did you divide up the work?

3. What parts of the research process are more likely to give you energy? What parts of the research process are more likely to drain your energy? (If in a group) How does this compare to others in the group?
4. Describe something you have learned from someone else that you may have never been able to learn on your own.
5. Do you approach work differently if you are responsible to others than when you are only responsible to yourself?
6. Under what circumstances do you prefer to use the following modes of communication when working with others: (i) in-person meetings, (ii) virtual meetings, (iii) email, (iv) instant messaging?
7. Describe something you are grateful for. How do you show your gratitude?
8. Why should other people be willing to collaborate with you? What do you have to offer?
9. What kind of person would you like to collaborate with?
10. In what way, if at all, do you relate to the story of Chanettia and Agustin? What do you take away from the story?

Chapter 12

Conclusion

Let's return to the first thing you read in this book—the title, *Paths to Research*. The previous chapters laid out much of the mechanics of walking a research path, but also motivations for why you might do it. We want to emphasize again here that there is more than one path to walk. There is a great multiplicity of paths.

These chapters have highlighted this multiplicity, including different aesthetic profiles (discussed in [Chapter 3](#)), research orientations (discussed in [Chapter 4](#))—and even the different purposes for exercising your research capabilities ([Part III](#))—we have delineated hundreds of different paths. You may be an inward-looking Discoverer who values novelty and usefulness and likes to write papers to record findings, present to impress, and collaborate to multiply your efforts. Or maybe that is just part of your research persona.

Once you consider research collaborations and ponder the paths a team of researchers can take together, the number of possibilities explodes into the millions. Maybe the inward-looking Discoverer in the previous paragraphs teams up with an outward-looking Problem Solver who values difficulty, reads papers voraciously to learn new tools, and collaborates for simple companionship. What a breathtaking combination! There is no end to the possible journeys up the treacherous mountain of research.

This also sounds well and good. There are many paths to research, but is there a path for you? Even if there is, should you take it? This leads us to one last question that might still be burning in your mind: **“Is a career in research right for me?”** Only you can answer this question, so don't expect any answers coming from us. But we would like to take the remaining pages

of this book to share a few final thoughts from our personal experiences.

First, let's establish one fact. Nearly all of us need to have a career of some kind to survive. Maybe one of the most frustrating things about life is that you need a career at all. It might be nice to pursue your interests and ideals and not worry about how you will survive. Unfortunately, we know of no system that can deliver this kind of life to many people.

Given that you need to have a career, there are implications that you need to accept. There will be times when you have lost interest in what you are doing, but you have to do it anyway. There will be times when you need to do things you don't want to do. Just like there is no perfect life, there is no perfect career. A career is something you need, even if it is something you don't always want.

When you are learning about careers, it is relatively easy to learn about sugarcoated versions of the possible options. We have the police officer who protects the weak and champions justice. We have a restaurant server who brings joy to their patrons and helps create life-long memories night after night. We have the researcher who discovers great truths and advances human knowledge. These are the views on the work you find on company websites and work brochures.

It takes more effort to learn about what actually goes on in those careers. For a job like a police officer, we have TV shows like "The Wire" that expose some of the dark sides. Maybe we have even run into the law in real life. Being a police officer is not always about protection and justice. For restaurant servers, we can learn for ourselves by working at a restaurant or by observing how servers work long hours for low pay, high stress, and with aching backs and feet.

In academic research, the work is much less visible to the outside world. Maybe you are lucky, and you have a family member or friend in academia, but even here, you cannot easily "see over their shoulder" into what really goes on in their life. I (Chris) have two sons who are completely bewildered by what I do. They have a vague idea that I teach adults, do math, and write a lot. Beyond that, there's not much they can say. The same goes for most of my friends and family.

In this book, we have provided our honest assessment of what a career in research entails in our experience. We have attempted to be honest and have gone out of our way *not* to sugarcoat things. We have detailed many paths to research, but we have also made warnings of challenges along each of them.

There are probably some things in this book you have not heard about before. There are things in this book that you may find hard to believe.

But it is truly hard to know what are the frustrating aspects of a career for you because not everyone is frustrated by the same things. Sure, we can all agree that getting our papers rejected is not super fun. But we have known people who don't seem to mind rejection. They take it as a challenge to turn people's minds. If they didn't get rejected sometimes, they would feel they were not pushing themselves.

We have tried to provide tips to make the journey more manageable. We have not tried to oversell or undersell the worth of taking that journey. We figure the best we can do to help you decide is to tell our stories and what we have observed. We hope this book serves that purpose for you.

We have heard many reasons why people are interested in a research career. For some, it is the joy of discovery. For others, they like the possibility of getting a stable job protected by tenure. Still others enjoy teaching and interacting with young people. Some gravitate towards the job's flexibility and the fact you don't have a boss breathing down your neck every day. Some enjoy the fact you can think your own thoughts and not need to tow too many lines. Others raise doubts about whether an academic job even provides the stability, flexibility, and freedom that's advertised. Some suggest you run for the hills and work for a big company that may pay much more.

We strongly believe that the relationships you have with people surrounding you are often as or more important than the actual content of your work. You could be doing life-saving work that inspires millions in an office where people undermine and back-stab each other. You could have all the flexibility in the world to pursue your thoughts but be around people who invalidate every thought you have. If you enjoy the culture and personalities of the academics you have met, going after being around people like that makes a lot of sense.

In the end, a very concrete goal in writing this book was to assemble in one place all the things we wish people had told us before we got too far along the path toward being an academic researcher. We think it would have made the journey much easier. But we don't think it would have deterred us from pursuing our chosen career paths.

I (Chris) am very grateful I had the opportunity to do a Ph.D. and become an academic researcher. I would still start a research career knowing what I know now. For one, I love teaching in a university environment. But I also

just like learning things and explaining them in the written word. I am okay if my work is never life-changing or world-shaping. I enjoy the daily tasks of being a researcher.

I must admit that there were long periods when I was frustrated with research. Part of my frustration early in my career was a frustration of disillusionment. The job was not what I thought it was and not what I understood people told me it was. I came in thinking very naively that the role of being an educator was much more highly prized and valued than it was in the end. I was also under the impression that we would do research that would help people. Later on, I realized that most of our research does help certain people, primarily those close to us. It was not as helpful to the “person on the street” as I hoped it might have been. But I am okay with that now. I am not entirely sure how the system would work otherwise.

But I think my greatest joy in research comes in supporting the careers of young researchers coming up behind me. I hope this book is a testament to my desire to be of help. I truly do hope it is helpful. If even one or two students avoid some of the frustrations that have crippled me at times, then it will have been worth writing this book.

As for me (Runshan), I think I am lucky to pursue an academic career, and I would make the same choices if I had the opportunity to go back. I am curious about many things, and one pursuit of my life is to see and experience more of the world. Doing research allows me to observe and think about many different problems. This is a huge reward for me. I also enjoy sharing and explaining ideas with others. Of course, there are frustrating parts of the job that I do not enjoy. But over time, I have been learning to adjust my mindset to focus more on what is real and less on what I hope or imagine life should be. This greatly helps me reduce the negative feelings that come over me sometimes. I can better enjoy the work by focusing on what is real and true. One of my goals in writing this book was to present what I find to be real and true about a career in research. I hope that by shining a light on the truth I have witnessed, you may also have more opportunities for joy in your research.

We end this book with one final question we have no intention of fully answering: **Suppose you achieve success in a research career, what then?** Once you have been accepted into the community, what next? We have touched on it to some degree in **Chapter 4**, but we admit that this deliberation only scratches the surface. Some may argue this question is of greater significance than the question we attempted to tackle in this book. After all, isn't career

success just a practical step in a meaningful career as an academic? Isn't a job and tenure just a platform for a researcher to speak their truth? We are content to focus on the humbler goal of career success and leave you to answer the "What then?" question. We would be delighted to hear your perspectives. It might even inspire us to write a sequel to this book!

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