July 2023 2023 年 7 月

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 by making it.
如果你收集了来自许多不同领域关于如何出色完成工作的技巧清单，它们的交集会是什么样子？我决定通过制作清单来找出答案。

Partly my goal was to create a guide that could be used by someone working in any field. But I was also curious about the shape of the intersection. And one thing this exercise shows is that it does have a definite shape; it's not just a point labelled "work hard."
我的部分目标是创建一份任何领域工作者都可以使用的指南。但我也对交集的形状感到好奇。而这项练习表明，它确实有一个明确的形状；它不仅仅是一个标有"努力工作"的点。

The following recipe assumes you're very ambitious.
以下食谱假设你非常有雄心。

The first step is to decide what to work on. The work you choose needs to have three qualities: it has to be something you have a natural aptitude for, that you have a deep interest in, and that offers scope to do great work.
第一步是决定要做什么。你选择的工作需要具备三个特质：它必须是你在某方面有天赋、你真正感兴趣，并且有足够空间去做出色工作的事情。

In practice you don't have to worry much about the third criterion. Ambitious people are if anything already too conservative about it. So all you need to do is find something you have an aptitude for and great interest in.[1]
实际上，你不必太担心第三个标准。雄心勃勃的人在这方面反而可能过于保守。所以你只需要找到你既有天赋又真正感兴趣的事情即可。[1]

That sounds straightforward, but it's often quite difficult. When you're young you don't know what you're good at or what different kinds of work are like. Some kinds of work you end up doing may not even exist yet. So while some people know what they want to do at 14, most have to figure it out.
这听起来很简单，但实际上往往非常困难。年轻时，你不知道自己擅长什么，也不知道不同类型的工作是什么样的。你最终可能会从事的工作甚至可能还不存在。所以虽然有些人 14 岁时就知道自己想要做什么，但大多数人必须自己摸索出来。

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's fine. It's good to know about multiple things; some of the biggest discoveries come from noticing connections between different fields.
确定要做什么的方法就是去尝试。如果你不确定要做什么，就猜一个方向。但选择一个方向并开始行动。你可能会猜错，但这没关系。了解多种事物是好事；一些最重要的发现来自于注意到不同领域之间的联系。

Develop a habit of working on your own projects. Don't let "work" mean something other people tell you to do. If you do manage to do great work one day, it will probably be on a project of your own. It may be within some bigger project, but you'll be driving your part of it.

养成独立开展项目的习惯。不要让"工作"变成别人告诉你该做的事。如果你真的某天做出了杰出工作，那很可能是在自己的项目上。它可能是一个大项目的一部分，但你会主导你负责的那部分。

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. At 7 it may seem excitingly ambitious to build huge things out of Lego, then at 14 to teach yourself calculus, till at 21 you're starting to explore unanswered questions in physics. But always preserve excitingness.

你的项目应该是什么？任何让你觉得充满激情且雄心勃勃的事。随着你的成长，你的项目品味会逐渐演变，激情与重要会逐渐融合。7 岁时，用乐高搭建巨大东西可能让你觉得充满激情且雄心勃勃；14 岁时，自学微积分可能让你觉得充满激情且雄心勃勃；21 岁时，你开始探索物理学中未解之谜。但始终要保持激情。

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.

有一种充满激情的好奇心，既是杰出工作的引擎也是其舵手。它不仅会驱动你，如果你顺其自然，它还会指引你该做什么。

What are you excessively curious about — curious to a degree that would bore most other people? That's what you're looking for.

你对自己极度好奇的事——那种会令大多数人觉得无聊的事？这就是你要找的。

Once you've 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. Knowledge expands fractally, and from a distance its edges look smooth, but once you learn enough to get close to one, they turn out to be full of gaps.

一旦你找到了自己极度感兴趣的事物，下一步就是学习足够多的知识，以便让你达到知识的前沿。知识以分形的方式扩展，从远处看其边缘似乎平滑，但一旦你学习到足够接近某个边缘，就会发现它们充满了空白。

The next step is to notice them. This takes some skill, because your brain wants to ignore such gaps in order to make a simpler model of the world. Many discoveries have come from asking questions about things that everyone else took for granted. [2]

下一步是注意到这些空白。这需要一些技巧，因为你的大脑想要忽略这些空白，以便构建一个更简单的世界模型。许多发现都来自于对那些其他人视为理所当然的事物提出疑问。[2]

If the answers seem strange, so much the better. Great work often has a tincture of strangeness. You see this from painting to math. It would be affected to try to manufacture it, but if it appears, embrace it.

如果答案显得奇怪，那反而更好。伟大的工作往往带有几分怪异。这一点从绘画到数学都能看到。试图刻意制造这种感觉会适得其反，但如果它出现了，就拥抱它。

Boldly chase outlier ideas, even if other people aren't 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 a bet as you'll find.[3]

大胆追逐那些不受其他人关注的外围想法——事实上，尤其是当它们不受关注时。如果你对某些被其他人忽视的可能性感到兴奋，并且你有足够的专业知识能够精确指出他们都在忽略什么，那这就是你能找到的最佳赌注。[3]

Four steps: choose a field, learn enough to get to the frontier, notice gaps, explore promising ones. This is how practically everyone who's done great work has done it, from painters to physicists.

四步：选择一个领域，学习足够达到前沿，发现差距，探索有潜力的方向。这是几乎所有做出伟大成就的人都遵循的方式，从画家到物理学家。

Steps two and four will 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. That's 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.

第二步和第四步需要付出艰苦的努力。虽然不能证明你必须努力才能成就伟大，但经验证据的规模与死亡证据相当。这就是为什么专注于你深感兴趣的事情至关重要。兴趣会驱使你比单纯的勤奋更努力。

The three most powerful motives are curiosity, delight, and the desire to do something impressive. Sometimes they converge, and that combination is the most powerful of all.

三个最强大的动机是好奇心、愉悦感和做出令人印象深刻的事情的愿望。有时它们会交汇，这种组合是最强大的。

The big prize is to discover a new fractal bud. You notice a crack in the surface of knowledge, pry it open, and there's a whole world inside.

大奖是发现一个新的分形嫩芽。你注意到知识表面的裂缝，将其撬开，里面有一个完整的世界。

Let's talk a little 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. Which means the four steps overlap: 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 thus not learning about, most other kinds of work. So in the worst case you choose late based on very incomplete information.[4]

让我们再稍微谈谈如何确定要做什么工作的复杂问题。之所以困难，主要是因为你无法在不实际去做的情况下判断大多数类型的工作是什么样的。这意味着这四个步骤是相互重叠的：你可能需要花数年时间才能了解自己喜欢多少，或者擅长多少。而在此期间，你并没有在做，因此也没有在学习其他大多数类型的工作。所以，在最坏的情况下，你基于非常不完整的信息做出了选择。[4]

The nature of ambition exacerbates this problem. Ambition comes in two forms, one that precedes interest in the subject and one that grows out of it. Most people who do great work

have a mix, and the more you have of the former, the harder it will be to decide what to do.
雄心的本质加剧了这个问题。雄心有两种形式，一种是先于对主题的兴趣，另一种是源于兴趣。大多数做出伟大工作的人都有这两种形式的混合，你拥有的前者越多，就越难决定要做什么。

The educational systems in most countries pretend it's easy. They expect you to commit to a field long before you could know what it's really like. And as a result an ambitious person on an optimal trajectory will often read to the system as an instance of breakage.
大多数国家的教育系统假装这很容易。他们期望你在可能还不知道它真正是什么样的情况下，就早早地选择一个领域。结果，一个有雄心且处于最佳发展轨迹的人，往往会将系统视为一个例外的破坏。

It would be better if they at least admitted it — if they admitted that the system not only can't do much to help you figure out what to work on, but is designed on the assumption that you'll somehow magically guess as a teenager. They don't tell you, but I will: when it comes to figuring out what to work on, you're on your own. Some people get lucky and do guess correctly, but the rest will find themselves scrambling diagonally across tracks laid down on the assumption that everyone does.
如果他们至少能承认这一点——承认这个系统不仅不能帮你弄清楚该做什么工作，而且是在假设你会以某种神奇的方式在青少年时期猜到的。他们不会告诉你，但我会：在弄清楚该做什么工作时，你只能靠自己。有些人运气好猜对了，但其他人会发现自己横冲直撞，沿着假设每个人都这样做铺设的轨道前进。

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. But there is no systematic procedure you can follow. 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 they happen to pick up. So you need to make yourself a big target for luck, and the way to do that is to be curious. Try lots of things, meet lots of people, read lots of books, ask lots of questions.[5]
如果你年轻有抱负但不知道该做什么，该怎么办？你不该被动地随波逐流，假设问题会自行解决。你需要采取行动。但没有什么系统性的程序可以遵循。当你阅读那些做出伟大成就的人的传记时，你会发现其中涉及了多么多的运气。他们通过偶然的相遇或偶然拿起的一本书发现了该做什么工作。所以你需要让自己成为幸运的目标，而做到这一点的方法是保持好奇心。尝试很多事情，结识很多人，阅读很多书，问很多问题。[5]

When in doubt, optimize for interestingness. Fields change as you learn more about them. What mathematicians do, for example, is very different from what you do in high school math classes. So you need to give different types of work a chance to show you what they're like. But a field should become *increasingly* interesting as you learn more about it. If it doesn't, it's probably not for you.
当不确定时，优先考虑有趣性。随着你对某个领域了解得更多，这个领域也会随之变化。例如，数学家所做的事情与你在高中数学课上的学习截然不同。因此，你需要给不同类型的工作一个机会，让他们展示自己的特点。但当你对某个领域了解得越多，它应该变得越来越有趣。如果它没有，那可能不适合你。

Don't worry if you find you're interested in different things than other people. The stranger your tastes in interestingness, the better. Strange tastes are often strong ones, and a strong taste for work means you'll be productive. And you're more likely to find new things if you're looking where few have looked before.

如果你发现自己感兴趣的事情与其他人不同，不要担心。你的品味越是独特，越好。独特的品味往往更强烈，而对工作的强烈兴趣意味着你会更有生产力。而且，如果你去探索那些鲜有人涉足的地方，你更有可能发现新事物。

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.

当你喜欢那些其他人觉得枯燥或可怕的部分时，这表明你可能适合某种类型的工作。

But fields aren't people; you don't owe them any loyalty. If in the course of working on one thing you discover another that's more exciting, don't be afraid to switch.

但领域不是人，你不需要对它们有任何忠诚。如果在处理某件事的过程中，你发现另一件更令人兴奋的事情，不要害怕转换方向。

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 you want to use. Since your friends probably have similar interests, this will also get you your initial audience.

如果你在为人们创造某物，确保它是他们真正想要的。做到这一点的最佳方式是创造你自己真正想要的东西。写下你想读的故事；构建你想使用的工具。由于你的朋友可能有着相似的兴趣，这也会帮你获得最初的受众。

This *should* follow from the excitingness rule. Obviously the most exciting story to write will be the one you want to read. The reason I mention this case explicitly is that 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.[6]

这应该遵循令人兴奋的规则。显然，最令人兴奋的故事就是你想要读的。我特别提到这种情况的原因是，有太多的人做错了。他们不是创造自己想要的，而是试图创造一些想象中更复杂的受众想要的。一旦你走那条路，你就会迷失。 [6]

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 wishes, eminent frauds. 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.

当你试图确定要做什么工作时，有很多力量会把你引偏。自负、时尚、恐惧、金钱、政治、别人的愿望、显赫的骗子。但如果你坚持你真正感兴趣的东西，你就能抵御所有这些。如果你感兴趣，你就不会迷失方向。

Following your interests may sound like a rather passive strategy, but in practice it usually means following them past all sorts of obstacles. You usually have to risk rejection and failure. So it does take a good deal of boldness.

跟随你的兴趣听起来可能是一种相当被动的策略，但在实践中，它通常意味着克服各种障碍去追随它们。你通常必须冒被拒绝和失败的风险。所以这确实需要相当大的勇气。

But while you need boldness, you don't usually 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. Instead of making a plan and then executing it, you just try to preserve certain invariants.

但是，虽然你需要勇气，但你通常不需要太多的计划。在大多数情况下，做伟大工作的秘诀很简单：努力去执行那些令人兴奋且雄心勃勃的项目，然后就会有好的结果。与其制定计划然后执行它，你只是试图保持某些不变性。

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 to as a child and then tenaciously pursuing that goal, but you can't discover natural selection that way.

计划的问题在于它只适用于你能事先描述的成就。你可以通过在童年时决定要赢得金牌或变得富有，然后坚定地追求这个目标来做到这一点，但你不能通过这种方式发现自然选择。

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 "staying upwind." This is how most people who've done great work seem to have done it.

我认为对于大多数想做伟大工作的人来说，正确的策略是不做太多计划。在每个阶段做任何看起来最有趣且能给你未来最佳选择的事情。我称这种方法为"保持顺风"。这是大多数做出伟大工作的人似乎都采用的方法。

Even when you've found something exciting to work on, working on it is not always 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 aren't like that.

即使你找到了令人兴奋的工作内容，着手去做也未必一帆风顺。有时会有新想法让你清晨一跃而起，立刻投入工作。但更多时候，情况并非如此。

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.

你不会只是扬起风帆，任由灵感将你向前推动。会有逆风、暗流和暗礁。因此，工作需要技巧，就像航行一样。

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 for four or five hours a day.

例如，虽然你必须努力工作，但可能用力过猛，那样你会发现收益递减：疲劳会让你变笨，最终甚至损害你的健康。工作收益递减的临界点因类型而异。有些最艰苦的类型，你可能每天只能做四五个小时。

Ideally those hours will be contiguous. To the extent you can, try to arrange your life so you have big blocks of time to work in. You'll shy away from hard tasks if you know you might be interrupted.

理想情况下，这些时间应该是连续的。在可能范围内，尽量安排你的生活以便有整块时间工作。如果你知道可能会被打断，就会回避艰巨的任务。

It will probably be harder to start working than to keep working. You'll often have to trick yourself to get over that initial threshold. Don't worry about this; it's the nature of work, not a flaw in your character. Work has a sort of activation energy, both per day and per project. And since this threshold is fake in the sense that it's higher than the energy required to keep going, it's ok to tell yourself a lie of corresponding magnitude to get over it.

开始工作可能比继续工作更难。你常常需要用一些小计谋来克服最初的门槛。别担心这一点；这是工作的本性，而不是你性格上的缺陷。工作有一种类似"活化能"的东西，既针对每天的工作，也针对每个项目。而且，因为这个门槛在某种意义上是虚假的——它比继续前进所需的能量要高，所以告诉自己一个相应规模的谎言来克服它是可以的。

It's 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 I've found something that seems mistaken or incomplete, and I'm off.

如果你想要做伟大的工作，通常对自己撒谎是一个错误，但这是少数例外情况之一。早上当我不想开始工作时，我常常骗自己说"我只会先看看我之前的工作内容"。五分钟后，我发现了一些看似错误或不完整的地方，然后我就开始了。

Similar techniques work for starting new projects. It's ok to lie to yourself about how much work a project will entail, for example. Lots of great things began with someone saying "How hard could it be?"

类似的技巧也适用于开始新项目。例如，对自己撒谎关于一个项目需要多少工作量是可以的。很多伟大的事情都是因为有人说了"这有多难呢？"而开始的。

This is one case where the young have an advantage. They're more optimistic, and even though one of the sources of their optimism is ignorance, in this case ignorance can sometimes beat knowledge.

这是年轻人有优势的情况之一。他们更乐观，尽管他们乐观的一个来源是无知，但在这种情况下，无知有时可以战胜知识。

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 tidiness or self-discipline. In many projects a lot of the best work happens in what was meant to be the final stage.

尽管如此，还是尽量完成你开始的事情，即使它最终比预期的要费更多功夫。完成事情不仅仅是一个整洁或自律的练习。在许多项目中，最好的工作往往发生在原定的最后阶段。

Another permissible lie is to 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 after all.[7]

另一个可以接受的谎言是夸大你所做事情的重要性，至少在你自己的心里。如果这能帮助你发现新事物，那么它最终可能根本不是谎言。[7]

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 the more dangerous. You put off starting that ambitious project from year to year because the time isn't quite right. When you're procrastinating in units of years, you can get a lot not done.[8]

由于开始工作有两种含义——按天和按项目——因此也存在两种形式的拖延。按项目的拖延远比按天的拖延更危险。你因为时机还不够成熟而逐年推迟开始那个雄心勃勃的项目。当你按年拖延时，你会有很多事情没完成。[8]

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. So per-project procrastination doesn't set off the alarms that per-day procrastination does. You're too busy to notice it.

按项目拖延之所以危险的一个原因是，它通常伪装成工作。你不仅仅是在无所事事；你正在勤奋地做着其他事情。因此，按项目拖延不会像按天拖延那样触发警报。你太忙了，以至于没有注意到它。

The way to beat it is to stop occasionally and ask yourself: Am I working on what I most want to work on? When you're young it's ok if the answer is sometimes no, but this gets increasingly dangerous as you get older.[9]

战胜它的方法是偶尔停下来问问自己：我是否在从事我最想从事的工作？年轻时答案有时是"不"还可以，但随着年龄增长，这种情况会变得越来越危险。[9]

Great work usually entails spending what would seem to most people an unreasonable amount of time on a problem. You can't think of this time as a cost, or it will seem too high. You have to find the work sufficiently engaging as it's happening.

伟大的工作通常需要花费大多数人看来不合理的时间来解决问题。你不能把这段时间看作成本，否则会觉得太高。你必须让工作在发生时足够吸引人。

There may be some jobs where you have to work diligently for years at things you hate before you get to the good part, but this is not how great work happens. Great work happens by focusing consistently on something you're genuinely interested in. When you pause to take stock, you're surprised how far you've come.

或许有些工作，你必须勤奋地做多年你讨厌的事情，才能进入好的部分，但这不是伟大工作的发生方式。伟大工作是通过持续专注于你真正感兴趣的事情来发生的。当你停下来盘点时，你会惊讶自己已经走了多远。

The reason we're surprised is that we underestimate the cumulative effect of work. Writing a page a day doesn't sound like much, but if you do it every day you'll write a book a year. That's the key: consistency. People who do great things don't get a lot done every day. They get something done, rather than nothing.

我们惊讶的原因是我们低估了工作的累积效应。每天写一页听起来不多，但如果你每天都这样做，一年就能写一本书。关键在于：一致性。做伟大事情的人每天不会做很多事。他们完成了一些事，而不是什么都没做。

If you do work that compounds, you'll get exponential growth. Most people who do this do it unconsciously, but it's worth stopping to think about. Learning, for example, is an instance of

this phenomenon: the more you learn about something, the easier it is to learn more. Growing an audience is another: the more fans you have, the more new fans they'll bring you.

如果你做的是复合型工作，你会获得指数级增长。大多数做这种工作的人都是无意识的，但值得停下来思考一下。学习就是一个例子：你越了解某件事，就越容易学习更多。培养受众也是一样：你拥有的粉丝越多，他们就会为你带来更多新粉丝。

The trouble with exponential growth is that the curve feels flat in the beginning. It isn't; it's still a wonderful exponential curve. But we can't grasp that intuitively, so we underrate exponential growth in its early stages.

指数级增长的问题在于，在初期曲线看起来是平坦的。其实不是，它仍然是一个美妙的指数曲线。但我们无法直观地理解这一点，所以在早期阶段往往低估了指数级增长。

Something that grows exponentially can become so valuable that it's worth making an extraordinary effort to get it started. But since we underrate exponential growth early on, this too is mostly done unconsciously: people push through the initial, unrewarding phase of learning something new because they know from experience that learning new things always takes an initial push, or they grow their audience one fan at a time because they have nothing better to do. If people consciously realized they could invest in exponential growth, many more would do it.

一个呈指数增长的事物可能会变得非常宝贵，值得付出非凡的努力来启动它。但由于我们在早期阶段低估了指数级增长，这也大多是无意识的：人们因为知道从经验中学习新事物总是需要最初的推动，而坚持学习新事物的最初、无回报的阶段；或者他们一个一个地增加粉丝，因为他们没什么更好的事情可做。如果人们能意识到他们可以投资于指数级增长，会有更多的人这样做。

Work doesn't just happen when you're trying to. 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 little, you'll often solve problems you were unable to solve by frontal attack.

工作并非只在刻意尝试时发生。当你散步、淋浴或躺在床上时，会进行一种无目的的思考，这种思考可以非常有力。通过让你的思绪稍微漫游，你常常能解决那些通过正面攻击无法解决的问题。

You have to be working hard in the 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 it questions.[10]

然而，要从中受益，你必须以正常的方式努力工作。你不能只是漫无目的地闲逛。这种白日梦必须与有意识的、能提供问题的思考工作交织进行。[10]

Everyone knows to avoid distractions at work, but it's also important to avoid them in the other half of the cycle. 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. (Exception: Don't avoid love.)

每个人都知道在工作中要避免干扰，但在周期的另一半也要避免干扰同样重要。当你让思绪漫

游时，它会漫游到你那一刻最关心的事情上。因此，要避免那种将你的工作推出首位干扰，否则你会浪费这种宝贵类型的思考在干扰上。（例外：不要避免爱情。）

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.
有意识地培养你在领域内所做工作的品味。直到你知道什么是最好的以及是什么使其成为最好的，你才知道你追求的目标是什么。

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's true. It could be because ambition is a phenomenon where almost all the error is in one direction — where almost all the shells that miss the target miss by falling short. Or it could be because ambition to be the best is a qualitatively different thing from ambition to be good. Or maybe being good is simply too vague a standard. Probably all three are true.[11]
这就是你追求的目标，因为如果你不去争取成为最好，你甚至不会变得优秀。这一观察已被许多不同领域的人多次提出，或许值得思考为什么这是真的。这可能是因为雄心壮志是一种现象，几乎所有的错误都发生在同一个方向——几乎所有的脱靶都因为不够用力而落空。或者，追求最好与追求优秀在本质上有所不同。或者，优秀可能只是一个过于模糊的标准。也许这三者都是真的。[11]

Fortunately there's a kind of economy of scale here. Though it might seem like you'd be taking on a heavy burden by trying to be the best, in practice you often end up net ahead. It's exciting, and also strangely liberating. It simplifies things. In some ways it's easier to try to be the best than to try merely to be good.
幸运的是，这里有一种规模经济效应。虽然尝试成为最好似乎会让你承担沉重的负担，但实际上你往往最终受益。这令人兴奋，也奇怪地让人感到解放。它简化了事情。在某些方面，尝试成为最好比仅仅尝试变得优秀更容易。

One way to aim high is to try to make something that people will care about in a hundred years. Not because their opinions matter more than your contemporaries', but because something that still seems good in a hundred years is more likely to be genuinely good.
一种追求高目标的方式是尝试创造一些在一百年后人们仍会关心的事物。不是因为他们的意见比你的同时代人更重要，而是因为一百年后仍被认为好的事物更有可能真正优秀。

Don't try to work in a distinctive style. Just try to do the best job you can; you won't be able to help doing it in a distinctive way.
不要试图以独特的风格工作。只需尽力做好你能做的工作；你自然会以独特的方式完成它。

Style is doing things in a distinctive way without trying to. Trying to is affectation.
风格就是不经意地以独特的方式做事。刻意追求反而显得做作。

Affectation is in effect to pretend that someone other than you is doing the work. You adopt an impressive but fake persona, and while you're pleased with the impressiveness, the fakeness is what shows in the work.[12]

做作实际上就是假装工作是由另一个人完成的。你采用一个令人印象深刻但虚假的形象，虽然你满意这种印象，但正是虚假之处在作品中显露无遗。[12]

The temptation to be someone else is greatest for the young. They often feel like nobodies. But you never need to worry about that problem, because it's self-solving if you work on sufficiently ambitious projects. 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.
年轻人最容易产生成为他人的诱惑。他们常常觉得自己一无是处。但你永远不必担心这个问题，因为如果你从事足够宏伟的项目，它就会自我解决。如果你成功完成一个宏伟的项目，你就不是一无是处的人；你就是完成这件事的人。所以只需做工作，你的身份自然会得到确立。

"Avoid affectation" is a useful rule so far as it goes, but how would you express this idea positively? How would you say what to be, instead of what not to be? The best answer is earnest. If you're earnest you avoid not just affectation but a whole set of similar vices.
"避免做作"这条规则在某种程度上是有用的，但如何用积极的方式表达这个想法？如何说应该成为什么样的人，而不是不应该成为什么样的人？最好的答案是真诚。如果你真诚，你不仅会避免做作，还会避免一系列类似的恶习。

The core of being earnest is being intellectually honest. We're taught as children to be honest as an unselfish virtue — as a kind of sacrifice. But in fact it's a source of power too. To see new ideas, you need an exceptionally sharp eye for the truth. You're trying to see more truth than others have seen so far. And how can you have a sharp eye for the truth if you're intellectually dishonest?
真诚的核心是保持思想上的诚实。我们从小被教导诚实是一种无私的美德——某种形式的牺牲。但实际上，它也是一种力量的源泉。要看到新的想法，你需要有极其敏锐的洞察力。你试图看到比其他人更深刻的真理。如果你在思想上是 dishonest（不诚实的），怎么可能拥有敏锐的洞察力呢？

One way to avoid intellectual dishonesty is to maintain a slight positive pressure in the opposite direction. Be aggressively willing to admit that you're mistaken. Once you've admitted you were mistaken about something, you're free. Till then you have to carry it.[13]
避免思想不诚实的另一种方法是保持轻微的、相反方向的正向压力。要积极地承认自己可能是错的。一旦你承认自己在某件事上是错的，你就会解脱。在那之前，你必须背负这个负担。[13]

Another more subtle component of earnestness is informality. Informality is much more important than its grammatically negative name implies. It's not merely the absence of something. It means focusing on what matters instead of what doesn't.
真诚的另一个更微妙的部分是非正式。非正式的重要性远超其语法上负面的名称所暗示的。它不仅仅是缺乏某种东西。它意味着关注重要的事情，而不是不重要的事情。

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. But any energy that goes into how you seem comes out of being good. That's one reason nerds have an advantage in doing great work: they expend little effort on seeming anything. In fact that's basically the definition of a nerd.
正式和做作共有的特点是，在做事的同时，你还在努力表现得某种样子。但任何用于表现自己

的精力都来自于做好事。这就是为什么技术宅在取得伟大成就时具有优势：他们很少费力去表现什么。事实上，这基本上就是技术宅的定义。

Nerds have a kind of innocent boldness that's exactly what you need in doing great work. It's not learned; it's preserved from childhood. So hold onto it. 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.

技术宅有一种天真的大胆，这正是做伟大工作时所需要的。它不是后天学来的；它是从童年时期保留下来的。所以请保持它。做那个把事情摆出来的人，而不是那个坐在后面对它们提出听起来很复杂的批评的人。"批评很容易"在字面意义上是真实的，而通往伟大成就的道路从来都不是容易的。

There may be some jobs where it's an advantage to be cynical and pessimistic, but if you want to do great work it's an advantage to be optimistic, even though that means you'll risk looking like a fool sometimes. There's an old tradition of doing the opposite. The Old Testament says it's better to keep quiet lest you look like a fool. But that's advice for *seeming* smart. If you actually want to discover new things, it's better to take the risk of telling people your ideas.

在某些工作中，愤世嫉俗和悲观可能是一种优势，但如果你想做伟大的工作，乐观则是一种优势，尽管这意味着你有时会冒看起来像傻瓜的风险。有一个古老的传统是做相反的事。旧约圣经说保持沉默更好，以免看起来像傻瓜。但这只是为了让别人显得聪明。如果你真的想发现新事物，冒风险告诉别人你的想法是更好的选择。

Some people are naturally earnest, and with others it takes a conscious effort. Either kind of earnestness will suffice. But I doubt it would be possible to do great work without being earnest. It's so hard to do even if you are. You don't have enough margin for error to accommodate the distortions introduced by being affected, intellectually dishonest, orthodox, fashionable, or cool.[14]

有些人天生认真，而对另一些人则需要有意识地努力。任何一种认真程度都足够了。但我怀疑如果没有认真，就不可能做伟大的工作。即使你认真了，也很难做到。因为你没有足够的余地来容忍受影响、不诚实、正统、时髦或酷所带来的扭曲。[14]

Great work is consistent not only with who did it, but with itself. It's usually all of a piece. So if you face a decision in the middle of working on something, ask which choice is more consistent.

伟大的工作不仅与是谁做的相一致，而且与它自身相一致。它通常是完整统一的。所以如果你在从事某事的过程中面临一个决定，问问自己哪个选择更一致。

You may have to throw things away and redo them. You won't necessarily have to, but you have to be willing to. And that can take some effort; when there's something you need to redo, status quo bias and laziness will combine to keep you in denial about it. To beat this ask: If I'd already made the change, would I want to revert to what I have now?

你可能不得不扔掉东西并重新做。你不必这样做，但你必须愿意这样做。而且这可能需要一些

努力；当你需要重做某事时，现状偏见和懒惰会结合起来让你否认这一点。为了克服这一点，问自己：如果我已经做了改变，我会想回到我现在拥有的状态吗？

Have the confidence to cut. Don't keep something that doesn't fit just because you're proud of it, or because it cost you a lot of effort.
要有勇气删减。不要因为自己为它付出了心血，或者因为它耗费了你很多精力，就保留那些不合适的东西。

Indeed, 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; and you won't be able to lie to yourself about whether there's anything real there.
确实，在某些类型的工作中，将你正在做的事情剥离到本质是件好事。结果会更加集中；你会更理解它；而且你将无法欺骗自己，判断其中是否真的有什么有价值的东西。

Mathematical elegance may sound like a mere metaphor, drawn from the arts. That's what I thought when I first heard the term "elegant" applied to a proof. But now I suspect it's conceptually prior — that the main ingredient in artistic elegance is mathematical elegance. At any rate it's a useful standard well beyond math.
数学上的优雅听起来可能只是一个比喻，源自艺术。当我第一次听到将"优雅"这个词用于证明时，我就是这么认为的。但现在我怀疑它概念上更为优先——艺术优雅的主要成分是数学上的优雅。无论如何，这是一个超越数学的有用标准。

Elegance can be a long-term bet, though. Laborious solutions will often have more prestige in the short term. They cost a lot of effort and they're hard to understand, both of which impress people, at least temporarily.
优雅有时可以是一项长期赌注。费力的解决方案在短期内往往更有声望。它们耗费了大量精力，而且难以理解，这两点都能给人留下印象，至少是暂时的。

Whereas some of the very 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.
有些极好的工作看起来似乎花费相对较少的努力，因为它们在某种程度上已经存在了。它们不需要被建造，只需要被发现。当难以说清自己是在创造某物还是在发现某物时，这是一个非常好的迹象。

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 through which the ideas take their natural shape.
当你所做的工作既可以说是创造也可以说是发现时，倾向于发现。试着将自己想象成一个通道，通过这个通道，想法自然地形成。

(Strangely enough, one exception is the problem of choosing a problem to work on. This is usually seen as 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.)
（奇怪的是，有一个例外是选择要解决的问题。这通常被视为搜索，但在最好的情况下，它更像是在创造某物。在最好的情况下，你在探索的过程中创造了这个领域。）

Similarly, if you're trying to build a powerful tool, make it gratuitously unrestrictive. A powerful tool almost by definition will be used in ways you didn't expect, so err on the side of eliminating restrictions, even if you don't know what the benefit will be.

类似地，如果你正在尝试建造一个强大的工具，让它任意地不受限制。一个强大的工具几乎可以定义为以你未曾预料的方式被使用，所以倾向于消除限制，即使你不知道这将带来什么好处。

Great work will often be tool-like in the sense of being something others build on. So it's a good sign if you're creating ideas that others could use, or exposing questions that others could answer. The best ideas have implications in many different areas.

伟大的工作往往具有工具性，意味着它是他人可以在此基础上进行构建的事物。因此，如果你正在创造他人可能使用或提出他人可能回答的问题，这便是一个好迹象。最好的想法会在许多不同领域产生影响。

If you express your ideas in the most general form, they'll be truer than you intended.

如果你以最一般的形式表达你的想法，它们会比预期的更加真实。

True by itself is not enough, of course. Great ideas have to be true and new. And it takes a certain amount of ability to see new ideas even once you've learned enough to get to one of the frontiers of knowledge.

当然，仅仅真实是不够的。伟大的想法必须既真实又新颖。并且，即使你已经学到了足以达到知识前沿的知识，还需要一定的能力才能看到新的想法。

In English we give this ability names like originality, creativity, and imagination. And it seems reasonable to give it a separate name, because it does seem to some extent a separate skill. It's possible to have a great deal of ability in other respects — to have a great deal of what's often called *technical* ability — and yet not have much of this.

在英语中，我们用诸如原创性、创造力和想象力等名称来称呼这种能力。而且，给它一个单独的名称似乎是合理的，因为它在某种程度上确实是一种独立的技能。在其他方面可能拥有很强的能力——拥有通常所说的技术能力——但在这方面却可能并不突出。

I've never liked 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, like an angle grinder throwing off sparks. They can't help it.

我从未喜欢"创意过程"这个术语。它似乎具有误导性。原创性不是一种过程，而是一种思维习惯。原创性思考者会不断抛出关于他们所关注事物的全新想法，就像角磨机进出火花一样。他们无法控制这一点。

If the thing they're focused on is something they don't understand very well, these new ideas might not be good. One of the most original thinkers I know decided to focus on dating after he got divorced. He knew roughly as much about dating as the average 15 year old, and the results were spectacularly colorful. But to see originality separated from expertise like that made its nature all the more clear.

如果他们所关注的事物是他们并不非常了解的，那么这些新想法可能并不好。我所知道的最具原创性的思考者之一在离婚后决定专注于约会。他对约会的了解程度与一个普通 15 岁少年差不多，结果非常多彩。但将原创性与专业知识如此分离，反而更清晰地揭示了它的本质。

I don't know if it's possible to cultivate originality, but there are definitely ways to make the most of however much you have. For example, 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. [15]

我不知道是否有可能培养原创性，但确实有方法充分利用你拥有的任何原创性。例如，当你专注于某项工作时，更有可能产生原创性想法。原创性想法不会来自试图产生原创性想法的努力。它们来自试图构建或理解一些略微超出你能力范围的事物。[15]

Talking or writing about the things you're interested in is a good way to generate new ideas. When you try to put ideas into words, a missing idea creates a sort of vacuum that draws it out of you. Indeed, there's a kind of thinking that can only be done by writing.

谈论或书写你感兴趣的事物是产生新想法的好方法。当你试图将想法用语言表达时，一个缺失的想法会形成一种真空，将之吸引出来。确实，有些思考方式只能通过写作来完成。

Changing your context can help. If you visit a new place, you'll often find you have new ideas there. The journey itself often dislodges them. But you may not have to go far to get this benefit. Sometimes it's enough just to go for a walk.[16]

改变你的环境也有帮助。如果你去一个新地方，你常常会发现那里有新的想法。旅程本身往往能将它们激发出来。但你可能不需要走很远就能获得这种益处。有时只是散步就足够了。[16]

It also helps to travel in topic space. You'll have more new ideas if you explore lots of different topics, partly because it gives the angle grinder more surface area to work on, and partly because analogies are an especially fruitful source of new ideas.

在主题空间中旅行也有帮助。如果你探索许多不同的话题，你会产生更多新想法，部分原因是这为角磨机提供了更多的操作表面，部分原因是类比是新想法的一个特别丰富的来源。

Don't 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.[17] Be professionally curious about a few topics and idly curious about many more.

但不要在许多话题之间平均分配你的注意力，否则你会分得太散。你希望根据更接近幂律来分配。[17] 对少数话题保持专业的好奇心，对更多话题保持随意的的好奇心。

Curiosity and originality are closely related. Curiosity feeds originality by giving it new things to work on. But the relationship is closer than that. Curiosity is itself a kind of originality; it's roughly to questions what originality is to answers. And since questions at their best are a big component of answers, curiosity at its best is a creative force.

好奇心和原创性密切相关。好奇心通过提供新的素材来滋养原创性。但它们的关系比这更紧密。好奇心本身就是一种原创性；它大致相当于问题之于答案。而既然问题在最佳状态下是答案的重要组成部分，那么好奇心在最佳状态下就是一种创造力。

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. Why did no one think of this before?

产生新想法是一种奇怪的游戏，因为它通常涉及看到那些就在你鼻子底下的事物。一旦你看到了一个新想法，它往往显得显而易见。为什么之前没有人想到这个呢？

When an idea seems simultaneously novel and obvious, it's probably a good one.
当一个想法同时显得新颖和显而易见时，它很可能是一个好想法。

Seeing something obvious sounds easy. And yet empirically having new ideas is hard. What's the source of this apparent contradiction? It's that seeing the new idea usually requires you to change the way you look at the world. We see the world through models that both help and constrain us. When you fix a broken model, new ideas become obvious. But noticing and fixing a broken model is hard. That's how new ideas can be both obvious and yet hard to discover: they're easy to see after you do something hard.
看到显而易见的事物听起来很容易。然而，实际上产生新想法很难。这种明显矛盾的原因是什么？是因为看到新想法通常需要你改变看待世界的方式。我们通过既帮助我们又限制我们的模型来看待世界。当你修正一个有缺陷的模型时，新想法就变得显而易见。但发现并修正一个有缺陷的模型很难。这就是新想法可以既显而易见又难以发现的原因：在你做了件难事之后，它们就很容易被看到。

One way to discover broken models is to be stricter than other people. Broken models of the world leave a trail of clues where they bash against reality. Most people don't want to see these clues. It would be an understatement to say that they're attached to their current model; it's what they think in; so they'll tend to ignore the trail of clues left by its breakage, however conspicuous it may seem in retrospect.
一种发现模型错误的方法是比其他人更严格。世界模型的错误会在它们与现实碰撞时留下线索。大多数人不想看到这些线索。说他们依恋当前的模型都算轻描淡写；那才是他们思考的方式，所以他们会倾向于忽视模型破裂时留下的线索，无论这些线索在事后看起来多么明显。

To find new ideas you have to seize on signs of breakage instead of looking away. That's what Einstein did. He was able to see the wild implications of Maxwell's equations not so much because he was looking for new ideas as because he was stricter.
要发现新想法，你必须抓住模型破裂的迹象，而不是视而不见。这就是爱因斯坦的做法。他能看到麦克斯韦方程式的惊人推论，不仅仅是因为他在寻找新想法，更是因为他更严格。

The other thing you need is a willingness to break rules. Paradoxical as it sounds, if you want to fix your model of the world, it helps to be the sort of person who's comfortable breaking rules. From the point of view of the old model, which everyone including you initially shares, the new model usually breaks at least implicit rules.
你需要的是打破规则的意愿。虽然听起来有些矛盾，但如果你想修正你对世界的认知模型，成为一个能够坦然打破规则的人会很有帮助。从最初每个人（包括你自己）都共享的旧模型角度来看，新模型通常会打破至少一些隐性的规则。

Few understand the degree of rule-breaking required, because new ideas seem much more conservative once they succeed. They seem perfectly reasonable once you're using the new model of the world they brought with them. 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.
很少有人理解所需的打破规则的程度，因为一旦新想法取得成功，它们看起来就会更加保守。

当你开始使用它们带来的新世界模型时，它们似乎非常合理。但在当时并非如此；地心说模型被普遍接受甚至在天文学家之间也是如此，这花费了大部分时间，因为它感觉非常错误。

Indeed, if you think about it, a good new idea has to seem bad to most people, or someone would have already explored it. So what you're looking for is ideas that seem crazy, but the right kind of crazy. How do you recognize these? You can't with certainty. Often ideas that seem bad are bad. But ideas that are the right kind of crazy tend to be exciting; they're rich in implications; whereas ideas that are merely bad tend to be depressing.

事实上，如果你仔细思考，一个好的新想法必须对大多数人来说看起来很糟糕，否则早有人探索过了。所以你在寻找的是看似疯狂，但却是正确类型的疯狂的想法。你如何识别这些？你无法确定。通常看起来很糟糕的想法就是很糟糕。但正确类型的疯狂想法往往令人兴奋；它们蕴含着丰富的意义；而仅仅是糟糕的想法则往往令人沮丧。

There are two ways to be comfortable breaking rules: to enjoy breaking them, and to be indifferent to them. I call these two cases being aggressively and passively independent-minded.

有两种方式让人在打破规则时感到舒适：享受打破规则，或对规则漠不关心。我把这两种情况称为主动和被动地independent-minded.

The aggressively independent-minded are the naughty ones. Rules don't merely fail to stop them; breaking rules gives them additional energy. For this sort of person, delight at the sheer audacity of a project sometimes supplies enough activation energy to get it started.

主动独立的头脑是淘气的。规则不仅无法阻止他们，打破规则反而给他们增添能量。对于这样的人，对项目纯粹大胆的喜悦有时能提供足够的激活能量来启动它。

The other way to break rules is not to care about them, or perhaps even to know they exist. This is why novices and outsiders often make new discoveries; their ignorance of a field's assumptions acts as a source of temporary passive independent-mindedness. Aspies also seem to have a kind of immunity to conventional beliefs. Several I know say that this helps them to have new ideas.

打破规则的另一种方式是漠不关心它们，甚至可能根本不知道它们的存在。这就是为什么新手和局外人经常能做出新发现；他们对某个领域假设的无知暂时成为了一种被动的源泉。Aspies似乎也对传统观念有一种免疫力。我所认识的一些人说，这帮助他们产生了新想法。

Strictness plus rule-breaking sounds like a strange combination. In popular culture they're opposed. But popular culture has a broken model in this respect. It implicitly assumes that issues are trivial ones, and in trivial matters strictness and rule-breaking *are* opposed. But in questions that really matter, only rule-breakers can be truly strict.

严格性与打破规则听起来像是一种奇怪的结合。在流行文化中它们是相互对立的。但流行文化在这方面有一个破败的模式。它隐含地假设问题都是琐碎的，而在琐碎的事情上严格性和打破规则是相互对立的。但在真正重要的问题上，只有打破规则的人才能真正做到严格。

An overlooked idea often doesn't lose till the semifinals. You do see it, subconsciously, but then another part of your subconscious shoots it down because it would be too weird, too risky, too much work, too controversial. This suggests an exciting possibility: if you could turn off such filters, you could see more new ideas.

一个被忽视的想法往往不会在半决赛中失去。你确实会下意识地看到它，但你的潜意识中的另一部分会将其击垮，因为它太奇怪、太冒险、太费事、太有争议。这暗示了一个激动人心的可能性：如果你能关闭这些过滤器，你就能看到更多新想法。

One way to do that is to ask what would be good ideas for *someone else* to explore. Then your subconscious won't shoot them down to protect you.
做到这一点的办法是问一下，对于其他人来说，哪些是值得探索的好想法。这样，你的潜意识就不会为了保护你而将它们击垮。

You could also discover overlooked ideas by working in the other direction: by starting from what's obscuring them. Every cherished but mistaken principle is surrounded by a dead zone of valuable ideas that are unexplored because they contradict it.
你也可以通过反向工作来发现被忽视的想法：从掩盖它们的地方开始。每一个被珍视却错误的原理，都被一群未被探索的宝贵想法所包围，这些想法之所以未被探索，是因为它们与该原理相悖。

Religions are collections of cherished but mistaken principles. So anything that can be described either literally or metaphorically as a religion will have valuable unexplored ideas in its shadow. Copernicus and Darwin both made discoveries of this type.[18]
宗教是一系列被珍视却错误的原理的集合。因此，任何可以被字面或比喻地描述为宗教的事物，在其阴影中都有宝贵的未被探索的想法。哥白尼和达尔文都做出了这类发现。[18]

What are people in your field religious about, in the sense of being too attached to some principle that might not be as self-evident as they think? What becomes possible if you discard it?
在你的领域里，人们在某种意义上过于执着于某些原理，这些原理可能不像他们所想的那样显而易见？如果你抛弃它，会变得可能？

People show much more originality in solving problems than in deciding which problems to solve. Even the smartest can be surprisingly conservative when deciding what to work on. People who'd never dream of being fashionable in any other way get sucked into working on fashionable problems.
人们在解决问题时表现出更多的原创性，而不是在决定要解决哪些问题。即使是聪明的人，在决定要做什么工作时，也会出人意料地保守。那些在其他方面绝不会追求时尚的人，会被卷入解决时尚问题。

One reason people are more conservative when choosing problems than solutions is that problems are bigger bets. A problem could occupy you for years, while exploring a solution might only take days. But even so I think most people are too conservative. They're not merely responding to risk, but to fashion as well. Unfashionable problems are undervalued.
人们在选择问题时比在选择解决方案时更保守的一个原因是，问题往往涉及更大的赌注。一个问题可能会占据你数年，而探索一个解决方案可能只需几天。即便如此，我认为大多数人还是过于保守。他们不仅是在应对风险，也是在应对潮流。不受欢迎的问题往往被低估。

One of the most interesting kinds of unfashionable problem is the problem that people think has been fully explored, but hasn't. Great work often takes something that already exists and

shows its latent potential. Durer and Watt both did this. So if you're interested in a field that others think is tapped out, don't let their skepticism deter you. People are often wrong about this.

不受欢迎的问题中最有趣的一种是那些人们认为已经被充分探索，但实际上并未被充分探索的问题。伟大的工作往往能发掘那些已经存在的事物中潜藏的潜力。丢勒和瓦特都做到了这一点。所以如果你感兴趣于一个被其他人认为已经枯竭的领域，不要让他们的怀疑阻碍你。人们往往在这个问题上判断错误。

Working on an unfashionable problem can be very pleasing. There's no hype or hurry. Opportunists and critics are both occupied elsewhere. The existing work often has an old-school solidity. And there's a satisfying sense of economy in cultivating ideas that would otherwise be wasted.

处理一个不受欢迎的问题可能会带来极大的满足感。没有炒作或匆忙。投机者和批评家都忙于其他事情。现有的工作往往具有老派的坚实性。而且，培养那些原本会被浪费掉的想法会带来一种令人满意的节约感。

But the most common type of overlooked 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. 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.

但最常见的被忽视的问题并非在时尚意义上明确过时。它只是看起来不像它实际那么重要。你如何发现这些问题？通过自我放纵——让你的好奇心自由发展，并暂时忽略你脑海中那个告诉你只应该处理"重要"问题的声音。

You do need to work on important problems, but almost everyone is too conservative about what counts as one. And if there's an important but overlooked problem in your neighborhood, it's probably already on your subconscious radar screen. So try asking yourself: if you were going to take a break from "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.

你需要处理重要问题，但几乎每个人对于什么才算重要都过于保守。如果你所在社区有一个重要但被忽视的问题，它很可能已经在你的潜意识雷达上。所以试着问自己：如果你打算从"严肃"工作中休息，只为它真正有趣而处理某事，你会做什么？答案可能比你想象的更重要。

Originality in choosing problems seems to matter even more than originality in solving them. That's what distinguishes the people who discover whole new fields. So what might seem to be merely the initial step — deciding what to work on — is in a sense the key to the whole game.

在选择问题时展现原创性似乎比解决问题时的原创性更重要。这就是发现全新领域的人与众不同的地方。所以，看似仅仅是第一步——决定要处理什么——在某种程度上是整个游戏的关键。

Few grasp this. One of the biggest misconceptions about new ideas is about the ratio of question to answer in their composition. People think big ideas are answers, but often the

real insight was in the question.

很少有人能理解这一点。关于新想法的最大误解之一在于其构成中问题与答案的比例。人们认为大想法是答案，但真正的洞见往往在于问题本身。

Part of the reason we underrate questions is the way they're used in schools. In schools they tend to exist only briefly before being answered, like unstable particles. But a really good question can be much more than that. A really good question is a partial discovery. How do new species arise? Is the force that makes objects fall to earth the same as the one that keeps planets in their orbits? By even asking such questions you were already in excitingly novel territory.

我们低估问题的部分原因在于学校里它们的使用方式。在学校里，问题往往在被回答前只存在短暂的时间，就像不稳定的粒子。但一个真正好的问题可以远不止于此。一个真正好的问题是部分发现。新物种是如何产生的？使物体坠向地球的力与使行星保持在轨道上的力是否相同？仅仅提出这样的问题，你就已经进入了激动人心的新领域。

Unanswered questions can be uncomfortable things to carry around with you. But the more you're carrying, the greater the chance of noticing a solution — or perhaps even more excitingly, noticing that two unanswered questions are the same.

未解决的问题可能会让你感到不舒服。但你携带得越多，注意到解决方案的机会就越大——或者更令人兴奋的是，注意到两个未解决的问题实际上是同一个问题。

Sometimes you carry a question for a long time. Great work often comes from returning to a question you first noticed years before — in your childhood, even — and couldn't stop thinking about. People talk a lot about the importance of keeping your youthful dreams alive, but it's just as important to keep your youthful questions alive.[19]

有时候你带着一个问题很长时间。伟大的工作往往来自于回到你多年前注意到的问题——甚至是在童年时期——并且无法停止思考的问题。人们经常谈论保持年轻梦想的重要性，但保持年轻问题的活力同样重要。[19]

This is one of the places where actual expertise differs most from the popular picture of it. In the popular picture, experts are certain. But actually the more puzzled you are, the better, so long as (a) the things you're puzzled about matter, and (b) no one else understands them either.

这是实际专业知识和大众对其的普遍看法差异最大的地方之一。在普遍的看法中，专家是确定的。但实际上，你越困惑，越好，只要（a）你困惑的事情很重要，并且（b）没有人理解它们。

Think about what's happening at the moment just before a new idea is discovered. Often someone with sufficient expertise is puzzled about something. Which means that originality consists partly of puzzlement — of confusion! You have to be comfortable enough with the world being full of puzzles that you're willing to see them, but not so comfortable that you don't want to solve them.[20]

想想在新的想法被发现前那一刻正在发生的事情。通常，某个具有足够专业知识的人会对某事感到困惑。这意味着原创性部分包含困惑——混乱！你必须足够习惯于世界充满谜题，以至于你愿意看到它们，但又不能太习惯以至于不想解决它们。[20]

It's a great thing to be rich in unanswered questions. And this is one of those situations where the rich get richer, because the best way to acquire new questions is to try answering existing ones. Questions don't just lead to answers, but also to more questions.

拥有许多未解答的问题是一件好事。而这种情况正是富者愈富的典型，因为获取新问题的最佳方式就是尝试回答现有问题。问题不仅会引出答案，还会引出更多问题。

The best questions grow in the answering. You notice a thread protruding from the current paradigm and 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. You can rarely predict that. It's hard enough even to notice the thread, let alone to predict how much will unravel if you pull on it.

最好的问题是在回答中不断成长。你注意到当前范式中露出一根线头，试着拉一下，它就会变得越来越长。所以不要在尝试回答问题前要求它必须明显重大。你很少能预测到这一点。即使注意到线头已经很困难了，更不用说预测拉动它会 unravel 多少。

It's better to be promiscuously curious — to pull 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.

最好保持广泛的好奇心——牵动许多线头，看看会发生什么。大事往往从小处开始。大事的初始版本常常只是实验，或副项目，或演讲，然后逐渐发展成更大的事物。所以开始许多小事物。

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 don't work. You can't have a lot of good ideas without also having a lot of bad ones.[21]

多产往往被低估。尝试的事情越多，发现新事物的机会就越大。但要知道，尝试大量事情也意味着会尝试大量不成功的事情。没有大量坏想法，你不可能有很多好想法。[21]

Though it sounds more responsible to begin by studying everything that's been done before, you'll learn faster and have more fun by trying stuff. And you'll understand previous work better when you do look at it. So err on the side of starting. Which is easier when starting means starting small; those two ideas fit together like two puzzle pieces.

虽然从研究所有先前做过的事情开始听起来更负责任，但尝试各种事情会让你学得更快，也更有趣。当你确实查看这些工作时，你也会更好地理解它们。所以倾向于开始。而开始意味着从小处着手，这两个想法就像拼图的两块一样契合。

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 cleverer and more ambitious than anything you could have planned.

你如何从小处着手做到伟大的事情？通过制作连续版本。伟大的事物几乎总是通过连续版本制作的。你从一个小的开始，并逐步发展它，最终版本比你所能计划的任何东西都要聪明和雄心勃勃。

It's particularly useful to make successive versions when you're making something for people — to get an initial version in front of them quickly, and then evolve it based on their

response.

当你为人们制作某物时，制作连续版本特别有用——快速向他们展示初始版本，然后根据他们的反馈来发展它。

Begin by trying the simplest thing that could possibly work. Surprisingly often, it does. If it doesn't, this will at least get you started.

先尝试最简单可能奏效的方法。出乎意料的是，这种方法往往真的有效。如果无效，至少能让你开始。

Don't try to cram too much new stuff into any one version. There are names for doing this with the first version (taking too long to ship) and the second (the second system effect), but these are both merely instances of a more general principle.

不要试图在任何一个版本中塞入太多新内容。用第一个版本（发布时间过长）和第二个版本（第二系统效应）来做这种事都有专门的名称，但这些都只是更一般原则的特例。

An early version of a new project will sometimes be dismissed as a toy. It's a good sign when people do this. That means it has everything a new idea needs except scale, and that tends to follow.[22]

新项目早期版本有时会被当作玩具来看待。当人们这样评价时，这其实是个好迹象。这意味着它具备新想法所需的一切，除了规模，而规模往往会随之而来。[22]

The alternative to starting with something small and evolving it is to plan in advance what you're going to do. And planning does usually seem the more responsible choice. It sounds more organized to say "we're going to do x and then y and then z" than "we're going to try x and see what happens." And it is more *organized*; it just doesn't work as well.

从小的开始并逐步演进的做法的替代方案是事先规划你要做什么。规划通常看起来是更负责任的选择。说"我们要先做 x，然后 y，再然后 z"听起来比"我们要试试 x，看看会发生什么"更有条理。它确实更有条理；只是效果没那么好。

Planning per se isn't good. It's sometimes necessary, but it's a necessary evil — a response to unforgiving conditions. It's something you have to do because you're working with inflexible media, or because you need to coordinate the efforts of a lot of people. If you keep projects small and use flexible media, you don't have to plan as much, and your designs can evolve instead.

规划本身并不好。它有时是必要的，但是一种必要的恶——对无情条件的应对。你必须这样做，因为你是在与不灵活的媒介合作，或者你需要协调很多人的努力。如果你保持项目规模小，使用灵活的媒介，你就不必做太多规划，你的设计也可以随之演进。

Take as much risk as you can afford. In an efficient market, risk is proportionate to reward, so don't look for certainty, but for a bet with high expected value. If you're not failing occasionally, you're probably being too conservative.

你能承受多大的风险，就冒多大的风险。在一个有效市场中，风险与回报成正比，所以不要寻找确定性，而要寻找期望值高的赌注。如果你偶尔不失败，你可能过于保守了。

Though conservatism is usually associated with the old, it's the young who tend to make this mistake. Inexperience makes them fear risk, but it's when you're young that you can afford

the most.

虽然保守通常与年长者相关联，但年轻人往往犯这个错误。缺乏经验让他们害怕风险，但年轻时你才能承受最多的风险。

Even a project that fails can be valuable. In the process of working on it, you'll have crossed territory few others have seen, and encountered questions few others have asked. And there's probably no better source of questions than the ones you encounter in trying to do something slightly too hard.

即使一个项目失败了，它也可能很有价值。在完成它的过程中，你将跨越很少有人见过的领域，遇到很少有人提出的问题。而且，在尝试做一些稍微太难的事情时遇到的问题，可能没有比这更好的来源了。

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. With effort you can acquire some of the latter when young and keep some of the former when old.

在你拥有青春优势时利用它们，在你拥有年龄优势后利用年龄优势。青春的优势是精力、时间、乐观和自由。年龄的优势是知识、效率、金钱和权力。通过努力，你可以在年轻时获得其中一些，在年老时保留其中一些。

The old also have the advantage of knowing which advantages they have. 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 slightly frivolous ways: 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.

老年人也有优势，那就是他们知道自己有哪些优势。年轻人往往拥有这些优势却意识不到。最大的优势可能就是时间。年轻人根本不知道自己拥有多么宝贵的时间。将时间转化为优势的最佳方式是稍微有些轻浮地使用它：出于好奇心去学习一些你并不需要知道的东西，或者只是因为觉得酷而尝试去建造一些东西，或者变得在某件事上异常擅长。

That "slightly" is an important qualification. 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. The former is at least a bet, and possibly a better one than you think.[23]

这个"稍微"是一个重要的限定。年轻时可以大方地浪费时间，但不要简单地浪费它。在你担心某件事可能是在浪费时间和你确信某件事一定会浪费时间的两者之间，存在着巨大的差异。前者至少是一种赌注，而且可能是比你想象的更好的赌注。[23]

The most subtle advantage of youth, or more precisely of inexperience, is that you're seeing everything with fresh eyes. When your brain embraces an idea for the first time, sometimes the two don't fit together perfectly. Usually the problem is with your brain, but occasionally it's with the idea. A piece of it sticks out awkwardly and jabs you when you think about it. People who are used to the idea have learned to ignore it, but you have the opportunity not to.[24]

青春最微妙的优势，或者说更准确地说是不经世事的优点，就是你用新鲜的眼光看待一切。当

你的大脑第一次接受一个想法时，有时两者并不完全契合。通常问题出在你的大脑上，但偶尔是想法本身。其中一部分显得格格不入，当你思考时就会刺痛你。习惯这个想法的人已经学会了忽略它，但你有机会不这么做。[24]

So when you're learning about something for the first time, pay attention to things that seem wrong or missing. You'll be tempted to ignore them, since there's a 99% chance the problem is with you. And you may have to set aside your misgivings temporarily to keep progressing. But don't forget about them. When you've gotten further into the subject, come back and check if they're still there. If they're still viable in the light of your present knowledge, they probably represent an undiscovered idea.

所以当你第一次学习某事物时，注意那些看起来错误或缺失的地方。你可能会想忽略它们，因为 99%的可能性问题出在你身上。你可能不得不暂时放下疑虑以保持进步。但不要忘记它们。当你对这一主题有更深入的了解后，回来检查它们是否仍然存在。如果它们在你当前的知识背景下仍然成立，它们很可能代表一个尚未被发现的想法。

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.

从经验中获得的最宝贵的知识之一是知道哪些事情不必担心。年轻人知道所有可能重要的事情，但不知道它们的相对重要性。因此，他们平等地担心所有事情，而实际上他们应该更担心少数几件事，几乎不必担心其余的事情。

But what you don't know is only half the problem with inexperience. The other half is what you do know that ain't so. You arrive at adulthood with your head full of nonsense — bad habits you've acquired and false things you've been taught — and you won't be able to do great work till you clear away at least the nonsense in the way of whatever type of work you want to do.

但缺乏经验的问题不仅在于你不知道的事情。另一半问题在于你知道的但并不正确的事情。你带着满头的胡言乱语进入成年——这些胡言乱语包括你养成的不良习惯和你被灌输的错误观念——直到你至少清除掉与你想要从事的工作类型相关的胡言乱语，你才不会做得出色的工作。

Much of the nonsense left in your head is left there by schools. We're so used to schools that we unconsciously treat going to school as identical with learning, but in fact schools have all sorts of strange qualities that warp our ideas about learning and thinking.

你头脑中留下的许多胡言乱语是由学校造成的。我们如此习惯于学校，以至于在不知不觉中将上学等同于学习，但实际上学校具有各种奇怪的特性，这些特性扭曲了我们对学习和思考的理解。

For example, schools induce passivity. Since you were a small child, there was an authority at the front of the class telling all of you what you had to learn and then measuring whether you did. But neither classes nor tests are intrinsic to learning; they're just artifacts of the way schools are usually designed.

例如，学校会培养被动性。从你小时候开始，教室前排就有一个权威人物告诉你必须学习什

么，然后衡量你是否做到了。但无论是课堂还是考试，都不是学习的本质；它们只是学校通常设计方式下的产物。

The sooner you overcome this passivity, the better. If you're still in school, try thinking of your education as your project, and your teachers as working for you rather than vice versa. That may seem a stretch, but it's not merely some weird thought experiment. It's the truth economically, and in the best case it's the truth intellectually as well. The best teachers don't want to be your bosses. They'd prefer it if you pushed ahead, using them as a source of advice, rather than being pulled by them through the material.

你越早克服这种被动性越好。如果你还在上学，试着把你的教育当作你的项目，把你的老师当作为你工作的人，而不是反过来。这听起来可能有些牵强，但它不仅仅是一个奇怪的假设。从经济角度看这是事实，在最好的情况下，它也是智力上的事实。最好的老师不希望成为你的老板。他们更希望你能主动前进，把老师当作建议的来源，而不是被老师带着学习材料。

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 soluble using no more than 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 soluble at all.

学校也给你对工作本质的误导性印象。在学校里，他们会告诉你问题是什么，而且几乎总是可以用你学到的知识来解决。但在现实生活中，你必须自己找出问题是什么，而且你常常不知道这些问题是否能够解决。

But perhaps the worst thing schools do to you is train you to win by hacking the test. You can't do great work by doing that. You can't trick God. So stop looking for that kind of shortcut. The way to beat the system is to focus on problems and solutions that others have overlooked, not to skimp on the work itself.

但也许学校对你做的最糟糕的事是训练你通过作弊考试来获胜。那样做无法成就伟大的工作。你不能愚弄上帝。所以停止寻找那种捷径。战胜规则的方法是关注那些被他人忽视的问题和解决方案，而不是偷工减料。

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.

不要认为你依赖于某个守门人给你一个"重大机遇"。即使这是真的，获得它的最佳方法也是专注于做好工作，而不是追逐有影响力的人。

And don't take rejection by committees to heart. The qualities that impress admissions officers and prize committees are quite different from those required to do great work. The decisions of selection committees are only meaningful to the extent that they're part of a feedback loop, and very few are.

也不要把委员会的拒绝放在心上。让录取官和评奖委员会印象深刻的是那些与成就伟大工作所需品质截然不同的特质。选拔委员会的决定只有在作为反馈循环的一部分时才有意义，而这种情况非常少见。

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 by 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.

一个领域的新手往往会复制现有工作。这本身没什么不好。没有比尝试复制它更好的方法来学习它是如何运作的了。而且复制也不一定会让你的工作失去原创性。原创性是拥有新想法，而不是没有旧想法。

There's a good way to copy and a bad way. If you're going to copy something, do it openly instead of furtively, or worse still, unconsciously. This is what's meant by the famously misattributed phrase "Great artists steal." The really dangerous kind of copying, the kind that gives copying a bad name, is the kind that's done without realizing it, because you're nothing more than a train running on tracks laid down by someone else. But at the other extreme, copying can be a sign of superiority rather than subordination.[25]

有一种好的复制方式，也有一种坏的复制方式。如果你要复制某样东西，应该公开地做，而不是偷偷摸摸地，更糟糕的是，不知不觉地。这就是所谓"伟大的艺术家在窃取"这句著名引语被误传的意思。真正危险的复制，那种给复制带来坏名声的复制，是那种没有意识到自己在做什么的复制，因为你只不过是一列在别人铺设的轨道上运行的火车。但在另一个极端，复制可以是一种优越的标志，而不是从属的标志。[25]

In many fields it's almost inevitable that your early work will be in some sense based on other people's. Projects rarely arise in a vacuum. They're usually a reaction to previous work. When you're first starting out, you don't have any previous work; if you're going to react to something, it has to be someone else's. Once you're established, you can react to your own. But while the former gets called derivative and the latter doesn't, structurally the two cases are more similar than they seem.

在许多领域，你的早期作品在某种程度上基于他人的几乎是不可避免的。项目很少是凭空产生的。它们通常是针对先前工作的反应。当你刚开始时，你没有先前的工作；如果你要做出反应，那必须是别人的。一旦你站稳脚跟，你就可以针对自己的作品做出反应。但前者被称为衍生作品，而后者则不被这样称呼，但从结构上看，这两种情况比它们看起来更为相似。

Oddly enough, the very novelty of the most novel ideas sometimes makes them seem at first to be more derivative than they are. New discoveries often have to be conceived initially as variations of existing things, *even by their discoverers*, because there isn't yet the conceptual vocabulary to express them.

说来也怪，最新颖想法的独特性有时反而让人觉得它们最初更像是模仿之作。新发现往往最初需要被构想为现有事物的变体，即使是由发现者本人提出，因为目前还没有足够的概念词汇来表达它们。

There are definitely some dangers to copying, though. One is that you'll tend to copy old things — things that were in their day at the frontier of knowledge, but no longer are.

模仿确实存在一些风险。一个是你会倾向于模仿旧事物——那些曾经是知识前沿的事物，但现在已不再是了。

And when you do copy something, don't copy every feature of it. Some will make you ridiculous if you do. Don't copy the manner of an eminent 50 year old professor if you're 18, for example, or the idiom of a Renaissance poem hundreds of years later.

当你确实需要模仿时，不要模仿它的所有特征。有些特征模仿了会让你显得很可笑。例如，如果你只有 18 岁，就不要模仿一位 50 岁著名教授的作风，或者模仿几百年后的一首文艺复兴诗歌的文风。

Some of the features of things you admire are flaws they succeeded despite. Indeed, the features that are easiest to imitate are the most likely to be the flaws.
你所欣赏的一些事物特征是它们成功克服的缺陷。事实上，最容易模仿的特征最可能是缺陷。

This is particularly true for behavior. Some talented people are jerks, and this sometimes makes it seem to the inexperienced that being a jerk is part of being talented. It isn't; being talented is merely how they get away with it.
这在行为上尤其如此。有些有才华的人是混蛋，有时这让经验不足的人觉得混蛋是才华的一部分。不是这样的；有才华只是让他们能逃脱。

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.
最强大的复制方式之一是将一个领域的东西复制到另一个领域。历史上充满了这种偶然的发现，因此，通过故意学习其他类型的工作来帮助偶然性可能是值得的。如果你把它们当作隐喻，你可以从相当遥远的领域获取想法。

Negative examples can be as inspiring as positive ones. In fact you can sometimes learn more from things done badly than from things done well; sometimes it only becomes clear what's needed when it's missing.
负面例子可以和正面例子一样鼓舞人心。事实上，有时你可以从做不好的事情上学到更多，而不是从做好的事情上；有时只有当它缺失时，才会变得清晰需要什么。

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.[26]
如果你们领域中的许多最好的人聚集在一个地方，通常去那里待一段时间是个好主意。这将增加你的雄心，而且，通过向你展示这些人也是人，还会增加你的自信心。[26]

If you're earnest you'll probably get a warmer welcome than you might expect. 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's interest in it, and hobbyists always want to talk about their hobbies.
如果你真诚，你可能会得到比预期更热烈的欢迎。大多数非常擅长某事的人都很乐意与任何真正感兴趣的人谈论它。如果他们真的擅长自己的工作，那么他们很可能对它有业余爱好者的兴趣，而业余爱好者总是想谈论他们的爱好。

It may take some effort to find the people who are really good, though. Doing great work has such prestige that in some places, particularly universities, there's a polite fiction that everyone is engaged in it. And that is far from true. People within universities can't say so openly, but the quality of the work being done in different departments varies immensely.

Some departments have people doing great work; others have in the past; others never have.

尽管如此，找到真正擅长的人可能需要一些努力。做伟大的工作具有如此高的声望，以至于在某些地方，特别是在大学里，存在一种礼貌的虚构，即每个人都从事这项工作。但这远非事实。大学内部的人不能公开这么说，但不同系所做的工作质量差异极大。有些系有人们在做伟大的工作；有些系过去有过；有些系从未有过。

Seek out the best colleagues. There are a lot of projects that can't be done alone, and even if you're working on one that can be, it's good to have other people to encourage you and to bounce ideas off.

寻找最好的同事。有很多项目无法单独完成，即使你正在做的项目可以独立完成，有其他人鼓励你并与你交流想法也是好的。

Colleagues don't just affect your work, though; they also affect you. So work with people you want to become like, because you will.

同事不仅影响你的工作，还影响你。所以，与你想成为的那种人一起工作，因为你会成为那样的人。

Quality is more important than quantity in colleagues. It's better to have one or two great ones than a building full of pretty good ones. In fact it's not merely better, but necessary, judging from history: the degree to which great work happens in clusters suggests that one's colleagues often make the difference between doing great work and not.

同事的质量比数量更重要。拥有一两个出色的人，比拥有一栋满是相当不错的人的建筑更好。事实上，从历史来看，这不仅是更好的选择，而且是必要的：伟大工作的集中程度表明，同事往往决定了你是否能做出伟大工作。

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. But it may be possible to give a more concrete answer than that. Here's an attempt: sufficiently good colleagues offer *surprising* insights. They can see and do things that you can't. So if you have a handful of colleagues good enough to keep you on your toes in this sense, you're probably over the threshold.

当你拥有足够好的同事时，你如何知道？根据我的经验，当你拥有时，你就知道。这意味着如果你不确定，你可能没有。但也许可以给出比这更具体的答案。这里是一个尝试：足够好的同事会提供惊人的见解。他们能看到和做到你做不到的事情。所以如果你有一小群足以让你保持警惕的同事，你可能已经跨过了门槛。

Most of us can benefit from collaborating with colleagues, but some projects require people on a larger scale, and starting one of those is not for everyone. If you want to run a project like that, you'll have to become a manager, and managing well takes aptitude and interest like any other kind of work. If you don't have them, there is no middle path: you must either force yourself to learn management as a second language, or avoid such projects.[27]

我们大多数人都能从与同事合作中受益，但有些项目需要更大规模的人员参与，而启动这类项目并非适合所有人。如果你想要负责这样的项目，就必须成为一名管理者，而管理好任何工作

一样需要天赋和兴趣。如果你不具备这些特质，就没有折中的办法：你必须要么强迫自己把管理作为第二语言来学习，要么避免参与这类项目。[27]

Husband your morale. It's the basis of everything when you're working on ambitious projects. You have to nurture and protect it like a living organism.
关照你的士气。在从事雄心勃勃的项目时，这是所有事情的基础。你必须像呵护生物一样去培养和保护它。

Morale starts with your view of life. You're more likely to do great work if you're an optimist, and more likely to if you think of yourself as lucky than if you think of yourself as a victim.
士气始于你对生活的看法。如果你是个乐观主义者，更有可能做出伟大的工作；如果你认为自己是幸运的，而不是认为自己是受害者，也更有可能做出伟大的工作。

Indeed, work can to some extent protect you from your problems. If you choose work that's pure, its very difficulties will serve as a refuge from the difficulties of everyday life. If this is escapism, it's a very productive form of it, and one that has been used by some of the greatest minds in history.
事实上，工作在一定程度上可以保护你免受问题的困扰。如果你选择纯粹的工作，其本身的困难将作为逃避日常生活困难的避难所。如果这是逃避主义，那也是一种非常富有成效的形式，而且历史上一些最伟大的人物都曾使用过它。

Morale compounds via work: high morale helps you do good work, which increases your morale and helps you do even better work. But this cycle also operates in the other direction: if you're not doing good work, that can demoralize you and make it even harder to. 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 to get something done.
工作提升士气：高昂的士气能助你做好工作，而做好工作又会提升你的士气，助你做得更好。但这个循环也会反向运作：如果你做得不好，那会打击你的士气，使情况更加困难。由于这个循环朝正确方向运行至关重要，当你陷入困境时，转而做些更容易的工作是个好主意，这样你就能开始有所进展。

One of the biggest mistakes ambitious people make is to allow setbacks to destroy their morale all at once, like a balloon bursting. You can inoculate yourself against this by explicitly considering setbacks a part of your process. Solving hard problems always involves some backtracking.
雄心勃勃的人常犯的最大错误之一是让挫折瞬间摧毁他们的士气，就像气球爆炸一样。你可以通过明确将挫折视为你过程的一部分来保护自己免受这种影响。解决难题总是涉及一些回溯。

Doing great work is a depth-first search whose root node is the desire to. So "If at first you don't succeed, try, try again" isn't quite right. It should be: If at first you don't succeed, either try again, or backtrack and then try again.
做好工作是深度优先搜索，其根节点是欲望。所以"如果第一次不成功，再试，再试"并不完全正确。应该是：如果第一次不成功，要么再试，要么回溯后再试。

"Never give up" is also not quite right. Obviously there are times when it's the right choice to eject. A more precise version would be: Never let setbacks panic you into backtracking more

than you need to. Corollary: Never abandon the root node.

"永不放弃"也不完全正确。显然，在某些情况下，放弃是正确的选择。更精确的说法是：不要让挫折让你过度退缩。推论：不要放弃根本节点。

It's 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. It depends how fast you're running. So learn to distinguish good pain from bad. Good pain is a sign of effort; bad pain is a sign of damage.

如果工作很艰难，这未必是坏事，就像跑步时喘不过气也不是坏事一样。这取决于你跑得多快。所以要学会区分好痛和坏痛。好痛是努力的标志；坏痛是损伤的标志。

An audience is a critical component of morale. If you're a scholar, your audience may be your peers; in the arts, it may be an audience in the traditional sense. Either way it doesn't need to be big. The value of an audience doesn't grow anything like linearly with its size. Which is bad news if you're famous, but good news if you're just starting out, because it means a small but dedicated audience can be enough to sustain you. If a handful of people genuinely love what you're doing, that's enough.

观众是士气的重要组成部分。如果你是学者，你的观众可能是你的同行；在艺术领域，可能是传统意义上的观众。无论哪种情况，都不需要很大。观众的价值与其规模并不完全成线性增长。这对名人来说可能是坏消息，但对刚起步的人来说却是好消息，因为这意味着一个虽小但忠实的观众就足够支撑你。如果少数人真心喜爱你所做的事情，那就足够了。

To the extent you can, avoid letting intermediaries come between you and your audience. In some types of work this is inevitable, but it's so liberating to escape it that you might be better off switching to an adjacent type if that will let you go direct.[28]

在你能做到的范围内，尽量避免让中介人出现在你和受众之间。在某些类型的工作中，这是不可避免的，但摆脱这种束缚会让你感到无比轻松，以至于如果你能直接接触受众，或许换一种相邻类型的工作会更好。[28]

The people you spend time with will also have a big effect on your morale. You'll find there are some who increase your energy and others who decrease it, and the effect someone has is not always what you'd expect. Seek out the people who increase your energy and avoid those who decrease it. Though of course if there's someone you need to take care of, that takes precedence.

你花时间与之相处的人也会对你的士气产生重大影响。你会发现有些人能让你精力充沛，而有些人则会消耗你的精力，而且一个人对你的影响并不总是你所期望的那样。寻找那些能让你精力充沛的人，避开那些消耗你精力的人。当然，如果你需要照顾某人，那另当别论。

Don't marry someone who doesn't understand that you need to work, or sees 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.

不要与那些不理解你需要工作，或视你的工作为争夺你注意力的竞争者结婚。如果你有雄心壮志，你就需要工作；这几乎就像一种医学状况；所以，那些不让你工作的人要么是不理解你，要么是理解了但不在乎。

Ultimately morale is physical. You think with your body, so it's important to take care of it. That means exercising regularly, eating and sleeping well, and avoiding the more dangerous kinds of drugs. Running and walking are particularly good forms of exercise because they're good for thinking.[29]

最终，士气是身体的。你用身体思考，所以照顾好它是很重要的。这意味着要定期锻炼，吃得好睡得好，并避免更危险的药物。跑步和走路是特别好的锻炼形式，因为它们对思考有益。[29]

People who do great work are not necessarily happier than everyone else, but they're happier than they'd be if they didn't. 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.

做伟大工作的人并不一定比其他人更快乐，但他们比不这样做的人更快乐。事实上，如果你聪明且有雄心，不保持生产力是危险的。那些聪明且有雄心但成就不大的人往往会变得痛苦。

It's ok 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.

想给别人留下印象是可以的，但要选择正确的人。你尊敬的人的看法是信号。名声，即你可能尊重也可能不尊重的更大群体的看法，只会增加噪音。

The prestige of a type of work is at best a trailing indicator and sometimes completely mistaken. If you do anything well enough, you'll make it prestigious. So the question to ask about a type of work is not how much prestige it has, but how well it could be done.

某种工作的声望充其量只是一个滞后指标，有时完全错误。如果你把任何事做得足够好，你就会让它变得有声望。所以关于某种工作的提问不是它有多少声望，而是它有多好。

Competition can be an effective motivator, but don't let it choose the problem for you; don't let yourself get drawn into chasing something just because others are. In fact, don't let competitors make you do anything much more specific than work harder.

竞争可以是一种有效的激励，但不要让它为你选择问题；不要因为别人在追就让自己陷入追逐。事实上，不要让竞争对手让你做比努力更具体的事情。

Curiosity is the best guide. Your curiosity never lies, and it knows more than you do about what's worth paying attention to.

好奇心是最好的向导。你的好奇心永远不会撒谎，而且它比你知道得更多，关于什么值得关注。

Notice how often that word has come up. 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."

注意这个词经常出现。如果你问一个神谕关于如何做伟大工作的秘诀，而神谕用一个词回答，我赌的是"好奇心"。

That doesn't translate directly to advice. It's not enough just to be curious, and you can't command curiosity anyway. But you can nurture it and let it drive you.

这不能直接翻译成建议。光有好奇心是不够的，而且你也不能命令好奇心。但你可以培养它，并让它驱动你。

Curiosity is the key to all four steps in doing great work: it will choose the field for you, get you to the frontier, cause you to notice the gaps in it, and drive you to explore them. The whole process is a kind of dance with curiosity.

好奇心是做好伟大工作的关键，它将为你选择领域，带你到达前沿，让你发现其中的差距，并驱使你探索这些差距。整个过程就像与好奇心共舞。

Believe it or not, I tried 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.

信不信由你，我尽量让这篇文章尽可能简短。但它的长度至少意味着它起到了筛选作用。如果你能读到这一步，你一定对做好伟大工作感兴趣。而且如果你感兴趣，你已经比你意识到的更进一步了，因为愿意这样想的人并不多。

The factors in doing great work are factors in the literal, mathematical sense, and they are: ability, interest, effort, and luck. Luck by definition you can't do anything about, so we can ignore that. And we can assume effort, if you do in fact want to do great work. So the problem boils down to ability and interest. Can you find a kind of work where your ability and interest will combine to yield an explosion of new ideas?

做好伟大工作的因素在字面和数学意义上都是因素，它们是：能力、兴趣、努力和运气。运气在定义上是你无法改变的，所以我们可以忽略它。而且我们可以假设努力，如果你确实想做好伟大工作。所以问题归结为能力和兴趣。你能找到一种工作，让你的能力和兴趣结合起来产生新的想法的爆发吗？

Here there are grounds for optimism. There are so many different ways to do great work, and even more that are still undiscovered. Out of all those different types of work, the one you're most suited for is probably a pretty close match. Probably a comically close match. It's just a question of finding it, and how far into it your ability and interest can take you. And you can only answer that by trying.

这里存在乐观的理由。有如此多的不同方式可以做出杰出成就，而且还有更多尚未被发现的方式。在所有这些不同类型的工作中，最适合你的那一种很可能与你非常匹配。可能是一种滑稽的匹配。问题仅仅在于找到它，以及你的能力和兴趣能将其带向多深的程度。而你只能通过尝试来回答这个问题。

Many more people could try to do great work than do. What holds them back is a combination of modesty and fear. It seems presumptuous to try to be Newton or Shakespeare. It also seems hard; surely if you tried something like that, you'd fail. Presumably the calculation is rarely explicit. Few people consciously decide not to try to do great work. But that's what's going on subconsciously; they shy away from the question.

比实际尝试做出杰出成就的人要多得多的人可以尝试。阻碍他们的因素是谦逊和恐惧的结合。尝试成为牛顿或莎士比亚似乎很自负。这也似乎很难；如果你尝试做类似的事情，你肯定会失

败。这种计算很少是明确的。很少有人有意识地决定不去尝试做出杰出成就。但这是在潜意识中发生的；他们回避这个问题。

So I'm going to pull a sneaky trick on you. Do you want to do great work, or not? Now you have to decide consciously. Sorry about that. I wouldn't have done it to a general audience. But we already know you're interested.
所以我要对你耍个小聪明。你想做出杰出成就吗？还是不想？现在你必须有意识地做出决定。抱歉。我不会对普通观众这样做。但我们已经知道你对此感兴趣。

Don't worry about being presumptuous. You don't have to tell anyone. And if it's too hard and you fail, so what? Lots of people have worse problems than that. In fact you'll be lucky if it's the worst problem you have.
别担心显得自大。你不必告诉任何人。就算太难了，你失败了又怎样？很多人有比这更糟糕的问题。事实上，如果你遇到的最坏问题是这个，那你真是幸运。

Yes, you'll have to work hard. But again, lots of people have to work hard. And if you're working on something you find very interesting, which you necessarily will if you're on the right path, the work will probably feel less burdensome than a lot of your peers'.
是的，你不得不努力工作。但同样，很多人都要努力工作。而且，如果你在做一件你发现非常有意思的事情——而如果你走对了路，这必然会发生——这项工作可能会比许多同龄人的工作感觉不那么沉重。

The discoveries are out there, waiting to be made. Why not by you?
那些发现就在那里，等待着被发现。为什么不由你来发现呢？

Notes 笔记

[1] I don't think you could give a precise definition of what counts as great work. 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. It's a matter of degree, and often hard to judge at the time anyway. So I'd rather people focused on developing their interests rather than worrying about whether they're important or not. Just try to do something amazing, and leave it to future generations to say if you succeeded.
[1]我不认为你能给出一个精确的定义来界定什么算作伟大的工作。做伟大的工作意味着把某件重要的事情做得如此出色，以至于你拓展了人们对可能性的认知。但重要性没有门槛。这是一个程度问题，而且无论如何，当时也很难判断。所以我认为人们应该专注于培养自己的兴趣，而不是担心它是否重要。只要尽力做一些令人惊叹的事情，让后代去评判你是否成功。

[2] A lot of standup comedy is based on noticing anomalies in everyday life. "Did you ever notice...?" New ideas come from doing this about nontrivial things. Which may help explain why people's reaction to a new idea is often the first half of laughing: Ha!
[2] 很多脱口秀喜剧是基于在日常中发现异常。"你有没有注意到...?" 新想法来自于对非琐碎事物进行这种观察。这可能有助于解释为什么人们对新想法的反应往往是笑的前半部分：哈!

[3] That second qualifier is critical. If you're excited about something most authorities discount, but you can't give a more precise explanation than "they don't get it," then you're

starting to drift into the territory of cranks.

[3] 第二个限定词非常关键。如果你对某件事感到兴奋，但大多数权威人士都否定，而你无法给出比"他们不懂"更精确的解释，那么你开始滑向偏执狂的领域。

[4] Finding something to work on is not simply a matter of finding a match between the current version of you and a list of known problems. You'll often have to coevolve with the problem. That's why it can sometimes be so hard to figure out what to work on. The search space is huge. It's the cartesian product of all possible types of work, both known and yet to be discovered, and all possible future versions of you.

[4] 找到可以着手的事情，并不仅仅是将当前版本的自己与已知问题列表进行匹配的问题。你通常必须与问题共同进化。这就是为什么有时很难弄清楚要做什么的原因。搜索空间非常巨大。它是所有可能类型的工作（包括已知的和尚未发现的）以及所有可能的未来版本的自己的笛卡尔积。

There's no way you could search this whole space, so you have to rely on heuristics to generate promising paths through it and hope the best matches will be clustered. Which they will not always be; different types of work have been collected together as much by accidents of history as by the intrinsic similarities between them.

没有办法搜索整个空间，所以你必须依赖启发式方法来生成有希望的路径，并希望最佳匹配会聚集在一起。但这并不总是会发生；不同类型的工作之所以被放在一起，既有历史偶然性的原因，也有它们之间内在相似性的原因。

[5] There are many reasons curious people are more likely to do great work, but one of the more subtle is that, by casting a wide net, they're more likely to find the right thing to work on in the first place.

[5] 好奇心强的人更有可能做出伟大的工作，原因有很多，但其中一种更微妙的是，通过广泛撒网，他们更有可能在开始时就找到正确的工作方向。

[6] It can also be dangerous to make things for an audience you feel is less sophisticated than you, if that causes you to talk down to them. You can make a lot of money doing that, if you do it in a sufficiently cynical way, but it's not the route to great work. Not that anyone using this m.o. would care.

[6] 对那些你认为不如你成熟的目标受众制作东西也可能很危险，如果这导致你对他们居高临下。如果你以足够愤世嫉俗的方式这样做，你可以赚很多钱，但这并不是通往伟大工作的道路。尽管使用这种策略的人可能并不在乎。

[7] This idea I learned from Hardy's *A Mathematician's Apology*, which I recommend to anyone ambitious to do great work, in any field.

[7] 我从哈代的《一个数学家的辩护》中了解到这个想法，我推荐任何有志于在任何领域做出伟大工作的人阅读这本书。

[8] Just as we overestimate what we can do in a day and underestimate what we can do over several years, we overestimate the damage done by procrastinating for a day and underestimate the damage done by procrastinating for several years.

[8] 就像我们高估自己一天能做什么而低估几年能做什么一样，我们高估了拖延一天造成的损害，而低估了拖延几年造成的损害。

[9] You can't usually get paid for doing exactly what you want, especially early on. There are two options: get paid for doing work close to what you want and hope to push it closer, or get paid for doing something else entirely and do your own projects on the side. Both can work, but both have drawbacks: in the first approach your work is compromised by default, and in the second you have to fight to get time to do it.

[9] 通常情况下，你无法做自己真正想做的事情来获得报酬，尤其是在早期。有两种选择：做与你想要接近的工作并获得报酬，希望将其推向更近的目标；或者做完全不同的工作，并在业余时间做自己的项目。两种方法都可以行得通，但都有缺点：在第一种方法中，你的工作从一开始就打了折扣；在第二种方法中，你必须争分夺秒地挤出时间来做它。

[10] If you set your life up right, it will deliver the focus-relax cycle automatically. The perfect setup is an office you work in and that you walk to and from.

[10] 如果你把生活安排得当，它就会自动提供专注-放松的循环。完美的设置是你工作的地方，也是你步行去和回来的地方。

[11] There may be some very unworldly people who do great work without consciously trying to. If you want to expand this rule to cover that case, it becomes: Don't try to be anything except the best.

[11] 可能有一些非常超凡脱俗的人，他们在没有刻意尝试的情况下也能做出伟大的工作。如果你想把这个规则扩展到这种情况，它就变成了：不要试图成为除最好之外的其他任何事物。

[12] This gets more complicated in work like acting, where the goal is to adopt a fake persona. But even here it's possible to be affected. Perhaps the rule in such fields should be to avoid *unintentional* affectation.

[12] 在表演这类工作中，目标是要扮演一个虚假的角色，这种情况会更复杂。但即便如此，也可能受到影响。或许在这些领域，规则应该是避免无意的做作。

[13] It's safe to have beliefs that you treat as unquestionable if and only if they're also unfalsifiable. For example, it's safe to have the principle that everyone should be treated equally under the law, because a sentence with a "should" in it isn't really a statement about the world and is therefore hard to disprove. And if there's no evidence that could disprove one of your principles, there can't be any facts you'd need to ignore in order to preserve it.

[13] 只有当你持有的信念是不可置疑的，并且它们也是无法被证伪的，你持有这些信念才是安全的。例如，持有"法律面前人人平等"的原则是安全的，因为包含"应该"的句子并不是关于世界的真正陈述，因此很难被证伪。如果没有任何证据能够证伪你的某个原则，那么就没有任何事实是你需要忽略以维护它的。

[14] Affectation is easier to cure than intellectual dishonesty. Affectation is often a shortcoming of the young that burns off in time, while intellectual dishonesty is more of a character flaw.

[14] 做作比智力不诚更容易纠正。做作往往是年轻人的缺点，随着时间的推移会消失，而智力不诚则更像是一种性格缺陷。

[15] Obviously you don't have to be working at the exact moment you have the idea, but you'll probably have been working fairly recently.

[15] 显然，你不需要在产生想法的那一刻就正在工作，但你很可能在不久前刚刚工作过。

[16] Some say psychoactive drugs have a similar effect. I'm skeptical, but also almost totally ignorant of their effects.
[16] 有人说致幻药物有类似的效果。我对此表示怀疑，但也几乎完全不了解它们的效果。

[17] For example you might give the nth most important topic $(m-1)/m^n$ of your attention, for some m > 1. You couldn't allocate your attention so precisely, of course, but this at least gives an idea of a reasonable distribution.
[17] 例如，你可以将第 n 个最重要主题（m-1）/m^n 的注意力分配给你，其中 m 大于 1。当然，你不可能如此精确地分配注意力，但这至少能给你一个合理的分配概念。

[18] The principles defining a religion have to be mistaken. Otherwise anyone might adopt them, and there would be nothing to distinguish the adherents of the religion from everyone else.
[18] 定义宗教的原则必须是错误的。否则，任何人都可以采用它们，而且宗教信徒与其他人之间将没有任何区别。

[19] It might be a good exercise to try writing down a list of questions you wondered about in your youth. You might find you're now in a position to do something about some of them.
[19] 尝试写下你年轻时曾疑惑过的问题清单可能是个不错的练习。你可能会发现，现在你有机会解决其中的一些问题。

[20] The connection between originality and uncertainty causes a strange phenomenon: because the conventional-minded are more certain than the independent-minded, this tends to give them the upper hand in disputes, even though they're generally stupider.
[20] 创新与不确定性之间的联系引发了一种奇怪的现象：由于传统思维者比独立思维者更确定，这往往让他们在争论中占据上风，尽管他们通常更愚蠢。

> The best lack all conviction, while the worst
> 最好的缺乏信念，而最差的
> Are full of passionate intensity.
> 充满热情的强度。

[21] Derived from Linus Pauling's "If you want to have good ideas, you must have many ideas."
[21] 源自林纳斯·保罗的"如果你想要好点子，就必须有很多点子。"

[22] Attacking a project as a "toy" is similar to attacking a statement as "inappropriate." It means that no more substantial criticism can be made to stick.
[22] 将项目称为"玩具"类似于将陈述称为"不恰当"。这意味着无法再提出更实质性的批评。

[23] One way to tell whether you're wasting time is to ask if you're producing or consuming. Writing computer games is less likely to be a waste of time than playing them, and playing games where you create something is less likely to be a waste of time than playing games where you don't.
[23] 判断你是否在浪费时间的一个方法是询问你是否在产出或消费。写电脑游戏比玩电脑游戏不太可能浪费时间，而玩能创造东西的游戏比玩不能创造东西的游戏不太可能浪费时间。

[24] Another related advantage is that if you haven't said anything publicly yet, you won't be biased toward evidence that supports your earlier conclusions. With sufficient integrity you could achieve eternal youth in this respect, but few manage to. For most people, having previously published opinions has an effect similar to ideology, just in quantity 1.

[24] 另一个相关优势是，如果你还没有公开说过任何话，你就不会偏向支持你早期结论的证据。只要有足够的正直，你可以在这一点上实现永葆青春，但很少有人能做到。对大多数人来说，之前发表过的观点会产生类似意识形态的影响，只是数量为 1。

[25] In the early 1630s Daniel Mytens made a painting of Henrietta Maria handing a laurel wreath to Charles I. Van Dyck then painted his own version to show how much better he was.

[25] 在 1630 年代初，丹尼尔·米滕斯画了一幅亨丽埃塔·玛丽亚将桂冠交给查理一世的画作。然后凡·戴克画了自己的版本，以展示自己有多么优秀。

[26] I'm being deliberately vague about what a place is. As of this writing, being in the same physical place has advantages that are hard to duplicate, but that could change.

[26] 我在故意含糊其辞地谈论一个地方是什么。截至本文写作时，身处同一物理地点具有难以复制的优势，但这可能会改变。

[27] This is false when the work the other people have to do is very constrained, as with SETI@home or Bitcoin. It may be possible to expand the area in which it's false by defining similarly restricted protocols with more freedom of action in the nodes.

[27] 当其他人必须做的工作非常受限时，比如 SETI@home 或比特币，这是错误的。通过定义具有更多节点行动自由的类似受限协议，可能会扩大这种错误不成立的范围。

[28] Corollary: Building something that enables people to go around intermediaries and engage directly with their audience is probably a good idea.

[28] 推论：创建一个让人们能够绕过中介，直接与受众互动的东西，可能是个好主意。

[29] It may be helpful always to walk or run the same route, because that frees attention for thinking. It feels that way to me, and there is some historical evidence for it.

[29] 总是走或跑相同的路线可能有助于集中注意力思考。我这样感觉，而且有一些历史证据支持这一点。