

## [Transcript] Founders / #314 Paul Graham (How To Do Great Work)

I just finished listening to this entire episode and I mentioned in the episode that it was not expecting to do another Paul Graham episode, especially an episode on a single one of his essays. A few months ago, I spent three weeks reading and rereading all Paul Graham's essays. I did three episodes on them. It's episode 275, 276, and 277. But I suspected that this essay, How to Do Great Work, was something that was really special because the sheer amount of people that listened to this podcast that sent me this essay. And as soon as I started reading it, I got excited about it. And that's a good indication that I should be making an episode on it right now. And just one quick thing before we jump into the episode, I've made something that is exclusively for enthusiasts of founders, for people that completely understand the benefit of this kind of crazy intense studying of the great people that came before us and how valuable it is to actually apply what they learned to whatever it is that you and I are working on. If that is you, I highly recommend that you sign up for the Private Founders AMA Feed. I've been making short episodes every week based on questions that I get from other members. If you become a member, you'll be able to ask me questions directly. There's actually a private email address that you get access to in the confirmation email. I read every single one of these emails myself. I do not have an assistant look over them. I read every single one. The questions I get from these emails, I turn into short AMA episodes. So that actually allows other members to learn from the questions of other members. You can also add your name and a link to your website with your question so other members can check out what you're working on. That feature alone is worth the investment. So far, I've made 31 episodes. I plan on making several episodes every week. If you consider yourself an enthusiast of Founders Podcast, highly recommend that you become a member and you can join by using the link that's in the show notes of your podcast player, or by going to [founderspodcast.com](http://founderspodcast.com). How to do great work. If you collected lists of techniques for doing great work in a lot of different fields, what would the intersection look like? I decided to find out. The following recipe assumes you're very ambitious. The first step is to decide what to work on. The work you choose needs to have three qualities. Number one, it has to be something you have a natural aptitude for. Number two, you have to have a deep interest in it. And number three, it offers the scope to do great work. In practice, you don't have to worry much about the third criteria. All you need to do is find something you have an aptitude for and a great interest in. And right away, we get to one of Paul's first footnotes. He says, doing great work means doing something important so well that you expand people's ideas of what's possible. But there's no threshold for importance. That's something he's going to repeat many times in different ways throughout the essay. So just keep that idea in mind. There's no threshold for importance. It's a matter of degree and it's often hard to judge at the time anyway. So I'd rather people focus on developing their interests rather than worrying about whether they're important or not. Just try to do something amazing and leave it to the future generations to say if you succeeded. Back to the top of the essay, he left off, he says, all you need to do is find something you have an aptitude for and a great interest in. That sounds straightforward, but it is often quite difficult. When you're young, you don't know what you're good at. And some kinds of work that you end up doing may not even exist yet. It's funny because that's the same advice I went and I went and talked at career day at my daughter's school when she was in fourth grade. And I don't think the teachers liked it very much because I started the conversation

with don't worry about what your parents or your teachers think. Just find out what you're naturally interested in and become a learning machine. And so in my own way, I told a bunch of nine year olds that because I was like, listen, when I was your age, there was no such thing as podcasting. It didn't even exist. Back to what Paul said, the way to figure out what to work on is by working. If you're not sure what to work on, guess, but pick something and get going. You'll probably guess wrong some of the time, but that is fine. And this is an idea that you and I have talked about multiple times. You see it in these biographies over and over again. The act of finding your life's work for an entrepreneur usually requires that you're going to have to start more than one business. I'd have to go back to the episodes to see if I could even find somebody that got it right the first time. Maybe I think the closest example of this might be Mark Zuckerberg. Back to this. It's good to know about multiple things. Some of the biggest discoveries come from noticing connections between different fields. Develop a habit of working on your own projects. Do not let work mean something that other people tell you to do. And that is such an important sentence. Don't let work mean something other people tell you to do. That is the lived experience of most people, alive and dead. In fact, Paul has another essay called How to Do What You Love, where he says only a few hundred thousand, and his guess, a few hundred thousand people ever actually figured this out. Let me read, I'm going to just read an excerpt from that other essay real quick. He says, with such powerful forces leading us astray, it's not surprising that we find it so hard to discover what we like to work on. Most people are doomed in childhood by accepting the axiom that work equals pain. Those who escape this are nearly all lured onto the rocks by prestige or money. How many even discover something they love to work on? A few hundred thousand perhaps out of billions. And so I think that sentence hits even harder after you read the excerpt from his past essay. Don't let work mean something other people tell you to do. If you do manage to do great work one day, it'll probably be on a project of your own. What should your projects be? Whatever seems to you excitingly ambitious, as you grow older and your taste in projects evolves, exciting and important will converge, but always preserve excitingness. I love that maxim, always preserve excitingness. There's a kind of excited curiosity that's both the engine and the rudder of great work. It will not only drive you, but if you let it have its way, will also show you what to work on in a small degree. This is why I wasn't expecting to do this essay as an episode. In fact, I'm reading two other books right now that I thought I was going to do before this. But so many, first of all, so many people that listened to Founder sent me this essay and then I started reading it and this is exactly what happened. I feel like I have goosebumps right now. I get excited and I'm like, nope. It completely cleared everything off. There's nothing else. So when I, people ask like how do I pick books or do I have a schedule or something like that for what I'm going to work on? I just go to a bookshelf where I have tons of unread books. It's like, what am I most excited to learn about right now? And I start reading Paul's essay and I'm like, oh, there's the answer. I'm most excited about this essay right now. And so therefore I use that. He just said, it's the both the engine and the rudder of great work. It will not only drive you, but if you let it have its way, it'll also show you what to work on back to Paul's essay. What are you excessively curious about? Curious to a degree that would bore most other people. This is what you're looking for. So again, I have another example of this. So people assume because I read a lot of books that I must have great recommendations on what to read, but time and time again, I actually recommend a book and then I'll hear back from that person

like, oh, that was how did, how the hell did you read that? Like that book was too boring. And this happened so many times. I'm like, oh my God, I didn't even understand that my threshold for quote unquote boring books is so much higher than most other peoples because those books weren't boring to me. I was reading them because I had to satisfy whatever curiosity I had, right? So you just said, what are you excessively curious about? Curious to a degree that would bore most other people. That is what you're looking for. Once you found something you're excessively interested in how I'm not, I'm a few paragraphs into the essay. Like he's telling you is like, follow your interest. What Charlie Munger says, follow your natural drift. What are you interested in? Go do that. Once you found something you're excessively interested in, the next step is to learn enough about it to get you to one of the frontiers of knowledge. And then once you're at a frontier of knowledge and whatever field that you're excessively interested in, that's where you're going to start to learn and you're actually going to start to see, you'll learn enough so you can actually see gaps. And so this is his point. Many discoveries have come from asking questions about things that everyone else took for granted. If the answer seems strange to you, right? So you're working in something that you're excessively interested in, you're doing the work necessary. Because again, you're not, he said, this essay is not for you if you're not ambitious. If you're just trying to like skate through life, there's no point in reading it. But he's assuming, okay, you're following, you're excessively interested, you're willing to, you want to do great work, which then means you'll do enough work to get to the frontier of knowledge. And then once you get there, you'll notice the gaps and you're like, oh wait, other people are taking this for granted. If the answer seems strange to you, so much the better. Great work often has a tincture of strangeness. And then he has advice on what you do next. Boldly chase outlier ideas, even if other people are not interested in them. In fact, especially if they aren't, if you're excited about some possibility that everyone else ignores, and you have enough expertise to say precisely what they're all overlooking, that's as good of a bet as you will find. And then this next paragraph, this last sentence in this next paragraph, which I'll get to in one second, is exactly why, you know, Paul Graham's essays, all of them are worth reading and rereading. And you've seen this through my own actions. Go back to what, 275, 276, 277. It's been like three weeks reading and rereading all of his essays. And like, I've read them a bunch for years. And you do this because he's able to compare ideas. And in this case, he's focused on great work in different fields. Entrepreneurship, arts, mathematics, whatever it is. So he says four steps. Well, number one, choose a field. Number two, learn enough to get to the frontier. Number three, notice the gaps. Number four, explore promising gaps. And this is the punchline of the paragraph, which I absolutely love. This is how practically everyone who's done great work has done it, from painters to physicists. Step two and four will require hard work. I'll pause there. There's a good thing that Paul has another great essay called How to Work Hard. So I will leave that link down below if you haven't read it. I'd obviously read this essay first and then follow all the links for his other essays, but how to work hard. It's like, should be in the footnote of this essay. So it says that steps two and four require hard work. It may not be possible to prove that you have to work hard to do great things, but the empirical evidence is on the scale of the evidence for mortality. So he's essentially, it's like a short bet is what he's telling us. That is why it's essential to work on something you're deeply interested in. Interest will drive you to work harder than mere diligence ever could. And we've seen this over

and over again, go back to Neval Ravakan, go back to Michael Jordan, Edwin Landle, Steve Jobs, he'll say the same thing, the fine work that feels like play. They're so intensely and deeply interested in it that they have no choice. And you just wanted to spot where this strange convergence happens, where you're working all the time and it feels like you're never working. And then he brings up the fact that you can't learn this without doing it. Let's talk a little bit more about the complicated business of figuring out what to work on. The main reason it's hard is that you can't tell what most kinds of work are like except by doing them. You may have to work at something for years before you know how much you like it or how good you are at it. And in the meantime, you're not doing and that's not learning about most other kinds of work. And I love just sitting there reading and rereading that paragraph because the sole point, you know, it's like, listen, we're going to try to do great work. We're going to try to find our life's work. This, you can't expect this to be easy because if it was easy, billions and billions of people in the past and currently wouldn't have failed to do it. And he hits at one of the most important part because you don't know what it's like until you start doing it. And that means you might be working at something for several years before you know how much you like it or how good you are at it. And in the meantime, you're not learning or doing other kinds of work. So you could go down a path and multiple paths even that are just a dead end all the way until you're in the grave. And I think this is why I think the essay is so important that I would dedicate an entire essay to it to like really stop and think and read and reread because this is not easy. And we only get one shot at life. So then he goes into some of the things like why is this so complicated? Like there's certain structures and institutions that society has created that doesn't make this easier. And if you read, I think I have notes on this later, but Paul's essay is a lot. We'll just kind of like, he's like constantly poking at the way we like our educational systems are set up. You can tell he's just not a fan and he's not hiding the fact that he's not a fan. So he says, educational systems in most countries expect you to commit to a field long before you could know what it's really like, which is madness. This doesn't make any sense. It would be better if they at least admitted, if they admitted that the system not only can't do much to help you figure out what to work on, but it is designed on the this is so crazy. This is the great thing about his writing because it's so like refined. Like he talks about his editing process all the time. I think he spent like half a year writing this essay, if I'm not mistaken. And so these ideas just get right into your brain. But listen to this. This is madness. But it is designed on the assumption that you'll somehow magically guess as a teenager. If I go back and think what I was like as a teenager, and if you do the same, like how many ideas did you believe are true? And you thought were valuable when you were teenagers, you still believe. I was thinking about this earlier. I was like, maybe one that reading books is probably a good use of my time, right? And reading the right books is probably a good use of my time. The idea is like, hey, we're going to pick what we're going to do. And we're going to figure it out and not even figure out guess, the magically guess is a great way to put it as a teenager. And then this is what I was alluring to like, why spend so much time thinking about like, don't glaze over this essay, read it, think about it. They don't tell you, but I will. When it comes to figuring out what to work on, you're on your own. And so then he gets into the importance of the only going to figure this out, you have to take actions, do not be passive. What should you do if you're young and ambitious, but don't know what to work on?

What you should not do is drift along passively, assuming the problem will solve itself. You need to take action. When you read biographies of people who've done great work, it's remarkable how much luck is involved. They discover what to work on as a result of a chance meeting or by reading a book that they happen to pick up. I need to pause here. This has been on my mind a ton. First of all, obviously, what Paul said there is completely right. You can read a lot of biographies, you just read people who've done great work, which is this entire podcast. It's remarkable how much luck is involved. Just look at the last few weeks. We don't have to go into the last seven years. How is it possible that both James Cameron and Christopher Nolan both watched the movie 2001 and Space Odyssey when they were kids and both arrived at the same conclusion that I can do that, that I can make movies? Maybe even the best example recently, you got to go back and listen to the Mark Twain episode. I think it's like episode 312 or something. This is the whole thing because it's about his young life. It's really about this essay. He's a young, ambitious person, doesn't know what to work on. He literally turns from Samuel Cummins to Mark Twain. Towards the end of the episode, in fact, I'll tell you, I think it's like 50-minute mark because I have the transcript in front of me. I'm going to read from this. The series of events that had to happen for Mark Twain to finally find his path, listen to the whole episode, but in case you haven't heard it, I'm just going to give you this like two paragraphs. This is me reading from transcript from the Mark Twain episode. And so I say, let's look back at what is taking place. First, he thinks he's going to be a cocaine dealer. He travels down the river, stumbles upon the best job that he thought he would have forever, which is the Mississippi Riverboat Steamboat pilot, which is kind of crazy that Mark Twain wasn't planning on being a writer and a lecturer. It's like, no, the best job ever is the Steamboat captains or Steamboat pilots. Then something completely out of his control, the Civil War, comes and causes him to flee to go out west and have all these series of adventures. He turns from Sam Clemens into Mark Twain along the way. He meets a bunch of people along the way that give him fantastic advice and change the trajectory of his life. He experiences unbelievable highs and then at the very bottom, unbelievable lows where he literally has a gun to his head. He is contemplating suicide. Then right after that, he does something smart, puts down the pistol, picks up the pen, right? He finds the opportunity of his life, writes what he thinks is just a silly story. That story goes viral. That story then creates all these other fans and people that love Twain and want to help him. He turns that into a trip to Hawaii. He meets another one of his fans who happens to be a very influential diplomat who leads him to the greatest journalistic scoop he has of all time. That scoop leads him to his lecture career, which further enhances his public profile, which then leads him to this 23 week trip in Europe, which in turn is going to lead to the source material for his first great book, the first publishing success that he ever has, which then in turn changes his life forever. That example from Mark Twain is a microcosm of exactly what Paul Graham is talking about here. What you should do if you're young and ambitious but don't know what to work on. Mark Twain was young and ambitious. He had no idea what to work on. He put pen to paper. He took action as another way to think about that, right? When you read biographies of people who've done great work, it's remarkable how much luck is involved. They discover what to work on as a result of a chance meeting or by reading a book that they happen to pick up. At the end of that paragraph, Paul has

a fantastic footnote. There are many reasons curious people are more likely to do great work, but one of the most subtle is that by casting a wide net, they're more likely to find the right thing to work on in the first place. Back up top, when in doubt, optimize for interestingness. Fields change as you learn more about them. A field should become increasingly interesting as you learn more about it. If it doesn't, it's probably not for you. Do not worry if you're interested in different things than other people. The stranger your tastes are in interestingness, the better. Strange tastes often are strong ones and a strong taste for work means you'll be productive and you'll be more likely to find new things if you're looking where few others have looked before. One sign that you're suited for some kind of work is when you like even the parts that other people find tedious or frightening. So that's another, he's reintroducing that idea, right? Find work that feels like play. If you're interesting, the work will pull you in. Fields are not people. You do not owe them any loyalty. If in the course of working on one thing, you discover another that's more exciting, do not be afraid to switch. If you're making something for people, make sure it's something they actually want. The best way to do this is to make something you yourself want. Write the story you want to read. Build the tool that you want to use. This should follow from the excitingness rule. So what is he saying there? Make what you are most excited about. That's the main message of what he's trying to teach us. He also has this, I think I talked about this before, but in other essays, he couldn't figure out, like he would notice a pattern when he's doing like office hours with founders at Y Combinator, that the ones that were kind of lost or like kind of not doing well, they weren't even using their own product. They were building a product that they thought other people might want or trying to solve a problem that they didn't really understand. And that's a big problem. If you're not, if you don't love your own product, if you're not using your own product, there's nothing like to orient yourself around. So it's like, why don't you just write the story that you want to read? Build a tool that you want to use. I mentioned this case explicitly because so many people get it wrong. Instead of making what they want, they try to make what some imaginary, more sophisticated audience wants. And once you go down that route, you're lost. So I kind of just ran over the point that he was making there. There are a lot of forces that will lead you astray when you're trying to figure out what to work on. Pretentiousness, fashion, fear, money, politics, other people's interests. In other words, like distractions that get you off your true authentic path. But if you stick to what you find genuinely interesting, you'll be proof against all of them. If you're interested, you're not astray. And so when I read that the first time, I said, that may be my favorite paragraph so far. And really, because that line, if you stick to what you find genuinely interesting, you'll be proof against all of them. If you're interested, you're not astray. This idea will be very familiar to you and I. It's why my number one recommendation is still James Dyson's first autobiography against the odds. Following your interests usually means following them past rejection and failure. It does take a good deal of boldness. While you'll need boldness, you usually don't need much planning. In most cases, the recipe for doing great work is simply work hard on excitingly ambitious projects and something good will come of it. The trouble with planning is that it only works for achievements you can describe in advance. You can win a gold medal or get rich by deciding as a child and then tenaciously pursuing that goal. But you can't discover natural selection that way. I think for most people who want to do great

work, the right strategy is not to plan too much. At each stage, do whatever seems most interesting and gives you the best options for the future. I call this approach. Let me pause. I need to repeat that. At each stage, do whatever seems most interesting. He's repeated that to us, what, 15 times maybe so far in different ways. At each stage, do whatever seems most interesting and gives you the best options for the future. I call this approach staying upwind. This is how most people who've done great work seem to have done it. Even when you found something exciting to work on, working on it is not always going to be straightforward. There will be times when some new idea makes you leap out of bed in the morning and get straight to work. But there will also be plenty of times when things are not like that. You don't just put out your sail and get blown forward by inspiration. There are headwinds and currents and hidden shoals. So there's a technique to working just as there is to sailing. For example, while you must work hard, it's possible to work too hard. And if you do that, you'll find you get diminishing returns. Fatigue will make you stupid and eventually even damage your health. The point at which work yields diminishing returns depends on the type. Some of the hardest types you might only be able to do four or five hours a day. Ideally, those hours will be continued continuous. To the extent you can, try to arrange your life so you have big blocks of time to work in. Edwin Land said it best. I'll never stop repeating this quote because it's one of my favorites. My whole life has been spent trying to teach people that intense concentration for hour after hour can bring out in people resources. They didn't know they had. Paul Graham says ideally those hours will be continuous. To the extent you can, try to arrange your life so you have big blocks of time to work in. It is usually a mistake to lie to yourself if you want to do great work. But this is one of the rare cases where it isn't. When I'm reluctant to start work in the morning, I often trick myself by saying, I'll just read over what I've got so far. Five minutes later, and I found something that seems mistaken or incomplete, and I'm off and running. So he's talking about tricking himself into working even when you don't feel like it. And he talks about the similar techniques work for starting new projects. And he says, it's okay to lie to yourself about how much a project will entail. For example, lots of great things began with someone saying, how hard could it be? He's absolutely right on that. How many times? I've lost count of how many times I've read in these biographies. After this is done, they'd be like, I would have never done this to begin with. I wouldn't have even tried it if I actually knew going into it how hard or difficult or painful it would wind up being. So there is some kind of like benefit to, I don't want to call it like blissful, I guess blissful ignorance, maybe is the way to think about that. Try to finish what you start, though even if it turns out to be more work than you expected. Finishing things is not just an exercise in self-discipline. In many projects, a lot of the best work happens in what was meant to be the final stage. So I got to pause there because I had a weird idea that came to mind that was prompted by reading that. So it's like, listen, you just try to finish what you start, even if it's more work than you expected, finishing things is not just an exercise in self-discipline. In many projects, a lot of the best work happens in what was meant to be the final stage. I would say that's true for people's lives too. I was trying to figure out when you're reading these stories, why do so many, you're not going to find many people, I'm talking about in entrepreneurship, not scientific discovery or something, you're not going to find many people that make their best business or their best product when they're like 20 or like 25. And this happened so much, I was like, why do so many people wind up doing their best work many decades into their career? I think the obvious answer would be like, I was thinking about this is like, okay, well, yeah, it's like

they've had three decades of as an entrepreneur practice, like they started a bunch of businesses, now they know their industry better, they just understand the shape of the work and what they want to do. And I think that is true. But one thing that I just can't let go of is, I think a key to doing truly great work is building a business that's authentic to you, to your true self. And what I realized is like, oh, yes, they have more experience, but they know themselves better. This process of self discovery of like really figuring out, okay, who am I truly? Not am I imitating what's around me? Like, who am I truly? What am I in true interest? And like, what do I actually want to do with my life? That process is usually take many, many decades. So it's like, yeah, they had a lot more experience and practice as an entrepreneur, but they also know themselves

way better multiple decades into the career than when they started. And I think there's a like a relationship between those two things back to this essay. Another permissible lies exaggerate the importance of what you're working on, at least in your own mind. If that helps you discover something new, it may turn out not to have been a lie at all. That is a fascinating idea that it might be a good idea to lie to yourself, exaggerate, right, the importance of what you're working on, at least in your own mind. If that helps you discover something new, it may turn out to not have been a lie at all. And so the footnote on that idea is he says, this is an idea I learned from this book called a mathematician's apology. And he says, I recommend to anyone ambitious to do great work in any field. And he follows that interesting idea up with another one. This is Paul's theory on per project procrastination, per project procrastination. Since there are two senses of starting work per day and per project, there are also two forms of procrastination per project procrastination is far more dangerous. You put off starting that ambitious project from year to year because the time isn't quite right. One reason per project procrastination is so dangerous is that it usually camouflages itself as work. You're not just sitting around doing nothing, you're working industriously on something else. You're too busy to notice it. And then I love his simple guide on how to catch if we're actually doing this. The way to beat this is to stop occasionally and ask yourself, am I working on what I most want to work on? When you're young, it's okay if the answer sometimes no, but this gets increasingly dangerous as you get older. Just ask yourself, am I working on what I most want to work on? Great work usually entails spending what would seem to most people an unreasonable amount of time on a problem. I have to pause there.

When I read that sentence, immediately becomes the mind when Steve Jobs said that everything great

has a shared ingredient time. Paul says great work usually entails spending what would seem to most

people an unreasonable amount of time on a problem. Great work happens by focusing consistently on something you're genuinely interested in. When you pause to take stock, you'll be surprised on how far you've come. The reason we're surprised is that we underestimate the cumulative effect of work. Oh, another great paragraph. This is going to be an essay I'm going to read multiple times throughout my life. I hope you read the entire thing when you're done listening to this. Writing a page per day doesn't sound like much, but if you do it every day, you'll write a book a year. That is the key consistency. People who do great things don't get a lot done every day. They get something done rather than nothing. And if you do work that compounds, you'll get exponential growth.



The trouble with exponential growth is that the curve feels flat in the beginning. It isn't. It is still a wonderful exponential curve, but we can't grasp that intuitively. So we underrate exponential growth in early stages, something that grows exponentially can become so valuable that it's worth making an extraordinary effort to get started. This is so good. Okay. Something that grows exponentially. Check this out. I'm going to tell you something that I was thinking about when I was reading this. Something that grows exponentially becomes so valuable that it's worth making an extraordinary effort to get started. So I listen to podcasts with my daughter. She's 11. And right now she loves Taylor Swift. So we were listening to the Acquired podcast episode on Taylor Swift. Acquired is made by my friends Ben and David. And I can't remember if Ben or David said it, but one of them said that Taylor Swift early in her career, right? She personally responded to like 25,000 messages from fans on the blogging platform Tumblr that was like popular back in the day when she was just starting out. So it's like this idea. It's like 25,000 personal responses to the small fan base that she had. It had to feel at that time to her like the curve is going to feel flat to her because it's still the beginning. Now you fast forward what 15 years later or maybe 20 years later, she might have been like 15 when she was doing it. So it might be 20 years ago. And now she's selling out stadiums back to back to back like multiple days. Like I was just looking her show in LA. I think she's doing the stadium there like five nights in a row or something like that. But this idea something that grows exponentially becomes so valuable that it's worth making an extraordinary effort to get started. This is something that a young Taylor Swift understood intuitively. It's amazing. Work doesn't just happen when you're trying. There's a kind of undirected thinking you do when walking or taking a shower or lying in bed that can be very powerful. By letting your mind wander a bit, you'll often solve problems you were unable to solve by frontal attack. And again, this has just been top of mind. I've heard of this a bunch, but like Christopher, I've been thinking about what like Christopher Nolan and David Ogilvy, just because it's been top of mind because I've made episodes about them both recently. It sounds like them. If you read about them like this is, they think like this. They use this. You have to be working hard in a normal way to benefit from this phenomenon though. You can't just walk around daydreaming. The daydreaming has to be interleaved with deliberate work that feeds its questions. They would tell you that you need to give your mind time to think, time to wander. I think is the term that Paul's about to use here, which is fantastic. When you let your mind wander, it wanders to whatever you care about most at that moment. So avoid the kind of distraction that pushes your work out of the top spot or you'll waste this valuable type of thinking on the distraction instead. There is one exception to this. Do not avoid love. Consciously cultivate your taste in the work done in your field until you know which is best and what makes it so. You don't know what you're aiming for. And then on the very extreme end, taste is actually a moat. And that is what you're aiming for because if you don't try to be the best, you won't even be good. So I need to back that up. I'm going to read that together because I interrupted this. He's gonna tell us like you must aim to be the best. Consciously cultivate your taste in the work done in your field until you know which is the best and what makes it so. You don't know what you're aiming for. And that is what you're aiming for. Because if you don't try to be the best, you won't even be good. This observation has been made by so many people in so many different fields that it might be worth thinking about why it is true.

Don't try to work in a distinctive style. Just try to do the best job you can. And if you do, you won't be able to help doing it in a distinctive way. Style is doing things in a distinctive way without trying to. Trying to is affectation. So let's define affectation because he's going to use this word a couple of times. It is behavior, speech, or writing that is artificial and designed to impress, which is the opposite of what really he's saying. It's like follow your natural drift, let what you're most interested in be your guide. And now he's saying some people do this and they're like putting on a show for other people. None of this is going to work if it's inauthentic to you. You adopt an impressive but fake persona. And while you're pleased with the impressiveness, the fakeness is what shows in the work. And I think all these ideas feed on each other. Follow your natural interest, it's authentic to you. And therefore, the work pulls you into it. And then you do it for a long time. So you get better at it as a byproduct. All these things work very well together. So again, you adopt an impressive but fake persona. And while you're pleased with the impressiveness, the fakeness is what shows in the work. If you succeed at an ambitious project, you're not a nobody. You're the person who did it. So just do the work and your identity will take care of itself. And so it's like a negative role. You could say, hey, avoid being fake, whatever the case is. But he's like, well, how do you actually express this idea positively? How would you say what to be instead of what not to be? And he says the best answer is earnest. And so we're going to define earnest, resulting from our showing sincere and intense conviction. I love that definition, intense conviction. The core of being earnest is being intellectually honest. You're trying to see more truth than others have seen so far, key to doing great work. And how can you have a sharp eye for truth if you're intellectually dishonest? Another subtle component of earnestness is informality. It means focusing on what matters instead of what doesn't. What formality and affectation have in common is that as well as doing the work, you're trying to seem a certain way as you're doing it. That's one reason nerds have an advantage in doing great work. They expend little effort on seeming anything. Nerds have a kind of innocent boldness. This is hilarious. Nerds have a kind of innocent boldness that's exactly what you need in doing great work. It is not learned. Be the one who puts things out there rather than the one who sits back and offers sophisticated sounding criticisms of them. It's easy to criticize is true in the most literal sense. And the route to great work is never easy. And another thing like who cares what the critics say, the loudest boozes always come from the cheapest seats. There's a theme that runs through a lot of the great founders. And it's the fact that they believe that their opinion on what they're working on, their opinion is greater than every other person's opinion around them regarding what they're working on. And they're too busy building their empires and making great products and creating a fortune for themselves and their family to stick their head up and start talking shit about what other people are doing. Paul's other essay, Life is Short, which I think you can read in like five or 10 minutes, perfectly pairs with this essay, because you realize outside of taking care of your health, working on your mission or being with your tribe, nothing else matters. Stop wasting your time. Focus on what you want to do. It's so hard. The route to great work is never easy. I doubt it would be possible to do great work without being earnest. Another sign of people that can do great work, they are willing to redo things. You may have to throw things away and redo them. You have to be willing to. When there's something you need to redo, status quo bias and laziness will combine to keep you in denial about it. Have the confidence to cut. Do not keep something that doesn't fit just because you're proud of it or because it costs you a lot of effort. In some

kinds of work, it's good to strip whatever you're doing to its essence. The result will be more concentrated. You'll understand it better. I don't even know why, but when I got to this section, I was thinking about this story I read in one of the biographies of Steve Jobs. They had spent so much time on this concept for the Apple Store. This is before it was like open to the public. I forgot who the person was, but the guy helping Steve on this. They were working on this for maybe I don't know, like a year or more than that. They had built this prototype of an Apple Store in this warehouse somewhere. Steve picks them up to go take a look at it. The guy had realized the day before, he was like, oh my God, we have this set up the incorrect way. We should have it set up by what the devices do or whatever the case was. The important part was Steve's reaction. The guy was like, shit, we put all this time and effort into it. We need to destroy everything we did and start over because this other way is a better idea. Steve flipped his lid, started getting upset, yelling. Then he gets silent because he has cancer. He's very sick when this is happening. He's just like, I don't know if I have the energy to do this from scratch again. Then it's like complete quiet. They're riding in the car in complete silence. They get to the prototype where all the rest of the people, the other employees at Apple are waiting for them. And then Steve quietly says, he's right. Do what he says, start from scratch. We're doing it all over again. You have to throw things away and redo them. You have to be willing to. Another sign of somebody that's doing great work. It looks easy. They make it look easy. Some of the best work will seem like it took comparatively little effort because it was in a sense already there. It didn't have to be built just seen. It's a very good sign when it's hard to say whether you're creating something or discovering it. When you're doing work that could be seen as either creation or discovery, err on the side of discovery. Try thinking of yourself as a mere conduit to which the ideas take their natural shape. Strangely enough, one exception is the problem of choosing a problem to work on. This is usually seen as a search, but in the best case, it's more like creating something. In the best case, you create the field in the process of exploring it. And I love what he says here next because I feel the same way. I never like the term creative process. It seems misleading. Originality isn't a process, but a habit of mind. Original thinkers throw off new ideas about whatever they focus on. And then he ties that idea to the idea he said earlier. It's like, don't sit there passively, man. You've got to work. Like you're going to learn what field you're working on and how to do great work by doing it. You're much more likely to have original ideas when you're working on something. Original ideas don't come from trying to have original ideas. They come from trying to build or understand something slightly too difficult. You'll have more new ideas if you explore lots of different topics. Do not divide your attention evenly between many topics though. It sounds almost like a contradiction, but it's not, because he's going to explain that. You'll have more new ideas, right? If you explore a lot of different topics, but do not divide your attention evenly between many topics though, or you'll spread yourself too thin. You want to distribute it according to something more like a power law. Oh my goodness. I love how he ties everything together. Be professionally curious about a few topics and I'd be curious about many more. Curiosity and originality are closely related. Having new ideas is a strange game, because it usually consists of seeing things that were right under your nose. Once you've seen a new idea, it tends to seem obvious. When an idea seems simultaneously novel and obvious, it's probably a good one. Then he gets into like, why is this such a strange game? Another way to say it's like, why is this president such a small percentage of humanity? Seeing the new idea usually requires

you to change the way you look at the world. It's admitting to yourself that you have a broken model of the world, right? Broken models of the world leave a trail of clues where they bash against reality. Most people do not want to see these clues. It would be an understatement to say that they're attached to their current model. I think underneath all this, the main cause is because people are terrified of change, which is strange, because we're living in a world where the only constant thing is change. In fact, I was reading this essay and then going through a bunch of other quotes that Paul collects on his website that I think are excellent, and there's a line that came to mind when I got to this section. I said, change breaks the brittle. Back to the essay. The other thing you need is a willingness to break rules. If you want to fix your model of the world, it helps to be the sort of person who's comfortable breaking rules. Few understand the degree of rule breaking required, because new ideas seem much more conservative once they succeed. That ties to what he said at the beginning of the section. The fact that it's fascinating that once we discover a new idea, it tends to seem obvious after the fact. They seem perfectly reasonable once you're using the new model of the world. They brought with them.

The ideas bring the models, not the other way around. That's fascinating, but they didn't at the time. It took the greater part of a century for the heliocentric model to be generally accepted, even among astronomers, because it felt so wrong. If you think about it, a good new idea has to seem

bad to most people, or someone would have already explored it. Back to breaking rules. There are two ways to be comfortable breaking rules. To enjoy breaking them or to be indifferent to them. I call these two cases being aggressively and passively independent-minded. The aggressively independent-minded are the naughty ones. Rules don't merely fail to stop them. Breaking rules gives these people additional energy. For this sort of person, delight at the sheer audacity of a project sometimes supplies them enough activation energy to get it started. The other way to break

rules is to not care about them, or perhaps not even know they exist. This is why novices and outsiders often make new discoveries. Their ignorance of a field's assumptions, or a field's rules, right, acts as a source of temporary, passive, independent-mindedness. And so it goes back to his original point that deciding what to work on is so important that a lot of people are getting distracted because they like working on fashionable problems. So Paul Graham puts this in an even better way, like the actual value is going to accrue to things that are not top of mind. He says unfashionable problems are undervalued. Working on an unfashionable problem can be very pleasing.

There's no hype or hurry. Opportunists and critics are both occupied elsewhere. And that's not the same as doing the opposite of what everybody else wants to do, right? It's the most common type of overlook problem is not explicitly unfashionable in the sense of being out of fashion. It just doesn't seem to matter as much as it actually does. And the only way to find this is you're following your natural interest. Again, everything ties together. How do you find these? By being self-indulgent, by letting your curiosity have its way and tuning out, at least temporarily, the little voice in your head that says you should only be working on important problems. And so it's important to note that the word important in that sentence has quotation marks. It's just like really, it's the opinion of other people, right? By being self-indulgent, by letting your curiosity have its way and tuning out that little voice in your head

that says you should only be working on quote unquote important problems. And so I want to pause there because I think this is so important. So in addition to reading this essay, I saw that Paul's wife and his partner on Y Combinator, Jessica said that she's going to reread every year.

I think she said every year she wants to reread this essay, how to do great work.

And she also rereads Paul's essay, Life is Short Every Year. And so when you read them back to back,

you realize, oh, wow, that's like really good. They go well together. And what was fascinating is there is this line in the Life is Short essay where he says the things that matter aren't

necessarily the ones people would call quote unquote important. He uses important in quotation marks again. So this ability to literally like not care to live an authentic life, ask yourself,

am I following my own interests? Is this what I want to work on? And if the answer is yes,

everybody else's opinion is irrelevant. And I think as you get older and you actually know

yourself, that becomes like easier to do. But it's obviously something that is not common,

right? The ability to do is not common because it comes up in these essays over and over again.

So much so that Paul has to explicitly tell the reader, give advice, hey,

that even if you hear that in like the back of your mind, that's fine, like shut it up.

Because it's taking you off the path that you're meant to be off. That little voice in your head

that says you should only be working on quote unquote important problems. Ignore it. Why?

Because most people that work on important problems and do great work, those problems were

not seen as important to the external world at the time they started working on them. And you

only know that if you go back and you study the creation of all these new industries and read

all these other biographies, it pops up over and over again. The example I use over and over again

is the fact that Henry Ford is sitting in Detroit in 1900 working on an internal combustion engine

in his fucking kitchen. And everybody around him saying, what are you doing? You're wasting

time. Cars are obviously going to be electric or they're going to be steam powered. His boss

tells him because he's working at the electric company as an engineer. If you keep up your

experimentations with your internal combustion engine, you either have to choose between the

engine or your job. And Henry Ford is like, cool, I'm choosing the engine. He followed his natural

interest with the entire outside world was telling him, this isn't important enough, this little stupid

thing that you're doing, focus on the big thing. Obviously electricity is the biggest invention.

Obviously all the cars are going to be electric. Fast forward 19 years from when they're having

that conversation, Henry Ford owns 100% of the Ford Motor Company has created and

manufactured

a car that made over 15 million people's lives better. After the 15th million model T rolled

off the assembly line, did that guy's opinion about what Henry Ford should be doing matter?

No, it didn't matter at number 15 and it didn't matter when he said it. And Paul describes this

perfectly says, try asking yourself, if you were going to take a break from quote unquote serious

work to work on something just because it would be really interesting, what would you do? The

answer is probably more important than it seems. What might seem to be merely the initial step

deciding what to work on is in a sense the key to the whole game. I don't even know how far we are

into this essay, but I think it's obvious why so many people that listen to this podcast sent

this to me. This is perfect. Few grasp this, the fact that picking what to work on is the key to

the whole game, right? Few grasp this. People think big ideas are the answers, but often the real

insight was in the question. Sometimes you carry a question for a long time. Great work often comes

from returning to a question you first notice years before and you could not stop thinking about. It is a great thing to be rich in unanswered questions. And a way to know that if you have a good unanswered question, he says the best questions grow in the answering. You'll notice a thread and then try pulling on it. And it just gets longer and longer. So don't require a question to be obviously big before you try answering it. It goes back to the Henry Ford just messing around following his own interest in his kitchen, right? You can rarely predict that. It's hard enough to even notice the thread, let alone to predict how much it will unravel if you pull on it. It is just better to be promiscuously curious to put a little bit on a lot of threads and see what happens. Big things start small. The initial versions of big things were often just experiments or side projects or talks which then grew into something bigger. So start lots of small things. Being prolific is underrated. The more different things you try, the greater the chance of discovering something new. Understand though that trying lots of things will mean trying lots of things that do not work. You cannot have a lot of good ideas without also having a lot of bad ones. And so I love the fact that Paul says being prolific is underrated. A few days before I read this, my friend, Anil, who's the founder of Meteor, he actually sent me his post. He, on his personal website, Anil collects examples, like historical examples of people that he calls outlandishly prolific. I'll leave the link down below in case you want to look at the list. It's fantastic. Air on the side of starting, which is easier when starting means starting small. Those two ideas fit together like two puzzle pieces. So air on the side of starting and then it's easier to start when it means starting small. How do you get from starting small to doing something great by making successive versions? Great things are almost always made in successive versions. You start with something small and evolve it. And the final version is both clever and more ambitious than anything you could have planned. See what he just did there? We're deep into this essay now. And now he finds ways to take ideas that he referenced, you know, maybe, I don't know, 5,000 words ago. This idea is like, don't spend too much time planning, get in, you'll start to learn more, and then he ties everything together. You're following your natural interests, you're having a bias to action, you're starting small, you're evolving it over time. And as a result, the final version is both clever and more ambitious than anything you could have planned. Begin by trying the simplest thing that could possibly work. Surprisingly often, it does. Do not try to cram too much new stuff into any one version. Evolve instead. And so there's a few times in the essay where he'll talk about, like, the difference between, like, what would you do if you're young or are a little bit older? I like how he describes this. Use the advantages of youth when you have them. And the advantages of age once you have those. The advantages of youth are energy, time, optimism, and freedom. The advantages of age are knowledge, efficiency, money, and power. The old also have the advantage of knowing which advantages they have. I like that line. The young often have them without realizing it. The biggest is probably time. The young have no idea how rich they are in time. The best way to turn this time to advantage is to use it in a slightly frivolous way. To learn about something you don't need to know about, just out of curiosity, or to try building something just because it would be cool or to become freakishly good at something. So I need to repeat that because he's like, listen, I'm not going to tell you to waste your time. But listen, if you are young, you have no idea how rich you are in time. The best way to turn this time into an advantage is to use it in a slightly frivolous way, especially saying, like, to experiment, to like, what are my true interests? What are the things I want to work on? How am I going to find a path that I can actually do, like, would turn into me being able to do great work?

And at the root core, it's like, well, just follow your curiosity, follow your interests, which again, it's the main theme here, right? And so he's going to elaborate on this though, because I wanted to repeat that because then he goes into what he means. Spend time lavishly when you're young, but don't simply waste it. There's a big difference between doing something you worry might be a waste of time, and doing something you know for sure will be. And so the ability to spend time lavishly is something that only the young have, right? Now he talks about if you're a little older, you have another advantage. One of the most valuable kinds of knowledge you get from experience is to know what you don't have to worry about. The young know all the things that could matter, but not their relative importance. So they worry equally about everything when they should worry much more about a few things, and hardly at all about the rest. And a source of this confusion about, okay, well, like, they're worried about everything equally when it's like, just focus on the really important things and ignore everything else. So the source of the confusion goes back to this is the problem with school. It just fills your head with a bunch of nonsense. So everybody almost by default will arrive at adulthood with a head full of nonsense. And he says much of that nonsense left in your head is left there by schools. Schools induce passivity. The sooner you overcome this, the better. Schools also give you a misleading impression of what work is like. In school, they tell you what the problems are, and they're almost always solvable using no more than what you've been taught so far. In real life, you have to figure out what the problems are, and you often don't know if they're solvable at all. But perhaps the worst thing that schools do is they train you to win by hacking the test. You cannot do great work by doing that. So stop looking for that kind of shortcut. Do not skimp on the work itself. Don't think of yourself as dependent on some gatekeeper giving you a big break. Even if this were true, the best way to get it would be to focus on doing good work rather than chasing influential people. It's that idea that you should try to be so good that people can't ignore you. People new to a field will often copy existing work. There's nothing inherently bad about that. There's no better way to learn how something works than trying to reproduce it. Nor does copying necessarily make your work unoriginal. Originality is the presence of new ideas, not the absence of old ones. And then he goes into why you want to study all the great work that came before you, not just to work in like your failure industry. One of the most powerful kinds of copying is to copy something from one field into another. History is so full of chance discoveries of this type that it's probably worth giving chance a hand by deliberately learning about other kinds of work. You can take ideas from quite distant fields if you let them be metaphors. I'm going to repeat that. I love that. One of the most powerful kinds of copying is to copy something from one field into another. History is so full of chance discoveries of this type that it's probably worth giving chance a hand by deliberately learning about other kinds of work. You can take ideas from quite distant fields if you let them be metaphors. And then he goes into something that's repeated through history a lot, especially at the beginning of industries. They all seem all the super talented people seem to be within close physical proximity to one another. If a lot of the best people in your field are collected in one place, it's usually a good idea to visit for a while. It will increase your ambition and also by showing you that these people are human, increase your self-confidence. If you are earnest, you'll probably get a warmer welcome than you might expect. Most people, this is so, I love this idea. Most people who are very good at something are happy to talk about it with anyone

who's genuinely interested. If they're really good at their work, then they probably have a hobbyist interest in it. And hobbyists always want to talk about their hobbies. Seek out the best colleagues. That is like the one line maximum of this section, right? Seek out the best colleagues.

If you listened to the Mark Twain episode I referenced earlier, the best advice Mark Twain ever got that he followed for the remaining 44 years of his life was this American diplomat in Hawaii told him, you have great ability. I believe you have genius. What you need now is refinement of association. Seek companionship among men of superior intellect and character. This is like the punchline for me. Refine yourself and your work. Never affiliate with inferiors. Always climb. Paul Graham's way of saying that is seek out the best colleagues. Colleagues don't just affect your work though. They also affect you. So work with people you want to become like because you will. And he's going to go into why. Like great work happens in clusters. We just see this over and over again. In fact, it's not merely better but necessary. Judging from history, the degree to which great work happens in clusters suggests that one colleague often makes the difference between doing great work and not. We got to read that again. The degree to which great work happens in clusters suggests that one's colleagues often make the difference between doing great work and not. That's why you see so many of history's great founders. They repeat over and over again the importance of recruiting and finding a way to attract the very best people. Whether it's partners, employees, it doesn't matter. They all repeat this over and over again. And then Paul has this great way to tell if you've done this or not.

How do you know when you have sufficiently good colleagues? In my experience, when you do, you know. Which means if you're unsure, you probably don't. And then the essay takes a fascinating turn because he talks about maintaining your own inner monologue and your own morale.

Husband, your morale. It is the basis of everything when you're working on ambitious projects. You have to nurture and protect it. I've done multiple episodes and read multiple books about Arnold Schwarzenegger. And he says this over and over again, it's very dangerous when you start doubting yourself. He would very deliberately get away physically from people that would try to either tell him, Arnold, don't do that. It's impossible. You can't pot. How can you be an actor? No one can even understand you or you can't get famous for lifting weights and becoming a bodybuilder. You can't become the governor of California. What are you talking about? And so this idea, it's very dangerous. You're going to get enough negative input from the external world. You can't have any coming from yourself. It's very dangerous when you start doubting yourself. Paul is saying here, it's like, listen, the basis of everything when you're working on ambitious projects. He goes back to why managing your inner monologue, your own morale,

is so important. Morale compounds via work. High morale helps you do good work, which increases your morale and helps you do even better work. If you're not doing work, that can demoralize you and make it even harder to do good work. Since it matters so much for this cycle to be running in the right direction, it can be a good idea to switch to easier work when you're stuck. Just so you start getting something done. One of the biggest mistakes ambitious people make is to allow setbacks to destroy the morale all at once, like a balloon popping. You can inoculate yourself against this by explicitly considering setbacks a part of your process. Solving hard problems will always involve some backtracking. Never let setbacks panic you into backtracking more than you need to. It is not necessarily a bad sign if work is a struggle, any more than it's a bad sign



to be out of breath while running. And so then he turns the essay and ties this all back into morale. An audience is a critical component of morale. And so this will become obvious, I think, as you go through this, but he's talking about the people using whatever it is that you're making, right? What is it, who is, you're making great work. It's not like you're the only person, it can't be great if you're the only person who sees it. So somebody is using the tool or listening to what you're doing or whatever, reading it, whatever it is. If you're just starting out, a small but dedicated audience can be enough to sustain you. If a handful of people genuinely love what you're doing, that is enough. Avoid letting intermediaries come between you and your audience. That point is actually very important. Avoid letting intermediaries come between you and your audience. When I first thought about that, I thought about Taylor Swift, on Tumblr, talking directly to her fans, to the people listening to her music. There's nobody in between her and her audience also can be applied in the more traditional business product sense, where you study the career of Edwin Land, he's always making innovations because he's creating new technology, new patents, new science. And he was trying to sell into like, he wasn't connected to the end user. So he's trying to make headlights, automobile headlights, safer. But he's got to sell and convince people, like a handful of people, maybe in Detroit. And when he did that for many years and failed it, it's like, I'm never letting anybody else get in between me and the customer. So then he starts making consumer goods, right? That he can talk directly to the customer, which is his cameras. And I think that's a really important observation. Like, okay, well, I failed when I couldn't communicate directly with the end customer, because I was selling to car companies, as opposed to the people driving the car. But I succeeded wildly when I had a direct connection with the customers, the one actually buying my cameras. And so Paul says, avoid letting intermediaries come between you and your audience, it is so liberating to escape it, that you might be better off switching to an adjacent type of work, if that work will let you go direct. And then back to this idea, get away from the naysayers, flee the doubters, like you don't need them, the people you spend time with will also have a big effect on your morale. Seek out the people who increase your energy and avoid those who decrease it. Do not marry someone who doesn't understand that you need to work or seize your work as competition for your attention. If you're ambitious, you need to work. It's almost like a medical condition. So someone who won't let you work either doesn't understand you or does and doesn't care. And I think Paul answers the question, I've been asked this question in the past, like, do I think the people I cover and study in the podcast are like happier than the average person? And I was like, I don't know how to really answer that. And like, I kind of waffle. But this idea where it's like, oh, if you're ambitious, you need to work, it's almost like a medical condition. He says people who do great work are not necessarily happier than everyone else, but they're happier than they'd be if they didn't. That is a better way to think about it than I've ever come up with. In fact, if you're smart and ambitious, it's dangerous not to be productive. People who are smart and ambitious but don't achieve much tend to become bitter. And then he goes back into this reoccurring theme, do not be guided by prestige. It is okay to want to impress other people, but choose the right people. The opinion of people you respect is signal. Fame, which is the opinion of a much larger group you might or might not respect, just adds noise. The prestige of a type of work is at best a trailing indicator. If you do anything well enough, you will make it prestigious. The

## [Transcript] Founders / #314 Paul Graham (How To Do Great Work)

question to ask about a type of work is not how much prestige it has, but how well it could be done. And this might be the best line of the entire essay. Curiosity is the best guide. Your curiosity never lies, and it knows more than you do about what's worth paying attention to. If you asked an oracle the secret to doing great work, and the oracle replied with a single word, my bet would be on curiosity. This whole process is a dance with curiosity. Believe it or not, I try to make this essay as short as I could, but its length at least means it acts as a filter. If you made it this far, you must be interested in doing great work. And if so, you're already further along than you might realize, because the set of people willing to want to is small. The discoveries are out there waiting to be made. Why not by you? And that is where I'll leave it. I highly, highly, highly recommend reading the entire essay. It'll be linked down below. And I will also link to Paul's essay *Life is Short*, which I think should be read directly after reading this one. Thanks for listening this far, and I'll talk to you again soon.