A Guide to Scientific Research
by Daniel Green

Starting out in research is hard. It is difficult to find people with experience that can tell you what its like and it is easy to get stuck with misconceptions about research that undermine your long term goals. My hope in writing this guide is to give some general lessons that will both help you make better career decisions and enjoy the research process.

This guide is not targeted at any specific field but my opinions are certainly shaped by my experience as a theoretical physicist. My hope is to help younger researchers reach their goals, particularly undergraduates through postdocs. I would like to think it will help you be a successful senior researcher as well, but I admit that would be a bit presumptuous on my part. Fortunately, more senior researchers can look at the excellent essay and book by Richard Hamming that have informed many of my thoughts.

Disclaimer: Some of this advice will be unpopular with established researchers. Research is like driving, everyone thinks that they are doing it right and that everyone else is doing it wrong. However, the real message I hope comes across is that there is no one right way to be a researcher. I am only offering suggestions for how to find the style of research that works for you.

1 Big Picture

By its very nature, research is a creative process: you are trying to create knowledge that didn't exist before. However, unlike other creative endeavors, nature is an impartial arbiter of this process. While trends in research do follow the whims and tastes of individuals, fundamentally good research should be making contact with the world as it is. Since most people have no intuition for the research process, I will lean on two kinds of analogies that capture these two sides (creative and unforgiving) of the research process: art and sports.

Art captures the creative process and the infinitely many ways you can be a great researcher. Since the entire point is creating something new, you can't say ahead of time what skills and techniques are essential for success. Imagine trying to explain to Picasso that the correct way to be a great artist is to make your paintings realistic. What makes some scientific papers great is that they completely redefine what kinds of questions people in the field ask, creating a whole new style of research.

Analogies with sports are also useful as sports are subject to much more impartial standards for success. Unlike the arts, the winner of a race is not subject to the whims of critics or popular culture. Success in scientific research is similar both because nature is an impartial judge and science can be competitive. The most vexing problems of the day are being pursued by many people and making new contributions to world knowledge requires getting the result first. Being first is also what makes research hard, you have no one else to compare to.

Finding Your Style

One of the most damaging ideas in science is the *fixed mindset*, the idea that being a successful scientist requires some innate talent, i.e. you need to be born to do it. The problem with this

idea is explained in great detail and backed with data in *Mindsets* by Carroll Dweck (see also *The Art of Learning* by Josh Waitzkin). However, we can understand the fallacy by going back to the fact that research is a creative process. What does it mean that you are "good" or "bad" at something if what you are trying to do has never been done before? How can you possibility know what skills are essential for success?

Instead, when you think about what defines the great scientists, it is usually their unique way of doing research (e.g. choice of topics, techniques, goals). In some cases these different styles reflect the different approaches you need to solve problems in different fields. But even within one subject, there can be multiple successful styles (see Section 3 for examples from theoretical physics). This idea was articulated by Howard Georgi, who wrote eloquently on the subject:

the number of ways of being a great scientist is at least as large as the number of great scientists

When you are starting out, it is helpful to learn about successful scientists, what drives them, and how the solve problems. This can be useful in your own work in expanding kinds of problems you work on or how you solve them. Yet, the ultimate sign you have found your lane is when you can look around and realize that there is no one else more prepared to solve your research problems than you are.

If you are just starting out, it probably doesn't feel like the path to being a great scientist is so flexible. You take a well laid-out set of courses and are given grades on how you perform on well-defined tasks. A useful analogy here is being a great chef : the best chefs in the world are defined by creativity and imagination, but you can't be creative until you learn a lot of basic techniques (like how not to give your patrons food poisoning). The absolute best chefs combine incredible technical skill and creativity in harmony, but you can still make creative and delicious food without mastering every technique under the sun.

As career advice, this means what you are trying to do is find your style as a researcher (also known as 'finding your voice' in writing). This is a combination of finding the area that makes the most of your experience (technical skills) and your interests. Concretely, in any given field or even within a big project, there are lots of different problems that need solving and different kinds of skills that are needed to solve them. As illustration, you might have a knack for solving well-defined coding problems, like speeding up algorithms or finding signals in messy data. On the opposite end of the spectrum are people who like puzzles where all you know is that something is wrong (e.g. there is a problem with your theory, equipment, code, data) but you don't know what it is or where to get started. There are many such axes along which you have some choice of how you spend your time, and you want to have a strategy for making these choices that maximizes your enjoyment of the work and your impact on the world.

No matter what you do, it is going to take a lot of hard work to learn all the background material and develop the tools you need to succeed. To make this a bit easier on you, it helps to find problems where you have a unique insight that gives you a competitive advantage. E.g. if you have more coding experience than your peers, are there more computational problems you might be better suited to attack now? At the same time, if you don't enjoy the majority of the work you would need to do, even if you are capable of doing it, then maybe you should find something that fits your interests better[1].

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[1]Research is still a job so you will always have to do things you don't exactly want to do.

Your style doesn't have to (and probably shouldn't) be fixed over the course of your career. Once you have a routine that works for you, it makes sense to expand it, try new things, collaborate with people who look at things differently. I have been a string theorist, particle theorist and cosmologist in my career. The most fun part of my work is that I made friends in all of these fields and I continually find ways to connect with them and their research. A side benefit (much less important than the pure fun) is that I can draw on a much wider range of inspiration for both problems to work on and strategies for solving them.

Setting Goals

The goal of your research is (hopefully) to produce excellent results. It is not maximizing citation counts or numbers of papers or some other fixed metric. It is excellence[2]. However, as you proceed through your career, it might seem like citations and paper counts are important. Ed Catmull gives a great explanation of the balance of these two competing thoughts in his book, *Creativity Inc*, where he describes the values at Pixar. Their singular goal is making excellent movies; but in order to have money to make excellent movies they still have to keep on schedule and bring in enough revenue at the box office. The same is true for your career, you need to produce enough papers and get enough citations to advance to the next stage of your career; however, it is important that you don't lose sight of the true goal, the pursuit of excellence, which is both more rewarding and more fun. The problem, of course, is that because we can calculate your citation counts or your h-index, it is easy to think it is a good proxy for excellence.

One starting point is learning to recognize excellent work from good and pretty good. Of course, when you are starting out you shouldn't expect your papers to be at this level (mastering anything takes time and effort) but they should be your guide as you progress through your career. In order to chart your growth, recognize that research lies on a continuum, with *The Principia* and *On the Origin of Species* near one end and my 5th grade science project on the other. The exact order in the middle doesn't really matter and what you value is another reflection of your own style[3]; just having this continuum in mind will help you set goals for your own growth.

The problem most people encounter is that their only model for excellence is Einstein, or someone in the same category. I hate to burst your bubble, but you will not be Einstein (a once in human history level of impact) and so you need some definition of excellence that includes work within the realm of achievability. A warning sign that you don't have a good metric of excellence is if you try to think of the best papers written in the past 10 years and you either can't think of any or you only list papers that are essentially identical to your own work. You would be lucky to write the best paper of the decade in your field, so it is a good habit to know what kinds of papers you would put in that category and what makes them special.

While this description sounds competitive, it is just the recognition that research is a marathon and not a sprint. You both need to celebrate your own improvements while keeping in mind that far greater things are possible for you. To understand this point of view, imagine how hard

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[2]I am not advocating a purely fixed notion of excellence or that there is only one way to be excellent; your definition should evolve to be inclusive, not exclusive.

[3]Think of this like your own IMDB rating for science papers. You don't have to agree the IMDB top 250 in detail but you should at least understand roughly why some are high on the list and others are low.

it would be to write the best paper of anyone in your career stage at your university. E.g. if you are an undergrad at UCSD, that means writing the best research paper in a community of 30k undergraduate students. This is a very worthy goal (any paper as an undergrad should be celebrated) and one you are unlikely to achieve. Yet, it is also reasonable to expect that by the time you are an established researcher, the quality of your work will be beyond the level of any undergraduate.

Sports provides a useful analogy here: when you watch the olympics, it is very rare that the gold medal winner came out of nowhere. While it may be the first time you have heard of them, the people who follow that event closely usually saw their progress coming years before. E.g. in racing events, they usually posted times at earlier events that showed they were on a "world class" trajectory. The same is true in research, it is very rare that someone writes a world changing paper out of nowhere – usually they were writing papers of world class (and increasing) quality long before the result that made them famous.

With this in mind, you should set reasonable goals for where you can get within the next year, three years or five years. What defines the papers that you feel are better than yours right now and what is stopping you from getting there. Are there techniques you are missing? Or are you missing the connections between your specific area of research and bigger problems in adjacent fields? What could you do today to fill those gaps in your next paper?

Aside: I have encountered a lot of people that feel having a plan is some kind of purely careerist approach to research that runs against the spirit of exploration and creativity (the art analogy is that they think it is "selling out"). I simply cannot see this point of view: imagine an athlete that thought having a training plan and practicing was not in the spirit of playing sports but still wanted to make the olympics. If you just do stuff at random with no plan, you never get anywhere on average and you never get as far as a person who had a plan (this point is very well articulated by Hamming). The planning does not eliminate the need for creativity and spontaneity, it is just the acknowledgement that your best work will only come with practice and experience so you need to work towards it (a point of view very well explained by Dweck).

Science is Social

At some point as you progress through your career, learning from talking to people directly will take the place of classes and textbooks. In addition, for the vast majority of people, every aspect of research involves collaboration with others, both in small and large groups. Being able to learn from and work well with others is an essential research skill of the modern scientist.

People are complicated and there is no manual I can provide about how to manage your relationships effectively. However, I think there is an basic point that is underemphasized: helping to create a positive research community is both your job and something that benefits everyone (including you).

To start, it is important to understand that you are not in competition with your fellow researchers. If you work well in a group, you will all get further than you would as individuals. The same is true for the community that defines your particular sub-field; being part of a community where lots of exciting research is happening will benefit everyone ("the rising tide lifts all boats"). The academic job market isn't sum-zero game (at least for your purposes), areas where there is

lots of visible progress tend to have plenty of employment and funding opportunities. In short, your own career goals are in-line with supporting your community.

Here are some rules of thumb for helping to create such a community

- Be positive about good work. Share your excitement about good work done by others. This helps people feel included and appreciated. It also helps people outside your field appreciate all the exiting work being done in your field (including yours).

- Be respectful of others. The goal of talking to people is to learn, to improve the quality of your work, and to help others. Ask questions only with the intent of learning something. Respond to questions and comments honestly. Don't ask "gotcha" questions or try to embarrass people. Don't respond to honest scientific questions as if they are personal attacks.

- Have humility. What you understand will always be a tiny fraction of the sum of human knowledge. Appreciate that you can learn from others.

- Science is hard and it is easy to fool yourself. A healthy community is one that can both offer and accept constructive criticism without hurting people's feelings. Communities that can't do this become ripe for cargo cult science.

- Cite other people's work. This is (a) your job and (b) a bare minimum of what you can do to acknowledge the rest of the community. Intentionally not citing your colleagues (or only citing famous people) doesn't make you look good, it makes you look petty.

- Don't tolerate bad behavior (i.e. the opposite of the above list). This is a complex topic and many of the problems lie in the senior ranks of academia. Nevertheless, I highly recommend this TED talk on how to identify the bad actors and the negative consequences they have for the community.

2 Career Stages

Undergrad

The main purpose of undergrad research is to be exposed to the pace, style and techniques of everyday research. Sometimes part what you will get from this experience is a technique that they don't teach you in class. Other times it will be learning that research is not like homework problems: there is isn't a solution manual or grader to check your work. You are responsible for finding the right techniques and making sure they work. Regardless, experiencing any or all of the parts of the research process is a win at this stage of your career.

I gave a talk specifically about undergrad research here, so here are just the highlights:

- Start early: research experience is like investing, it builds on itself so the earlier you start, the bigger the return. The skills and experience you gain today will lead to better opportunities tomorrow.

- Getting experience may seem like a catch-22, you need experience to get a job. Fortunately, there are lots of ways to get experience without having a research job. If you take the time to build something yourself, write some code, analyze some data, learn to use important software packages, or really anything open-ended, you will learn skills and gain experience that will help you land a job in the future. If starting a big project on your own sounds daunting, there are also lots of free tutorials, kits, videos, and (online) communities that are there to help you along the way.

- You don't have to know what you want to do in the long run and undergrad is a good time to look around and see what fits. But that doesn't mean you should choose research opportunities at random. Reflect on what you learned from experiences so far and let it inform what you want to do next.

- One of the points of doing research as an undergrad is to get used to the idea that research is not like a homework assignment. Ask for open ended projects and learn to enjoy the uncertainty. Don't ask for the project to be broken down into something that looks like a problem set; it might be easier for you to finish, but that isn't the point.

Graduate Student

Graduate school is where most people first really come to grips with the reality of research. You might have gotten a taste as an undergrad but it is likely you had a small project designed to be finished in a short amount of time, or just a small piece of a bigger project. Graduate school is really about learning to take a project from beginning to end and mastering the uncertainty that comes in the middle.

While your PhD project will hopefully be an important scientific contribution, most of the time all you are doing is building habits that will lead to bigger and better results in the future. Some of these habits include:

- Learning to learn: you can never know everything you need to learn before you start research. In fact, the rate of knowledge grows so quickly that it is impossible to keep up. Instead, the skill you are developing is learning what you need to know when you need it.

- Getting comfortable with uncertainty: Every project is new so you have no idea if it will work. You usually reach a point when you think the whole thing is going to fail and yet most projects tend to work out in the long run. Seeing this happen a few times will give you the patience and confidence needed to navigate the obstacles you will inevitably face. A good analogy is debugging code: you don't know why it isn't working, but if you have successfully debugged code enough times you know you can figure it out.

- Sometimes confusion is the goal: Papers are written in the form "We asked question A and found answer B", but the actual research itself is very rarely linear. You usually start out with question A and think you can find answer B, but along the way you encounter problems and get confused why it didn't work as planned (this is explained nicely in this TED talk where this confusion is called "the cloud"). While you try to understand what

happened, you realize that you could have asked question C and can produced answer D (sometimes you find D before you realize C was the right question). You then write a paper saying "We asked question C and found answer D". The whole point of question A was just to find a place to start; it is just your entry point into an area where there are lots of results to be found. You don't simply abandon the area when you realize A is not the right question, it was only a rough starting point[4].

One piece of advice for succeeding in graduate school I learned from Steve Kivelson: pick a model[5], any model and learn absolutely everything about it. Learning everything doesn't mean you took a course and are aware of the main results, it really means having done everything yourself. You have reproduced every major result yourself, you know every technical assumption, every important detail. Then for your rest of your career, when someone tells you something new you can say "that is just like in the X model where Y happens". The point is that if you take the time to understand one example really well, it will serve as essential intuition to guide you through uncertainty later (nature has surprisingly few tricks, and many of them rhyme). If you are really ambitious, pick two models so that you can bring more than one perspective.

By the end of a PhD, my hope is that you have reached two goals:

- You are fully in command of your PhD research. You know all the important results that came before that made your work possible and you know all of the technical details needed to get your results.

- You know that you could do the same again for a different problem.

The second point is why PhDs are valued outside of their narrow field of study and outside of academia altogether. If I am working on something that no one has done before, I expect that someone with a PhD can look at the problem, learn the relevant material and chart a path to solve it. I flatly reject the idea that a PhD is only preparation to be an academic position (i.e. a professor), it is preparing you for a career (life) in an evolving and uncertain world.

Postdoc

Being a postdoc is the step of the academic ladder that is most difficult to navigate. Ideally, you are given a lot more freedom to define what you do. Unfortunately, this extra freedom is a double-edged sword and many people end up with self-inflicted wounds. Part of the problem is understandable, you may feel you have a short amount of time to prove yourself and the pressure to produce results on a short timeline doesn't lend itself to good decision making.

The hardest part of being a postdoc is setting both realistic and ambitious goals at the same time. You are aiming to be on a world class trajectory, but you can't rush the process. Before you start worrying about writing world famous papers, you need to learn how to come up with

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[4]This is closely related to suggestion made by Nima Arkani-Hamed, the goal is to put yourself in the "basin of attraction of the truth". In essence, nature is the judge of good and bad ideas, so as long as your starting point is close enough to the truth, the scientific method should guide you to the right kinds questions and answers.

[5]In the original context, this meant models for condensed matter system (Ising, Hubbard, etc). However, more generally, it means pick some representative example and study it to death.

an idea and see it to completion on a reasonable schedule, and then do it again. Once you can do that, you can start raising the difficulty and diversity of the kinds of problems you take on.

A postdoc is an excellent time to find your style. As a graduate student, your style will often mimic your advisors and being a postdoc is often the first chance you have to define yourself (once you are producing results consistently). This is also the point where you will start to find you own collaborators. Part of finding your style is also finding people you work well with. Working on something you don't like, either because the subject or the people don't seem like the right fit, is a warning sign and you should really think about whether something else might suit you better.

At the same time, there are new kinds of expectations for postdocs that are not true of grad students:

- You should know the field you work in, not just your own papers. Who else is working in this field and what are they doing? This means that if someone claims to have a big result in your field and a colleague asks you about it, you can give an well-reasoned answer to whether the result is right or wrong and whether it is an incremental improvement or a true breakthrough. If you hate reading other papers in the area you work in, then it might be a good idea[6] to find an area where you don't.

- You should be able to back up your ideas with results. If you think a problem can be solved a certain way, then you need to actually do it and show it. As a grad student, your PhD advisor will typically sort through your good and bad ideas for you and help you find the right strategy. A postdoc is a path to being an independent researcher, and the first step is debugging your ideas yourself.

At this stage of your career, I think there are two simple suggestions that are easy to follow that will help you a lot:

- Make a plan for what you want to get done in the next 6 months and the next year. A postdoc is not that much time, and if you don't prioritize the stuff that you really want to get done, then you run the risk of leaving your most important work unfinished.

- There are no bonus points to writing a huge number of "just okay" papers. It is easy to fall into the trap of thinking your goal is to write more papers than your peers. Not only is it unproductive for your career, it can take all the fun out of research. Writing better papers each year will have exponentially more upside than increasing the number of papers.

3   Style of Research in Theoretical Physics

This section will make more sense to people interested in a career in theoretical physics, so I have kept it separate from the more general research advice

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[6]I speak from experience here. I was trained in string theory in grad school and I really enjoyed my work. But there would be 100 page papers claiming big results that I thought were wrong but I didn't want to read the papers to figure out why. I essentially stopped working on string theory as a postdoc because I knew this was not a healthy situation for me.

Theoretical physics[7] is often a field where you hear discussions of "style". One common introduction to theoretical style comes is in the form of discussions of "west coast" versus "east coast" physics. While the differences between the two are often described in highly exaggerated terms (even by their proponents), these cartoonish descriptions are still a useful window into choosing your own brand of research.

I first encountered the idea of a "west coast style" when I was visiting graduate schools. I was sitting in the office of a well-known west coast physicist when they drew the following figure on the board (Figure 1): the axes were "interesting" and "well-defined" and this professor drew a horizontal line and said "at this institution, we work on problems up here". In short, on the west coast they only care about working on interesting problems and they don't care if the problems are well formulated mathematically (this was surely an exaggeration for emphasis). I did my PhD on the west coast (Stanford) and I got a great education in west coast physics. I spent much of my time trying to understand what problems were theoretically important and why. However, I spent less time developing the mathematical tools needed to solve these problems, the assumption being that I could pick them up as needed.
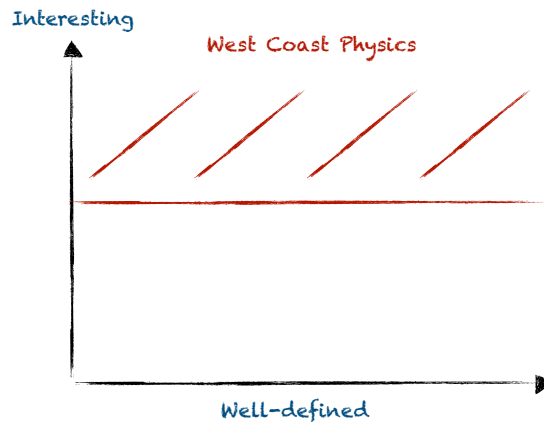

Figure 1: West coast physics (exaggeration): as long as it is interesting enough, it is worth thinking about.

I moved to the east coast for my first postdoc (Institute for Advanced Study, Princeton). It didn't take long to understand that in Princeton, there was much less patience for the kinds of hand-waving physical arguments I encountered during my PhD and much more appreciation for purely technical development of theoretical tools. With the same kind of cartoonish exaggeration, I viewed the style at the IAS as being one of pure mathematical rigor, Figure 2. Being exposed to this increased emphasize on rigor revealed a key problem of my west coast education: I had a very clear sense of what problems were important but I didn't have the tools to really solve them (yet). It isn't that I didn't know anything, picking good problems is half the battle of doing good research. Yet, it was very clear being around people at the IAS that details really matter when you want to solve the kinds of problems I was interested in and I had undervalued this kind of

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[7]Theoretical physicists also have a reputation for being "smart", which is almost a definition of a fixed mindset. If you have read this far, you should already know I reject the idea. I know a lot of theoretical physicists and what they have most in common is that they work extremely hard.

attention to detail (I really mean "I" here, I am not speaking for anyone else).
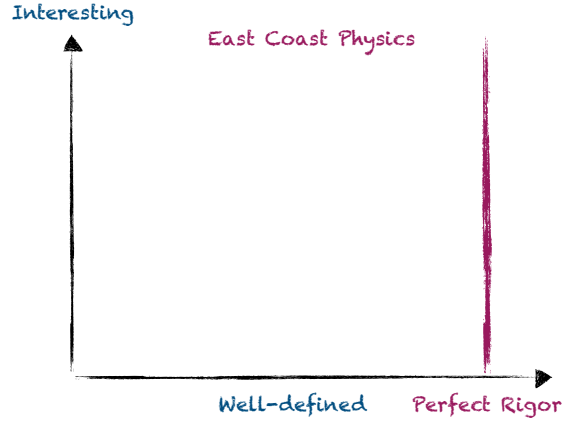

Figure 2: East coast physics (exaggeration): theoretical physics = mathematical physics. If you cannot translate your problem into a well-defined calculation, working on it is a waste of time.

One way of thinking about your personal style is as a curve in the interesting versus well-defined plane. During my postdoc at the IAS, I made a lot of changes to my research, switching from QFT and particle physics to cosmology. Part of this change was related to finding the balance between the kinds of problems I liked working on and the area where I had the skills to solve them. I had the skills to be a particle phenomenologist but not the interest in learning about collider physics (which was essential at the time because the LHC was just turning on). I was also very interested in pure QFT but I not was obsessed with the details at the level I felt was needed to make a big impact (I was lucky to be surrounded by amazing postdocs so it was not hard to make this assessment). The kind of cosmology being done in Princeton was the perfect balance of capturing my imagination, being theoretically precise, and connected to the real world to both fit my curve (Figure 3) and keep me motivated to learn everything I needed to make an impact.

I have worked with a lot of very successful people who have very different curves from mine and from each other. I have seen first hand the way Nati Seiberg can perform magic by pulling on the loose threads of a seemingly correct calculation, and understand why there are people who subscribe to the 'only well-defined problems' philosophy. The self-consistency of theoretical physics is in such a delicate balance, that you can get a very long way by demanding that the whole structure holds together (see Seiberg duality, Seiberg-Witten theory, AdS/CFT). On the other hand, the real world does not seem to fit nicely into models we can solve exactly. Particularly when it comes to designing experiments that will test our understanding of the universe, it helps to have a more intuitive idea of what new physics will look like. More generally, some of the most important ideas in physics were not connected by a series of mathematical arguments to what we knew already, but required a jump into the unknown. The development of the theory of inflation is a great example: the theory was essentially created as needed to understand the model after it was written down by Guth[8] and then improved by Linde. In fact, it is now 40 years

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[8]On several occasions, I have heard people say that they would not have published the paper if they were in
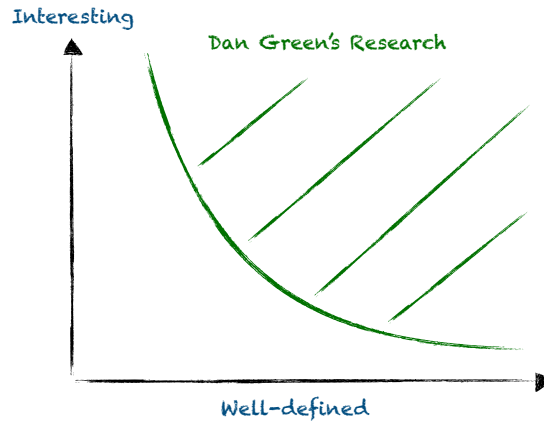
Figure 3: My research: I can only tolerate so much hand waving, but for really interesting problems it is worth it. On the opposite end, I don't have the patience to find exact results just for the sake of solving something exactly.

later and putting the predictions of inflation on the same footing as QFT in flat space remains an important open problem.

Research in theory benefits from a diversity of style, we even when one approach or the other may seem at face value like the necessary strategy. As a concrete illustration, Krizhnits and Linde explained why electroweak symmetry should be restored at high temperatures without having a rigorous QFT derivation. Weinberg, in his characteristic style, came along later and solved the QFT problem in complete detail, confirming Krizhnits and Linde's original insight (as did others around the same time). Theoretical physics is a balance of physical insight, mathematical skill and creativity but there is no perfect blend that works for every problem. Some remarkable papers have all of these things in spades, but you can still do work of incredible importance with your own unique blend.

This perspective on problem solving is captured by Politzers "principle of the virtual guru" (which I learned via Howard Georgi TASI lectures on heavy quarks). The idea is that there are many different ways to be a great researcher, but some may be better suited from some problems than others. If you encounter a new kind of problem, imagine how a particular researcher, in their unique style, would have approached the problem and copy it. Like an artist, eventually you want to make your own style, but trying on other people's is a good way to see what works for you.

A parting thought: theoretical physics is a seemingly impossible balance between hubris and humility. We have the audacity to believe that we can understand every facet of the universe with a pen and paper, and yet nature has taught us the harsh lesson that even our most beautiful theoretical structures are not inevitable in the natural world. It is normal to struggle to find this balance for yourself, as you build both confidence and humility along the way. I hope this guide helps you on your journey.

Thanks to Daniel Baumann, Tim Cohen, and Anže Slosar for their input and suggestions.

Guth's shoes, because his model had holes. If your style of research implies that you wouldn't publish one of the most important papers of the last 50 years, perhaps you need to revisit your style.